Reviewer #1:

The paper “Sensitivity of land-atmosphere coupling strength to perturbations of early-morning temperature and moisture profiles in the European summer” explores the uncertainty of classifying land-atmosphere interactions using the CTP-HI framework by systematically perturbing the temperature and moisture profiles from a regional climate model and then analyzing how the coupling classification changes. The paper is well written and has a logical organization that makes it easy to follow. The experiment design is interesting and the results provide new insights into the coupling between the land and the atmosphere for this particular model. Based on this, the paper is well suited for Earth System Dynamics and merits publication. Despite these positive aspects, the paper is not particularly clear in defining the larger research question and discussing the results in a way that is consistent with the work being done. Based on this, three suggestions for improvement are given below.

First, we would like to thank the reviewer for the time he invested in reviewing our manuscript, and for providing helpful and constructive comments. We hope we were able to address the issues raised appropriately in our responses, which are provided below each comment.

First, it is difficult to know how much to trust the results of this paper since the analysis is based on a regional climate model that may have its own set of biases that will skew the results from the coupling classification. As applied here, the CTP-HI classification is fixed and therefore, a model with a consistent bias in the atmospheric profiles will give skewed results. This climatological inconsistency in the CTP-HI framework for some data sets was shown in Ferguson and Wood (2011) and was the reason for developing a data set specific method of classifying the CTP-HI space (Roundy et al. 2013). One possible way of addressing this limitation is to compare the surface temperature and humidity from the model to observations. This would at least provide a means of assessing where the model is biased and may provide insights into the results such as why are there very few dry soil advantage days in the model (Figures 6 and 7). Regardless of what is done to address this, there needs to be a clearer discussion that the results in this paper are model specific and may or may not represent the real world.

Response: We agree with the reviewer that every climate model has its own set of biases and that there is potential for influencing the results. However, the goal of this work is to assess the extent at which temperature and moisture changes in the atmosphere might influence the land-atmosphere coupling regimes, and thus assess how the coupling signal changes rather that the classification of the coupling regimes themselves. The goal is not a verification of the coupling classification from the model.

Furthermore, while a comparison of the surface temperature field from the model with observations would of course be possible, such a comparison is challenging for surface humidity due to the lack of spatially comprehensive observations. The best option to evaluate the moisture fields is a comparison with reanalysis data such as the bias-corrected ERA5 reanalysis dataset (C3S, 2020).

To assess potential inconsistencies, we compared the temporal distributions of temperature and moisture from the model with reanalysis. As the frequency of occurrence of the coupling classes is rather linked to the temporal distribution of the temperature and moisture fields than to biases in the means, comparing the distributions is expected to be well suited to assess climatological inconsistencies. For this purpose, we applied two statistical measures on a cell-wise basis: a Z-statistic and the PDF skill score by Perkins et al. (2007). Both measures showed good agreement between the distributions of the model and the reanalysis data for both variables. The value of the Z-statistic remained below 2 for both variables throughout the entire domain, which means that the differences are statistically not significant. The PDF skill score has a value larger than 0.8 over most of the continent. Strongest discrepancies were found in the Mediterranean region, which is expected to be predominantly in atmospheric control.

We appreciate your insightful comment and agree that climatological inconsistency among datasets is a potential limitation which requires further space for discussion in the paper. This is why we will add a paragraph on this in the discussion section. However, as the focus of our study is to analyze changes in the coupling signal due to changes in moisture and temperature and not to verify the coupling signal in the model, we would not add an extra section with comparisons against reanalysis in the manuscript.

C3S: Near surface meteorological variables from 1979 to 2018 derived from bias-corrected reanalysis, https://doi.org/10.24381/CDS.20D54E34, 2020.

Perkins, S. E., Pitman, A. J., Holbrook, N. J., and McAneney, J.: Evaluation of the AR4 Climate Models' Simulated Daily Maximum Temperature, Minimum Temperature, and Precipitation over Australia Using Probability Density Functions, 20, 4356–4376, https://doi.org/10.1175/JCLI4253.1, 2007.

Second, on first reading the title and abstract, I thought this was more of a modeling study where the model was perturbed and then run like the original Findell paper. However, this work does not actually do any new model runs, nor does it actually look at coupled model processes within the model and could just as easily be applied to a reanalysis data set which would have the added benefit of having assimilated observations. This does not dimension the results but begs the question as to why a regional model run is used in the analysis as opposed to reanalysis? Why not do both and compare them?

Response: Thank you for your comment. The main reason for using the regional climate model run was to maintain consistency with the investigations of Jach et al. (2020) in which additional model simulations with modified land cover were analyzed and which is referred to throughout. This was meant to provide a comprehensive picture on the coupling strength and factors at the land surface and in the atmosphere which potentially influence the long-term coupling signal. Further, we intend to apply this methodology to model runs of future periods for which no reanalysis data exist.

Since we wanted to focus primarily on the changes in the coupling signal due to modifications in temperature and moisture in this work, we are convinced that the results are meaningful also without the benefit of assimilated observations as given by reanalysis data. Nevertheless, we agree that a comparison of the model results with results from reanalysis data would be interesting for estimating uncertainty coming from the climatological inconsistencies between datasets as you raised in your first comment. We think this is an interesting option for future analysis which we will mention in the conclusion, but it is beyond the scope of this paper.

At a minimum, revising the title and abstract so that it better reflects the work done would be beneficial. In my opinion, this work is interesting because it is answering the question of what happens to the coupling if there is a change in temperature or moisture? It would be great to see the title and abstract reflect this.

Response: We understand your point and agree with you. We will revise the title and the abstract so that the work done is better reflected in there.

Third, on a whole the results are fairly predictable in that if you change the temperature and humidity profiles then you will change the calculated CTP-HI, which will then change coupling classification for that particular day. This means that areas that will be most affected will be those that lie on the boundaries between the strict classification thresholds. So what is really being analyzed in this work is what regions are most often on the boarder of the classification regimes and what kind of perturbations will bump them into the other regime. This is not to say that work is not meaningful, but I think it would greatly improve the paper by discussing this simple idea extensively in the introduction to help better setup the results.

Response: It is indeed true that the perturbations are meant to test whether, where and under which conditions they modify -or as you say bump- the coupling classification into another class. This not apparent from the regime classification of the model output only, and thus needs to be characterized and quantified based on the model output and its perturbations. Assuming that the classification is accurate enough, the coupling is vulnerable to changes in temperature and moisture in a region in which the classification is regularly bumped into another class. This is because the atmospheric preconditioning is at the thresholds between the different classes and a bump implies that the likelihood for a certain response in the atmosphere changes from one to another. We will add a paragraph in the introduction and broached it of in the discussion.

In addition, below are several minor suggestions for improving the paper. Lines 74-75: The CTP-HI framework has been applied using satellite data and has given reasonable results (Roundy and Santanello 2017).

Response: Thank you for pointing out this study to us. We apologize that we omitted it and will revise the corresponding paragraphs.

Line 123: Consider revising to “but may limit the investigation of pre-conditioning”

Response: We will adopt this suggestion.

Lines 310 and 332: There are a couple instances of using the word chapter in the paper. For this kind of paper, “section” would be better.

Response: Thank you for mentioning that. We will change all occurrences of “chapter” to “section”.

Line 352: The figure caption needs more detail here. Is this the average for the entire domain or just part of it?

Response: To achieve the factors, we averaged over the entire domain. We will amend the caption and give it more details, as well as clarify it in the text.

Line 386: Precipitation is not really validated in this work. This may be true if one assumes that the Findell et al. framework holds for the model used in this study, but no analysis is given to show this. It is probably best to avoid making the jump to precipitation and just stick with the classification.

Response: We agree with your comment and also didn’t mean to imply that precipitation or the outcome in form of a traceable coupling event was validated. We will revise the paragraph.

References

Ferguson, C. R., and E. F. Wood, 2011: Observed Land-Atmosphere Coupling from Satellite Remote Sensing and Reanalysis. J. Hydrometeorol., 12, 1221–1254, https://doi.org/10.1175/2011jhm1380.1.

Roundy, J. K., and J. A. Santanello, 2017: Utility of Satellite Remote Sensing for Land-Atmosphere Coupling and Drought Metrics. J. Hydrometeorol., 18, 863–877,

https://doi.org/10.1175/JHM-D-16-0171.1.
Roundy, J.K., C. R. Ferguson, and E. F. Wood, 2013: Temporal Variability of Land–Atmosphere Coupling and Its Implications for Drought over the Southeast United States. J. Hydrometeorol., 14, 622–635, https://doi.org/10.1175/JHM-D-12-090.1.