



EGUsphere, referee comment RC2
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Comment on egusphere-2022-623

Anonymous Referee #2

Referee comment on "Opening Pandora's box: reducing global circulation model uncertainty in Australian simulations of the carbon cycle" by Lina Teckentrup et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-623-RC2>, 2022

Review of egusphere-2022-623

Opening Pandora's box: How to constrain regional projections of the carbon cycle

By Teckentrup et al.

In this study, the authors analyze the impact of varying meteorological forcing obtained from the historical CMIP6 GCMs / ESMs on the historical carbon cycle. More specifically, they assess the impact of the selection of the simulated meteorological forcing on the response of the Australian carbon cycle using different strategies, e.g. bias correction, random-forest approach, ensemble averaging methods, as well as one dynamic global vegetation model, LPJ-GUESS. The authors compare the different methods and report their effect on carbon cycle simulation of LPJ-GUESS in space and time.

The analyzes presented by Teckentrup et al. are very interesting, comprehensive and useful in understanding the impact of different meteorological forcing on the carbon cycle. In some places, the manuscript seems a bit overloaded, making it somewhat more difficult to grasp the full scope of the analyses. Overall, the manuscript is well written. I have a few general comments and a short list of specific comments. Thus, I recommend minor revisions before publication.

General comments:

- I like how the title reads and the scope of the study, but I find it a bit misleading. As far as I can judge, you are not looking into the projection of the carbon cycle, right? Projection, by definition, means simulating a potential future evolution of the system (e.g. boundary conditions are scenario-driven). Your analysis is based on historical simulations, where we have access to the boundary conditions, e.g. greenhouse gases, volcanic/anthropogenic aerosol loading, etc. So, I would use the word "regional simulations". Then I do not really see how you constrain the regional carbon cycle uncertainty. You show that one land model simulates different CC response dependent on different meteorological inputs. In your previous paper, you showed the uncertainty that is related to the variety of land models. So, I would say that you comprehensively demonstrate the entire uncertainty in simulating the carbon cycle related to the choice of models and choice of forcing, but I don't see really how you would go about in constraining this uncertainty. The proposed bias correction methods etc. do not really contribute to reduce the uncertainty, since, if we now ran all TRENDY models with your reanalysis-corrected / or "ensemble average weighting" meteorological forcing, we would end up with a similar uncertainty in the CC response. Bottomline is, maybe you should focus more on the "full uncertainty" aspect in communicating your analysis, than the "constraining" aspect.
- Overall, I am very surprised that the effect of CO₂ on plants, e.g. on water-use efficiency, or the direct stimulation of carbon assimilation, is not being discussed nor mentioned here at all. These effects are vital in simulating the carbon cycle under rising CO₂. Were these effects accounted for in the LPJ-GUESS setup? I think so, since almost all runs show an increase in C_{total}, even those which received a decrease in precipitation and increase in temperature as forcing. How would Australian ecosystems accumulate more carbon under these circumstances? To estimate the impact of meteorological forcing, the CO₂ effects might not be essential, but still, these effects need to be addressed and communicated.
- I am hesitant to suggest more analysis, since this manuscript already contains a lot of analysis and is a bit over-loaded. So, it is difficult to grasp the entire scope of the manuscript. Are that many supplementary figures needed? I would suggest to assess whether one could reduce some parts in the manuscript, so that it becomes better accessible to the reader and the key messages come across.
- But I have to suggest at least one additional analysis point: You only use one realization (r1i1p1f1) of each model. To really get an idea of how the specific GCM compares to reanalysis and other GCMs, one should analyze as many realizations as possible. I would even suggest to get meteorological forcing from grand / large ensembles and one can identify real biases in the model. One realization is not representative for the model, except when some data-assimilation / nudging is conducted (e.g. as in reanalysis). I know it would be too much work for this study, but one should think about it.

Specific Comments:

L18: What does "and above" mean here? and above global scale?

LL85-89: Rather long sentence containing many aspects - can you split it up in at least two separate sentences?

LL92-94: I don't understand the logic of this sentence. TRENDY models use the identical meteorological forcing and show a large difference in the response of the carbon cycle to the forcing. So, this calls for reducing uncertainty in the land-surface model predictions, rather than the meteorological forcing, no?

LL97-98: Can you provide more detail on what first generation and second generation DGVMs refer to?

L103: What simulation? Please be more specific. It is probably the "historical" simulation, but there are others, like esmHist, where the carbon cycle is fully coupled, etc.

L104: What about the information about atmospheric humidity, i.e. VPD?

LL106-7: Can you really do that? Shouldn't you recycle all the inputs consistently then? You can have strong precipitation with simultaneous high shortwave radiation - what does LPJ-GUESS make out of these physically implausible inputs?

LL108-109: This means you are doing some heavy down-scaling the input variables to a quite high resolution in comparison to the native resolution of the GCMs. Maybe better to remap to a common 1x1 degree grid, no? Or maybe it'd be better to use downscaled CMIP6 output, e.g. <https://eartharxiv.org/repository/view/2646/>

L125: I think, that is not true. ERA5 is in 0.5x0.5 grid and there is a derivative that is at 0.25x0.25, but 0.05 seems extremely high resolution for reanalysis.

Figure 1: I think it would benefit the understanding of Figure 1, if you provided a slightly more elaborate figure caption. At least, you could specify the acronyms used in the figure, so the figure is readable without searching in the text for the acronym definitions.

L140: Can you provide more information on this estimator?

Table 2: The definition of the the summation notation would need more information to be mathematically correct, but I guess it is understandable as it is.
https://en.wikipedia.org/wiki/Root-mean-square_deviation

L143: Well, these models historically evolved and they share code and concepts. It's hard

to define which models are independent. Also, the models that are used to create the reanalysis e.g. IFS for ERA5 share code with CMIP6 models.

L147-148: Also, models that are highly dependent might not "correlate more" on monthly time-scale as the atmosphere is chaotic and highly dependent on the initial state etc.; I would assume that correlation of the spatial pattern in the climatological mean would provide more information. So, I think similar spatial bias matching would give you an idea whether models are similar or not, but maybe you do that, I did not fully understand.

L166: "Let us define" ?

LL170-173: Does this part connect to any paragraph?

L180: If you used temperature in Kelvin scale (so no negative values), one could only use this function for scaling consistently for all variables, no?

L191: "Let us denote" ?

L205: Not sure how this fits in the structure of the paragraph.

LL231-232: Then I really wonder why some representation of atmospheric humidity is not an input to LPJ-GUESS.

LL276-277: Can you explain why you include non-physical parameters such as longitude and latitude in the random-forest approach. Especially for a regional study, I would advise against this practice.

Figure 2: b,d,f are the same - but I saw the uploaded corrected figure.

LL305-onwards: Would it make sense to compare carbon fluxes from the actual CMIP6 models to get an estimate for carbon cycle uncertainty? Not all models (e.g. MPI-ESM1-2-HR), but most have some representation of the carbon cycle and the carbon fluxes? I also understand if you only wanted to focus on the effect of the selection of the meteorological forcing.

Figure 3: "PPT" is a rarely seen abbreviation for precipitation, better pr?

LL446-447: In the context of Australia, I would assume one can also add "improved prediction of fire risk", as fire depends largely on the fuel load thus vegetation / carbon cycle.

LL589-590: Counter-argument: One should not only rely on using one DGVM for studies on ecosystem/carbon cycle impact. Maybe you can make the point, that we should use multiple DGVMs and multiple GCMs forcings.