Biogeosciences Discuss., referee comment RC2
https://doi.org/10.5194/bg-2022-92-RC2, 2022

Comment on bg-2022-92

Joe Melton (Referee)

Referee comment on "Drivers of intermodel uncertainty in land carbon sink projections" by Ryan S. Padrón et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-92-RC2, 2022

This manuscript takes 11 ESMs from the CMIP6 archive and attempts to unravel the root causes of the uncertainty in the NBP simulated by each model over the (roughly) 2 deg warming scenario. The models' sensitivity to CO2 (through the 1 percent runs) and to temperature and soil moisture are investigated for both short and long timescales. I found the paper figures to be generally well designed (though see my comment about Fig 3) but the text could be confusing at times. There is a lot of rather convoluted steps/arguments in producing the T/SM/sT/sSM metrics and it sometimes was hard to understand exactly what they were telling me about the models. I think the paper is publishable, but needs revisions for clarity. An obvious target for clarity/context would be to discuss the results of this work in the context of previous efforts as discussed in the introduction (principally Arora et al. 2020). I found myself comparing this work to that paper and not understanding why they differed strongly in some cases.

Main comments:

Fig 3: I found this to be a strange figure. So if a model has a positive correlation it counts towards the blue end of the colour scheme, whereby if it is negative it counts towards the red. However this seems to have no consideration of how positive or negative a model was. I think it would treat a model that is 0.9 the same as one that is 0.0009, which seems to be a bit too ambiguous. Also what if all 11 models are +0.0001 vs. all models are >0.9, as is they would appear the same in this figure but arguably the situation where all 11 are >0.9 is more interesting than the situation where the models are rather ambiguous (close to 0). I would suggest reconsidering this figure.

Deserts - how did you mask the deserts? I think the grid cell sizes of the ESM precludes removing many deserts, e.g. the Atacama. Instead it seems like only a few were removed (Sahara, around Middle East, and Gobi) but I am not sure why those made the cut but not, for example the deserts of western Australia or the US SW. What impact does it have keeping them in? Greenland makes sense since there is no vegetation at all but some of the world's dry regions have been fingered as influential in the global C cycle (e.g. Ahlström et al. 2015), so exactly where masking applies could have impact I would assume.

Fire - Fire is mentioned on line 335 but ignored otherwise, why? I see you mention which models do fire in Table S1.

Smaller comments:

Line 44: 'with drought-related observed decreasing trends in leaf area' consider rewording, confusing.

Line 103: CanESM has an implict N cycle (empirical downregulation scheme see Arora and Scinocca 2016)

L 160: This explanation is confusing. Perhaps spell it out in a bit more detail.

L 182: Why 22.5 degrees and not 25 or 30 or some other number? It just seems awfully precise for a seemingly arbitrary limit.

L 235: remind reader that both use CLM?

L 290: 'underestimation of the land carbon sink modelled by NorESM2-LM and CanESM5,' where is this shown? I can't seem to see any figure where CanESM5 sticks out with an underestimation of the land C sink but it is mentioned here and line 357, indeed in Figure 1 it seems to have one of the highest cumulative NBP. What am I missing?

CanESM5: Other papers (Arora et al. 2020) have suggested that CanESM5 has the largest land C uptake (at least for the 4X CO2 simulations) so it is surprising that it is suggested to be underestimated for the land C sink. Can you clarify how the same model appears to be on the low/high end depending on the analysis? I realize these are different scenarios but I would have assumed high CO2 sensitivity would follow in both (but be exaggerated in the 4XCO2 run), but I don't see high CO2 sens in Fig 7. I assume I missed something here as you mention the Arora et al. paper in the intro but don't return to place your results in context of those other works.

L 352: And assumedly many of them use Nemo for their ocean so model commonalities are not just atm/land. Never mind all who use Farquar photosynthesis etc.

L 378: 'Outperfom' seems out of place, consider swapping it out with something like 'be more important than'

Lit cited:

Ahlström, A., Raupach, M. R., Schurgers, G., Smith, B., Arneth, A., Jung, M., Reichstein, M., Canadell, J. G., Friedlingstein, P., Jain, A. K., Kato, E., Poulter, B., Sitch, S., Stocker, B. D., Viovy, N., Wang, Y. P., Wiltshire, A., Zaehle, S., and Zeng, N.: The dominant role of semi-arid ecosystems in the trend and variability of the land CO2 sink, Science, 348, 895–899, 2015.

Arora, V. K., Katavouta, A., Williams, R. G., Jones, C. D., Brovkin, V., Friedlingstein, P., Schwinger, J., Bopp, L., Boucher, O., Cadule, P., Chamberlain, M. A., Christian, J. R., Delire, C., Fisher, R. A., Hajima, T., Ilyina, T., Joetzjer, E., Kawamiya, M., Koven, C. D., Krasting, J. P., Law, R. M., Lawrence, D. M., Lenton, A., Lindsay, K., Pongratz, J., Raddatz, T., Séférian, R., Tachiiri, K., Tjiputra, J. F., Wiltshire, A., Wu, T., and Ziehn, T.: Carbon–concentration and carbon–climate feedbacks in CMIP6 models and their comparison to CMIP5 models, Biogeosciences, 17, 4173–4222, 2020.

Arora, V. K. and Scinocca, J. F.: On constraining the strength of the terrestrial CO2 fertilization effect in an Earth system model, https://doi.org/10.5194/gmd-2015-252, 2016.