Response to Reviewer #2

I thank Reviewer #2 for their comments, in particular General Comment #1. This additional analysis has improved the scope of the paper. My responses to each comment are below in blue.

Review of *The Biophysical Effects of Carbon Fertilization of the Terrestrial Biosphere*

The paper examines climate system responses to the biophysical effects of carbon fertilization using a subset of the C4MIP experiments from CMIP6. The paper advances understanding by employing a surface energy balance decomposition, which allows the author to quantify the individual contributions of biosphere-induced climate system changes on surface temperature responses. The paper is well written, and the analyses are appropriate. I have several suggestions that I hope the author will address in the next draft.

General Comments:

1. The experiments here quantify the carbon fertilization effect by subtracting a Preindustrial climate from the end of the 1pctCO2 climate. However, you can also quantify the carbon fertilization effect by subtracting the end of the 1pctCO2-rad climate from the end of the 1pctCO2 climate. Calculating the CO2 fertilization effect using this method provides insight into the influence of the carbon fertilization effect within the context of a warmer (higher atmospheric CO2 concentration) climate. I suggest the author examine whether the climate system responses shown in the current manuscript are consistent with the climate system responses using this alternative method. They do not need to replicate all of the analyses, but should at least focus on replicating the results in the Main Figures 1-3. This will help to understand how robust the changes are to the background climate state, and may lead to new insights that inform the previous analyses.

Thank you for this suggestion. Although this has involved considerable additional work, I have performed the companion analysis using 1pctCO2 minus 1pctCO2-rad. Additional figures and an additional section are included in the revision.

2. There has been a considerable amount of work dedicated to the analysis of climate responses in these C4MIP-type experiments. The author should expand upon their description of these works. Indeed, much of the results presented here are supported by previous investigations. Some possible papers to describe (among many others):
    - Swann, A. L. S., F. M. Hoffman, C. D. Koven, and J. T. Randerson, 2016*:* Plant responses to increasing CO2reduce estimates of climate impacts on drought severity. *Proc. Natl. Acad. Sci. USA*, *113*, 10 019–10 024, https://doi.org/10.1073/pnas.1604581113.
    - Skinner, C. B., C. J. Poulsen, and J. S. Mankin, 2018*:* Amplification of heat extremes by plant CO2physiological forcing. *Nat. Commun.*, *9*, 1094, https://doi.org/10.1038/s41467-018-03472-w.
    - Lemordant, L., P. Gentine, A. S. Swann, B. I. Cook, and J. Scheff, 2018*:* Critical impact of vegetation physiology on the continental hydrologic cycle in response

to increasing CO_2. *Proc. Natl. Acad. Sci. USA*, *115*, 4093–4098, https://doi.org/10.1073/pnas.1720712115.

- Zarakas, C. M., A. L. S. Swann, M. M. Laguë, K. C. Armour, and J. T. Randerson, 2020: Plant Physiology Increases the Magnitude and Spread of the Transient Climate Response to CO_2 in CMIP6 Earth System Models. *J. Climate*, **33**, 8561–8578, https://doi.org/10.1175/JCLI-D-20-0078.1.

These references (and others) have been added to the revision.

3. The paper largely groups responses into the tropics vs extratropics. This is mostly appropriate given the distinct responses between those two latitude bands. However, there are some very interesting regional changes that the author should discuss. Namely, the zonally anomalous changes in the Southwest U.S. and parts of Western and Central Asia (e.g., increases in latent heating (from transpiration); reduced downwelling shortwave). These water-limited regions behave differently than energy-limited regions.

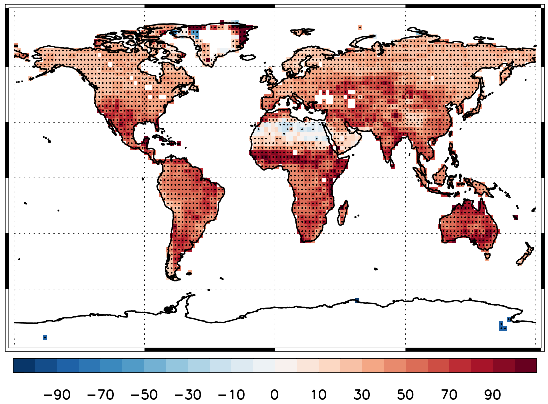
I have added some discussion on these water-limited regions.

4. Line 320-321: The author mentions that in areas with reduced transpiration, evaporation increases to satisfy the evaporative demand of the atmosphere. This may be partially true, but the primary reason for the increase in evaporation is likely because of increased canopy interception (from greater leaf area) and subsequent evaporation from the canopy.

I've added this point. Canopy evaporation increases by 1.02 ± 0.92 Wm^{-2} (9 of 13 models).

5. Lines 385-388: The large contribution of the surface albedo change to warming in semi-arid regions is likely because these are water-limited areas that see large percentage increases in LAI. These are the regions we expect to see the greatest parentage increases in photosynthesis/fertilization
   - Donohue, R. J., Roderick, M. L., McVicar, T. R. & Farquhar, G. D. Impact of CO_2 fertilization on maximum foliage cover across the globe's warm, arid environments. *Geophys. Res. Lett.***40**, 3031–3035 (2013).

Point made and reference added to the revision. The percentage change in LAI is relatively large in semi-arid regions, including for example the SW US (figure below).



6. Lines 390-392: You mention that the surface albedo effect is largest in the high latitudes due to the presence of snow and ice. Are the vegetation changes occurring in the presence of snow and ice? Aren't the vegetation responses to CO2 fertilization largely confined to the warmer season?

Yes, the annual mean albedo SEB term yields global land warming of 0.11 ± 0.06 K. In the tropics, the corresponding warming is smaller at 0.06 ± 0.04 K (11/15 models agree on the warming). In the extratropics, the corresponding warming is largest at 0.16 ± 0.09 K (13/15 models agree on warming). Based on the above figure showing the percent change in LAI, the larger extratropical warming due to surface darkening is not related to a larger percent increase in LAI. It is, however, consistent with the notion that vegetation induced surface darkening is larger when the vegetation covers brighter underlying surfaces, such as snow (e.g., Betts and Ball, 1997). There is obviously a seasonal dependency to this effect. For example, Li et al (2015) showed boreal forests have strong warming in winter and moderate cooling in summer with net warming annually, due to albedo effects dominating in winter and evapotranspiration effects dominating in summer, but with the albedo effects dominating in the annual mean.

Re-estimating the albedo SEB term by season in the extratropics (which is dominated by the Northern Hemisphere) shows the dominant warming effect occurs in DJF and MAM at 0.26 ± 0.15 K and 0.21 ± 0.12 K, respectively. Smaller warming occurs during JJA and SON at 0.07 ± 0.05 K and 0.11 ± 0.06 K, respectively. The larger SEB albedo term warming during the Northern Hemisphere cold months is consistent with the co-occurrence of snow and vegetation.

Betts, A. K., and J. H. Ball (1997), Albedo over the boreal forest, *J. Geophys. Res.*, 102(D24), 28901–28909, doi:10.1029/96JD03876.

Li, Y., Zhao, M., Motesharrei, S. *et al.* Local cooling and warming effects of forests based on satellite observations. *Nat Commun* **6**, 6603 (2015). https://doi.org/10.1038/ncomms7603

7. Can you speculate as to why GISS-E2-1-G simulates a decrease in LAI in response to CO2 fertilization? Is there something specific to the treatment of photosynthesis?

The GISS-E2-1-G vegetation model, the Ent Terrestrial Biosphere Model (Ent TBM; Schmidt et al., 2014; Kim et al., 2019; Ito et al. 2020) consists of multilayer canopy radiative transfer model (Spitters, 1986) and leaf gas exchange using the Ball-Berry stomatal conductance model (Ball and Berry, 1985) coupled with the Farquhar-von Caemmer photosynthesis model (Farquhar and von Caemmerer, 1982). Autotrophic and heterotrophic respiration is parameterized as in Kim et al. (2019).

GISS-E2-1-G prescribes fixed 2004 monthly LAI, so the model does not capture the impact of carbon fertilization on LAI. The change in LAI in GISS-E2-1-G is essentially zero at -0.00065. This has been clarified.

Ball, J. T., & Berry, J. A. (1987). A model predicting stomatal conductance and its contribution to photosynthesis under different environmental conditions. In I. Biggins (Ed.), Progress in Photosynthesis Research (Vol. IV, pp. 110–112). Dordrecht, Netherlands: Nijhoff.

Farquhar, G. D., & von Caemmerer, S. (1982). Modelling photosynthetic response to environmental conditions. In O. L. Lange, C. B. Osmond H. Ziegler (Eds.), Berlin, Encyclopedia of Plant Physiology (NS) (12B. pp. 549–587). Berlin: Springer.

Ito, G., Romanou, A., Kiang, N. Y., Faluvegi, G., Aleinov, I., Ruedy, R., et al. (2020). Global carbon cycle and climate feedbacks in the NASA GISS ModelE2.1. *Journal of Advances in Modeling Earth Systems*, 12, e2019MS002030. https://doi.org/10.1029/2019MS002030

Kim, D., Lee, M., & Seo, E. (2019). Improvement of soil respiration parameterization in a dynamic global vegetation model and its impact on the simulation of terrestrial carbon fluxes. *Journal of Climate*, 32(1), 127–143. https://doi.org/10.1175/JCLI-D-18-0018.1

Schmidt, G. A., Kelley, M., Nazarenko, L. et al. (2014). Configuration and assessment of the GISS ModelE2 contributions to the CMIP5 archive. *Journal of Advances in Modeling Earth Systems*, 6(1), 141–184. https://doi.org/10.1002/2013MS000265

Spitters, C. J. T. (1986). Separating the diffuse and direct component of global radiation and its implications for modeling canopy photosynthesis. Part II. Calculation of canopy photosynthesis. *Agricultural and Forest Meteorology*, 38(1-3), 231–242. https://doi.org/10.1016/0168-1923(86)90061-4

Technical Corrections:

1. Line 230-231: "Similar but less significant statements…". Reword this. It is unclear what less significant means here.

   Deleted "but less significant statements".