

Interactive comment on “Differences in carbon cycle and temperature projections from emission and concentration-driven earth system model simulations” by P. Shao et al.

P. Shao et al.

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1. All these comparisons are performed without any clear scientific question and accordingly the choice of analyzed variables looks quite arbitrary (many other variables exist in the CMIP5 archive).

Reply: The last paragraph in Introduction of the original manuscript did present the purpose of our study and the two scientific questions to be addressed. We have added sentences in the revised manuscript to give better justifications for focusing on the specific analyses in our study. For instance, we have already analyzed other carbon cycle variables (GPP, NPP, RA, RH, and NBP, etc.) and climatic variables (soil moisture, temperature, and precipitation) in our previous studies (Shao et al., 2013a, b).

2. In principle differences in concentration driven and emission driven experiments could reveal insight into the importance of the re-shuffling of carbon between ocean and land for the global carbon cycle because this re-shuffling is suppressed in the concentration driven experiments. But this issue is not tackled in the paper.

Reply: This re-shuffling is a major focus of our study; for example, it was addressed through the change of land and ocean carbon storages, and diagnostic emissions (Figs. 1b, c, d). We also used the ratio of ocean carbon accumulation over atmospheric accumulation, following Hoffman et al. (2014), to investigate the evolution of the contribution of ocean uptake to the atmospheric [CO₂] (Page 998 line 3–5 of our original manuscript). Therefore, we don't understand why the reviewer claimed that “this issue is not tackled in the paper”.

To further increase the clarity, we have added sentences: (a) to include the [CO₂] mass balance equation, and emphasize we discuss different terms of this equation; and (b) to discuss briefly the adjustment process (i.e., emission increases atmospheric [CO₂], which then forces the adjustment of land and ocean fluxes to satisfy the mass balance). Also see our Reply to Major Comment #1 of Dr. Jones (Reviewer #1).

3. Moreover, the discussion of a slightly modified sensitivity index alpha looks like an arbitrary add-on to the paper because no concrete scientific conclusions are drawn from its introduction (the proposed use in the Friedlingstein feedback formalism makes no sense).

Reply: To address this comment and those from other two reviewers, we have (a) deleted the sentence on the γ term, (b) emphasized the use of α and α' as a diagnosis of the climate response to CO₂ change, and (c) emphasized the need of using (linear) α , rather than (nonlinear) α' , in the carbon-climate feedback formalism of Friedlingstein et al. (2006).

Please note that the new insights gained from our study are summarized in our Reply to Major Comment #1 of Reviewer #3.

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