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Geoscientific Model Development Discussions

Interactive comment on "On constraining the strength of the terrestrial CO_2 fertilization effect in an Earth system model" *by* V. K. Arora and J. F. Scinocca

Anonymous Referee #2

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[General comments]

Authors present in this paper the structure of the new Earth system model developed in CCCma, and then they attempt to evaluate the model's performance to reproduce the global carbon budget and atmospheric CO2 concentration during 1850-2005 periods, with simulation ensembles and different parameters/configurations. In their evaluation, they focus on particularly the land ecosystem process so called "CO2 fertilization effect", which is strongly associated with the most uncertain feedback process within the global carbon cycle. It is noteworthy that the authors consider four types of observation constraints in their model evaluation, which makes their conclusions more robust. Overall, this paper is clearly written and well structured, and will contribute to the jour-

nal. Detailed comments are listed below, and I believe most of them will not require much effort to improve.

[Detailed comments]

p4, L7- "the uncertainty in the carbon-concentration feedback over land had somewhat reduced since the first coupled carbon cycle climate model intercomparison project (C4MIP)" I'm afraid this sentence might mislead readers. Since the 1st and 2nd MIP used different scenarios (SRES-A2 / 1pctCO2) and configurations (emission/concentration-driven) to evaluate carbon cycle feedbacks, we cannot directly compare the feedback strength between the two MIPs.

P10, L23-

It will be helpful for readers to briefly mention the decay-timescale of the pools for "short" and "long" (: from Arora and Boer 2011, it seems the two product pools are equivalent to litter/soil). This information will be helpful to understand the reduction of soil carbon mass in LUC simulation and the delayed response of soil carbon pools (Fig. 5c).

P23 L19; p24 L10; p27 L5

Should these "CanESM2" be replaced by "CanESM4.2" ?

P24 L28- p25 L2

In my understanding, your choice of "emission-driven" configuration might be one of the reasons to underestimate the LUC emission (E∼L): since LUC emission is omitted in the "without LUC experiments", the CO2 concentration stays lower level and the CO2 fertilization effect becomes weaker. As a result, the cumulative land carbon uptake in the "without LUC" experiment (FL') is more or less underestimated, which yields lower E∼L (=FL' - FL). I recommend the authors to mention this.

Discussion section

As commented above, simulations without LUC inevitably lead to lower CO2 concentration and weaker CO2 fertilization effect. I think this can be a “noise” when evaluating LUC emission/impacts. Specifically, in Fig.4(b), NPP in “without LUC” simulation are generally lower than “with LUC”, but it is difficult to identify the reason of the difference, because the NPP difference can be affected by CO2 fertilization, increased GPP by crops, and vegetation regrowth. I hope the authors to make a few discussions about the configuration settings for evaluating LUC impacts. I believe such information will be helpful when making simulation designs in the coming CMIP.

In Fig.2, Gamma_d=0.25 simulations display moderate land carbon sink among CMIP5-ESMs. I think this result is reasonable because most CMIP5-ESMs may not consider down-regulation mechanism; Fig.9 also supports the choice of the parameter value. However, the historical simulations with gamma_d=0.25 did not do a good job for reproducing land carbon uptake (Fig. 4). Although you discussed on this in the text, I suppose we have two more things to discuss. The first is the additional carbon uptake by vegetation regrowth. Although the regrowth mechanisms in the model are presented on p10-11, I’m not sure if the modeling was appropriate or not. If we can expect more carbon gain by vegetation regrowth, simulations with gamma_d=0.25 may work better. The second is the parameter value of humification factor. If you choose more moderate value for the humification factor (or modify the fractions of deforested/removed biomass that goes into fast/slow pools), soil carbon mass displayed in Fig. 5c will push up toward positive, and this treatment will also make the simulation with gamma_d=0.25 more realistic. . . I hope to see some discussions on these two points.

About Title:

I’m thinking the key feature of this paper is constraining the historical carbon budget of the model from different angles. Of course, it is necessary for your model to choose an appropriate value for the down-regulation, but its parameterization looks somewhat specific to your model. My suggestion is to change the title to reflect “CO2 fertilization”, “LUC”, and “historical carbon budget”: I believe these are the main issues in the

background and will have more meaningful messages for readers.

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