

September 21, 2014

We thank all the reviewers of our manuscript (gmd-2014-13) for their thoughtful comments, and give our responses below. Line numbers refer to the new version of the text (in which changes are marked in red).

Referee #1

The manuscript is in a better shape than before but it still needs some revisions. Abstract still needs improvements. It doesn't deliver the primary message clearly and in fact contains a few incorrect statements. The overall "loose" coupling of CLM and GCAM appears so "non-scientific". I am not sure if it is the way it is described or if it actually is, probably the former.

Abstract. Only on reading the manuscript the second time it was obvious to me what authors meant by "... which can incorporate land-use change as static inputs but do not simulate the policy or economic forces ...". This sentence still doesn't make sense. How can land-use be "changing" and still "static". What authors mean here is likely "prescribed". That land-use change is prescribed makes sense. I still can't appreciate "but do not simulate the policy or economic forces". This needs to be better worded. We have added "prescribed" and "actively" in what will hopefully clarify the sentence further (lines 24-25).

Abstract. "... focusing on how CESM climate effects on the carbon cycle can be shared with GCAM". No, this can't be your focus since this is an offline study. Well, the iESM itself is fully coupled. Changed "can" to "could" (30).

Abstract. "... quantify how carbon flux changes driven by climate (e.g. CO2 fertilization) ...". CO2 fertilization is NOT a climate effect. Wording slightly changed (32).

Abstract. "... to manipulate GCAM's assumptions of equilibrium ecosystem steady state carbon". Please consider using another word for "manipulate". Wording changed (36).

Line 55. "... different policy choices vis-à-vis LUC and carbon may result in greatly different configurations of the future carbon cycle". Still a vague sentence. Wording changed and hopefully sharpened (57-58).

Lines 64-66. "Such models may incorporate LUC as static inputs, but do not simulate policy options or economic forces, a significant limitation given how strongly humans can perturb the earth system". See comments above regarding "static" and "simulate policy options or economic forces". Changed "static" to "prescribed" (64).

Line 99. “CLM simulates the cycling and land-atmosphere exchange of energy, water, chemical elements, and trace gases”. CLM simulates fluxes of CO₂ and N₂O, as far as I know. “land-atmosphere exchange of chemical elements” sounds confusing. Please be explicit here.

We agree that “chemical elements” was confusing, and this has been changed (100). On the other hand, we don’t think it’s necessary to list all the processes CLM simulates.

Line 122. Market-clearing prices is a term unknown to ESM community.

This paper is written for both the ESM and IAM communities. We recognize that this may require readers to deal with concepts outside of their disciplinary specialty.

Line 126. What is a logit mathematical model?

This was probably an unnecessary distraction to include, and has been removed (125).

Line 130. “GCAM’s carbon model”. Wouldn’t a more appropriate term be “GCAM’s terrestrial carbon cycle model”.

This is more precise—thank you (129).

Line 137 and 140. “When a particular land-use contracts” and “When a land-use expands”. Wouldn’t it be appropriate to tell your reader what kind of land-uses GCAM deals with to provide some context.

We have added a short clarification for this (132, 137-144).

Line 168-169. “have a suitable proxy by which to change GCAM’s steady-state carbon assumptions”. You are not changing GCAM’s steady state carbon assumptions (it still assumes that pools are steady state) but rather the magnitude of the pools which GCAM assumes are in steady state. Please also address this subtlety elsewhere in the manuscript.

That is indeed subtle. We have added “the values of” to clarify (169).

Line 171. “ ... if the carbon stock of a CLM forest changes from one time step ... ”. Please reword to “ ... if the land carbon pool size of a grid cell with forested fraction simulated by CLM changes ... ”

Changed as requested (172-173).

Line 205. “In simulations S2-S4, we used the same looped climate, ... ”. Reword to “In transient 1850-2010 S2-S4 simulations, we used the 1901-1920 climate repeatedly ... ”

Changed to close to this wording (207-208).

Line 206, 207. Replace “factors” by “forcings” and elsewhere too.

In this case we find “forcings” a more ambiguous, and less appropriate, term, and have elected to leave this unchanged, unless the editor objects.

Line 237. “... even a perfect proxy variable will be subject to climate and LUC ... ”. You mean subject to “climate change and variability”. “subject to climate” doesn’t mean anything.

No, we think the meaning was correct, although have attempted to clarify the wording (240).

Line 258. “To distinguish LUC from climate signals ...”. Doesn’t this actually mean, “To distinguish the effect of LUC, on primary CO2 fluxes and land carbon pools, from that of climate ...”

We agree that this wording was poor, and have changed it (261-262).

Line 267. “The scalars were then ...” What scalars are being referred to here?
The scalars defined in the paragraph above.

Line 300-302. “ ... because a few CLM grid cells, located in GCAM’s “Middle East” region, were subject to LUC at the end of CLM’s spinup phase”. This is confusing for me as an ESM person. Typically a spin up is performed when all forcings are fixed. For example, a typical ESM control run spinup is done with land cover fixed at 1850 conditions and concentration of all GHGs at their 1850 values. In the context of this paper the spinup SI is done with 1901-1920 climate used repeatedly. Table 1 doesn’t say anything about land cover so I presume it would be specified and not changing but the above sentence implies that land cover is changing. If land cover is changing in a spin up simulation then how come it is a spin up.

My apologies for the incorrect wording; corrected (307).

Equations (1) and (2). Shouldn’t equation (1) be more appropriately written as
$$CA_i = CA_0 \times (NPP_i/NPP_0)$$

Both other reviewers pointed out this issue, and it has been changed. Thanks.

In regards to equation (2), isn’t a more appropriate quantity to describe change in the belowground carbon pool $(NPP - HR)_i / (NPP - HR)_0$, as opposed to the average of the NPP_i/NPP_0 and HR_i/HR_0 ratios. After all the rate of change of soil carbon is equal to NPP minus HR, assuming NPP equals litterfall rate. In a transient simulation NPP increases faster than HR, so the difference between the two will reflect how soil C pool is changing.

When $NPP=HR$, as is true (on average) in steady-state conditions, these formulations should be equivalent. They won’t be equivalent when the two fluxes diverge, and we agree it would be interesting to explore the practical effect of this alternative. For now, we’ve left equation 2 unchanged, but thank the reviewer for pointing this out.

Also, I presume, equations (1) and (2) are applied every 5 years.

Yes. This has been clarified (321).

Line 336. “The initial change in NPP was able to explain 20-92% of the variance ...”. You mean, “The change in NPP over the 2005-2009 period was able ...”

Clarified (341-343).

Lines 348-353. These sentences are redundant and provide no useful information.

We disagree, although have condensed the text.

Section 3.3. Wouldn't a more appropriate title be "Distinguishing the effects from LUC from climate"

We agree this was verbose, and have changed the section title as suggested (360).

Line 360. "... that as long as fewer than ~25% of the simulation cells were perturbed, the error (between the known climate signal and that inferred by the outlier test) .." What exactly is this error? What are its units? What are the units of the "climate signal"? I am sorry but I am unable to understand what error is being talked about here.

We have clarified the methods text (281-289) in this area.

Line 375. "Climate changes" doesn't seem right. It's either "Climatic changes" or "Climate change".

This is a matter of style, not correctness, but has been changed (262, 380).

Line 377,378. "In addition, it guarantees that if climate changes affect the carbon cycle, GCAM's equilibrium assumptions will change correspondingly for the same vegetation type and spatial location, feeding back into economic decisions about industrial and LUC CO2 emissions (e.g., Le Page et al., 2013), emissions that propagate back to CLM (Di Vittorio et al., 2014)". Are you sure, this is incorrect. I don't think LUC CO2 emissions from GCAM propagate back to CLM. It's the land use information, which is then translated to land cover information that is seen by CLM.

Thank you for catching this mistake! Changed (381-382).

Lines 384, 385. "In addition, the outlier-exclusion step will break down under extreme LUC scenarios, scenarios that can be a useful tool". I am unable to understand the last part of this sentence – "scenarios that can be a useful tool" – how does this relates to the first part of the sentence.

Clarified (388-389).

Line 400. "This work is only one step to a full coupling of an ESM and IAM; the second is described by Di Vittorio et al. (Di Vittorio et al., 2014)." Either don't mention the second step or if you do want to mention it then please briefly explain what is it.

We've added brief text explaining this second step (390-392).

Figure 5. I am unable to understand Figure 5. Like I mentioned earlier, what exactly is error. What are its units? The caption says "error between the inferred climate change signal and the known (artificial) signal". But what is "climate change signal"? Caption says x-axis is "LUC intensity" and the figure itself says "LUC effect".

We hope that the revised methods text has clarified this, and have added additional explanatory text to the figure caption as well.

Referee #2

Page 14, formulas (1) and (2): $C_{\{A\}}$ and $C_{\{B\}}$ would need an additional index to indicate the time step

We have clarified the notation and accompanying description.

Page 15, line 336: I don't see where the 20-92% comes from. Looking at the R squares Table 2, I can see a range of 6 to 92%. Please make sure to correct, and also not to round only the lower end in order to make things look nicer than they potentially are.
Corrected (341-343).

Page 16, line 347: Please include by how much (in relative terms) the regression slopes changes. Are we speaking about a 1, 5, or 50% change in the slope?
This is now specified (351-352).

Page 16, line 361: From figure 5, I cannot see where the "<10%" is coming from. For perturbations of 25% of the cells or less, I would say there is a maximum error of about 20%. Please correct.
Corrected (366); thank you.

Page 17, lines 381-387: This discussion of caveats should explicitly include the limitation that this approach has been tested for a scenario with modest climate change (RCP4.5). It's validity for high climate change futures is still to be tested.
We have added a sentence noting this issue (390-392).

Page 34, Figure 2 – caption: As far as my eyes are not tricking me, the acronyms are still not consistent, even not between caption and figure. Please go over this once again.
As far as we see, they are correct (extremely for a minor capitalization difference for "TOTC", which has been changed).

Page 35, Figure 3: Please also show the result for the final design decision and methodology.
Because of the short timeframe of this revision, I was not able to access these results (the EDISON supercomputer's file system has been offline). I have added a sentence (683-684) to the text noting there was no divergence, but not updated the figure; if the editor thinks this is critical, it can be changed.

Page 36, Figure 4: Please color the points that were excluded due to the outlier test.
The simulations (E1 and E2, lines 217-224) that produced these data did not incorporate the outlier test.

Page 37, Figure 5: I do not understand what the isoline labels are trying to indicate, as there appear to be isolines every 20% and the caption indicates that they show errors for every 25%.
Corrected (694).

On the argumentation for the choice of RCP4.5 vs 8.5: The only valid argument here is probably that RCP8.5 would have involved more work. Both RCP4.5 and RCP8.5 are part of the first tier of CMIP5 experiments and have thus been run by all climate modelling groups. Moreover, for RCP8.5 a large perturbed initial condition ensemble is

available from CESM, which arguably would be able to provide even more interesting information into the importance and influence of internal variability of a complex AOGCM on the uncertainties of the relationships suggested here.

We agree that RCP8.5 would have been a very interesting test, but also agree that it would have been much more work! Specifically, RCP4.5 was produced by the GCAM modeling team, so all the necessary input files and socioeconomic assumptions were ready to go. By contrast, RCP8.5 was produced by the MESSAGE IAM.

Small editorial comments:

Page 4, line 71: It is better to cite more recent papers which include the model description.

Meinshausen, M., et al. (2011). "Emulating coupled atmosphere-ocean and carbon cycle models with a simpler model, MAGICC6 – Part 1: Model description and calibration." Atmos. Chem. Phys. 11(4): 1417-1456.

Meinshausen, M., et al. (2011). "Emulating atmosphere-ocean and carbon cycle models with a simpler model, MAGICC6 – Part 2: Applications." Atmos. Chem. Phys. 11(4): 1457-1471.

References updated (71).

Page 7, line 140: No “-“ in land use.

This is actually correct, we think, because it’s modifying an implicit noun. We have added “category” to make the syntax clear (137, 140, 144).

Page 9, line 200: comma after “two”

Fixed (202); thank you.

Referee #3

Overall, I found the paper to be concise and well-written. The group that is trying to couple CESM to GCAM should be commended for their systematic approach to this challenging problem. This paper represents an important step both from a conceptual point of view in terms of how to complete a loose coupling and from an understanding point of view with respect to how the carbon cycle operates in CLM and in GCAM.

We are gratified that the reviewer found this clear, and the topic interesting!

1. p. 10, line 227: In these constant LUC experiments, how is wood harvest treated. In fact, it wasn’t clear how the impact of wood harvest was treated/considered throughout the paper. Wood harvest will change the stocks and may affect the climate change response, but may not result in a big shift in fluxes so could pass through the outlier exclusion test (?). Maybe this is fine, but some discussion is probably warranted.

We agree that this is important, but it’s a feature of the GCAM-to-CLM coupling step described by Di Vittorio et al. (2014), not the CLM-to-GCAM step described here. This is now noted (407-409).

2. *Line 282: Does this revised text better represent what you are trying to say here: “this inferred climate change induced carbon stock signal was then compared to the original known climate change induced carbon stock signal.” If so, maybe change the wording to clarify.*

That’s much better (286-289)–thank you.

3. *Equations 1 and 2: I think it would be clearer to include a subscript for CA and CB. E.g. $CA_i = CA_0(NPP_i/NPP_0)$. I am assuming that CA_0 , is the initial equilibrium carbon stock based on Houghton and that at each timestep i , this carbon stock is adjusted to CA_i .*

This has been changed per Reviewer 2’s similar comment. We have omitted the i subscripts for clarity, but adopt exactly your suggested CA_0 notation.

4. *Along the same vein, what exactly do you mean by timestep here? I think that you mean the IAM timestep of 5 years. Does NPP_i represent the average NPP over the prior 5 years? What exactly does NPP_0 represent?*

We have added text (321, 322-324) clarifying these points.

5. *Line 401: Should describe in a sentence or two the second step that is described in Di Vittorio et al.*

We have done so (406-409), thanks.