

June 5, 2014

We thank both referees for their careful and thoughtful comments.

Response to Referee #1 major comments:

1. *I am unable to understand the basic premise of how the coupling works since the GCAM model hasn't been introduced properly.*

We have greatly expanded the description of GCAM (section 2.1), describing it in the methods text as well as giving additional citations, focusing in particular on the model's carbon cycle. (That said, GCAM's algorithmic complexity is similar to that of an ESM, and inevitably we have to point the reader to other references for full detail.)

2. *There are terms in the paper that probably both the ESM and the IAM communities are unfamiliar with.*

Thank you for pointing this out. We now define terms consistently and carefully throughout.

3. *The basic concept of the long-term pools changes hasn't been explained properly and is unclear.*

This is tied to #1 above, and is now clearly—we hope—described in the GCAM model description section.

4. *It is also unclear what the final iESM simulation was. Was this a 2006-2100 RCP 4.5 simulation?*

We have clarified throughout the manuscript, as well as in the abstract and title, that this work does not describe a “final” iESM simulation, merely the development and testing of one half of the ESM-IAM coupling of the larger system.

5. *CESM1 is not just CLM. It seems that the authors forgot about other components of the Earth system model. Is the CLM the only place in the CESM1 where it is coupled to the GCAM. What about the simulated climate in the CESM1? Doesn't it affect the behavior of the GCAM IAM.*

We apologize for the lack of clarity on this point. Currently CLM is the only component of CESM1 that ties directly to GCAM (although this will change in the future). Climate does affect the GCAM IAM via its effects on the CLM carbon cycle. This is now described explicitly in the text.

6. *It is also unclear how is this a problem of steady-state as the title of the manuscript suggests...*

We have changed the title of the manuscript, per Referee #2's suggestion, to more accurately reflect the subject of the paper.

7. *In fact, if the CRUNCEP data is being used to drive the CLM model then how is this even a coupling of the CESM1 and GCAM models. In this case, it's not the ESM and IAM that are being coupled. It is coupling of CLM and GCAM models, driven offline with CRUNCEP data or climate from the CESM1 RCP 4.5 simulation. At least, that's the impression I got as a reader after reading page 1505.*

That is correct, and we have clarified this in the text.

Minor comments:

8. *Abstract. The opening sentence of the abstract seems a bit difficult to follow. In addition, it reads like as if a modeller is complaining – “Human activities are . . . posing a significant problem for ESMs . . .”*

This has been reworded and clarified.

9. *Abstract. Lines 3-4. “...which may incorporate static land-use change inputs...”. The term “static land-use change inputs” doesn't mean anything to me even as an ESM person. Maybe the authors actually mean “static land cover”.*

Our intention here was to note that LUC inputs to ESMs must be specified ahead of time, and don't change during a run (e.g., in response to evolving conditions in the ESM). We have clarified this sentence.

10. *Abstract. Line 10. Land use driven flux changes of what quantity?*

We're unclear how the quantity is relevant here, and apologize if we have misunderstood the reviewer's question. The sentence has been reworded.

11. *Page 1501, Line 5. The following sentence is not exactly true. “As a result, different policy choices vis-à-vis LUC and carbon are projected to result in greatly different configurations of the future carbon cycle and climate system.” If anything, the Brovkin et al. (2013) paper shows that the future land use change emissions and any effect on climate is much smaller compared to what the effect of historical land use change has been. Houghton estimates that historical LUC emissions are around 150 ± 75 Pg C. Figure 7b in Brovkin et al. (2013) shows that the effect of LUC in RCP 2.6 and 8.5 scenarios is around 10-50 Pg C, if the*

outlying large values from the MPI model are neglected. And, we know MPI generates such large LUC emissions because of its large initial pool sizes.

We have changed our citations here, while still acknowledging Brovkin et al. (2013) and their findings.

12. *Page 1501, Lines 8-13. The sentence “Such models may incorporate static LUC inputs, but do not actively, or interactively, simulate policy options or economic forces, except for example in simple (in a policy sense) “thought experiments” ...is unclear and confusing. Again static LUC inputs doesn’t mean anything. Do the authors mean static land cover?*

We have clarified this sentence.

13. *Page 1502. After the introductory section, the authors delve straight into the details of the CLM model without telling the user what purpose does CLM serves in the CESM1 model. A simple sentence telling that CLM simulates land-atmosphere fluxes of energy, water and CO2 would have been very helpful. Just throwing off big terms - carbon and nitrogen dynamics, biogeophysics, biogeochemistry etc. isn’t very helpful here. The model needs to be described in a much better way. Also note that, CLM doesn’t do the hydrologic cycle. It models the surface hydrological process. Hydrologic cycle is modeled by the full Earth system model.*

We now describe CLM’s role in CESM, and have slightly expanded other parts of this paragraph. We respectfully disagree, however, that a detailed description of CLM is necessary here. Finally, the text now says “surface hydrology”, although the CLM website (<http://www.cgd.ucar.edu/tss/clm/>) does describe it as modeling the “hydrologic cycle”.

14. *Page 1502. The following sentence “The model’s C and N cycles are closely coupled and include assimilation, plant growth and mortality, allocation, and subsurface cycling.” ...is another example of a poorly written sentence. Assimilation of what? Mortality of what? Allocation of what? Sub-surface cycling of what?*

We believe that “plant growth and mortality” is clear, but have clarified other terms in this sentence.

15. *Page 1503, Line 5. “is a dynamic recursive economic model” Just like carbon and nitrogen dynamics mean nothing to an economic modeler, the term “dynamic recursive economic model” means nothing to me as an ESM person. The paper needs to serve both communities and this means some extra effort needs to be put to make it palatable to both.*

Here, “dynamic recursive” means that decision-makers have imperfect information about the future (specifically, decisions made in one time period do not reflect future changes in population, GDP, technology, or prices). We have decided that this phrase is not relevant to this paper and potentially confusing, and have removed it from the text.

16. Page 1503, Line 13. What does “optional policy scenarios” mean?

In an IAM like GCAM, the future can be run with optional “policies”, e.g. a carbon tax at national or international levels. This has been clarified in the text.

17. Page 1503, Lines 21-25. *The IAMs focus on land use change, while what is actually needed by ESMs is land cover. ESMs need to know what kind of PFT grows (or can grow) in a grid cell. The intermediate step of going from GCAM’s land use change decisions to a land cover product that CLM can use is an important coupling step. Yet, this hasn’t been explained properly and the reader is pointed to two references. In my opinion, this important coupling step must be explained properly in the manuscript.*

We hope that our revisions (new title, clarified text, etc.) make clear that this manuscript does not aim to describe the full iESM system, but rather one specific aspect of it: the link from CLM to GCAM, and this is now noted in this paragraph. The GCAM-to-CLM link is quite complex and described fully by Di Vittorio et al. (2014), cited in the text.

18. Page 1503, Line 30. “...the former tracks real-time...”. What authors mean here is probably “time-varying” and not “real-time”. Page 1504, Line 10. “real-time” on this line and elsewhere probably also means “time varying”.

That’s a better term—thank you. Changed and/or removed.

19. I am a bit confused after reading Section 2.3 (lines 21-28)...An introduction to a simple equation like this and somehow relating this to what GCAM needs will likely help to understand how the coupling is being performed.

We have modified and clarified section 2.3, and added a new Table 1, providing more detail on the simulations and tests performed.

20. Page 1505, Line 1. Authors say “We tested the feasibility of these proxies in two ways”. What does “these” refers to in this sentence. Is it the vegetation, litter and soil carbon pools? Again, it would have been helpful to introduce the basic equations that describe the terrestrial carbon balance in the CLM and GCAM models, so that the linkages between the two can be easily explained by the authors. I am also unclear why the primary terrestrial carbon cycle fluxes and pools are described as proxy variables.

Please see previous response.

21. *Page 1505, Line 14. Please start a new paragraph when start discussing the CRUNCEP data. The way the simulations S1 through S6 are described it seems CESM1 was never run coupled to GCAM and all the simulations are offline in which CLM is driven with either CRUNCEP data or data from CESM1's RCP 4.5 simulations.*

We have clarified the extent of coupling in the simulations, here and throughout; it's correct that they were all offline, i.e. using CLM but not the fully coupled CESM1 system.

22. *Page 1505, Lines 17-18. "... and land use boundary conditions constant at their 1850 values." Again, probably what is meant is land cover was kept constant corresponding to year 1850.*

Clarified.

23. *Page 1506, Lines 1-9. I am unable to grasp the basic concept here. For example, I am unsure why the following is necessary. - "We examined the degree to which (i) NPP in the first 5 years of simulation S5 predicted total vegetation carbon in the final 5 years . . .". In absence of equations relating the output from the CLM model to the needs of the GCAM model, I am unable to understand this. It is also possible that, as an ESM person I am unable to follow the simple world of GCAM in which only equilibrium values of the pools matter. If this is the case, then the GCAM's philosophy needs to be explained properly.*

This text, and the entire section, has been extensively reworked and clarified. A new table summarizes the model runs performed, and the runs themselves have been renamed for clarity.

24. *Page 1506, Line 10 reads – "The overall control run—our base reference simulation—was based on the RCP4.5 stabilization pathway..." Yet, on page 1505 simulation S1 was introduced as the control simulation driven with CRUNCEP data for period 1901-1920 used repeatedly. Are there two kinds of control simulations?*

We agree that this text was confusing. We have deleted the second reference to a control run and clarified that simulation S1 is the control run only for the initial set of offline simulations S1-S4. In addition, a new Table 1 summarizes the simulation, some of which have been renamed for clarity.

25. *Page 1506, Lines 25-26. "For the iESM, even a perfect proxy variable will be subject to both climate and LUC during a CESM run, both before the run starts..." I am unable to follow this sentence.*

This text, and the entire section, has been extensively reworked and clarified.

26. Page 1507, Lines 3-5. *“Conversely, significant expansion of a PFT (e.g., agriculture reverting to forest) during the iESM run might appear to have drastically lowered productivity, leading GCAM redirect resources away from that region”. First, is the agricultural productivity higher than forests in CLM because of the higher per unit leaf area maximum photosynthesis rates for crops. Second, what does “redirect resources away from that region” means. You have to keep in mind as authors that the ESM community is not familiar with the GCAM model, and vice versa.*

We have clarified that these sentence refer to GCAM, not CLM, and explained in greater detail how this potential problem can happen.

27. Page 1507, Line 9-10. *“GCAM will, if it sees out-of-line values, potentially pour more resources into those cells, leading to a runaway feedback”. Sorry, I have no clue what this means. What are the units of resources? Is it money (\$), energy (W/m²) or something else? Without being introduced to the basic manner in which GCAM works, very few people in the ESM community can understand what this means.*

Here “resources” refers to land. In GCAM, extreme productivity values will result in high agricultural production and thus profit. Since the model allocates land based on profitability, this in turn leads to a change in land allocation towards a PFT (if its productivity is extremely high) or away (if its productivity is extremely low). This has been clarified in the text.

28. Page 1507, Line 11-12. *“...and profit maximization is the fundamental decision-making criterion in GCAM.” What is the currency of profit, money (\$) or carbon? Profit is what context?*

By profit maximization, we mean maximizing economic profit (revenue minus cost), measured in U.S. dollars; that is, landowners choose the land use that results in the largest amount of money. This is now clarified.

29. *I am unable to follow the basic premise of Sections 3.1 and 3.2.*

We have made many changes to these sections, and hopefully the changes to section 2 make all the results easier to understand.

30. Page 1511, Lines 17-18. *“By allowing climate effects from a full earth system model to modulate, in real time, the economic and policy decisions of an integrated assessment model...” I am confused if a full ESM simulation was performed in this study or not.*

This is a very good point, and one we have tried to clarify throughout the text.

31. Page 1522. Figure 3. To which year of the RCP 4.5 simulation does the result correspond? Color figures are free in online journals. Please consider using a color version of Figure 3.

We now provide a color version of Figure 3. The data shown in this figure are from the year 2065; this is now made clear in the caption.

32. Page 1524. Figure 5. What are the units on x-axis? Yes, the authors have explained LUC effect equals to 1 means no LUC. But what does a value of 5 means?

We have clarified the caption describing Figure 5.

We thank both referees for their careful and thoughtful comments.

Response to Referee #2 major comments:

1. *The title does not reflect the content: While the paper describes a small step in the coupling of the land component of the CLM model with the GCAM model, the title makes the reader believe something much larger: a full integration. Only one specific step in the CLM-GCAM coupling is described in detail in the manuscript. It would therefore be appropriate that the title reflects this content... Also it is unclear to me after reading the paper what the “problem” with steady state actually is, except for the fact that one needs to take into account that GCAM uses assumptions of long term ecosystem steady-state carbon to guide its economic decisions.*

We agree that the previous title was ambiguous and not very appropriate to the actual content of the manuscript. This has been changed, and both the introduction and conclusion now note that this work is only one (albeit necessary and not so small) step in coupling CLM with GCAM.

2. *The abstract is unclear and does not reflect what is presented in the paper. For instance, the abstract states that “By allowing climate effects from a full earth system model to dynamically modulate the economic and policy decisions of an integrated assessment model, this work provides a foundation for linking these models. . .”. As indicated above, the coupling is very limited and thus therefore not correspond to what was written above.*

We have attempted to clarify the abstract in many places, ensuring that it accurately reflects what is presented in the paper. We do however think that the final sentence is a fair summary of the current work (ensuring that climate change effects are reflected in the IAM carbon cycle), and it has been changed only slightly.

3. *The methodology is unclear, as the basic mechanics of how carbon pools are taken into account in the GCAM iterations are not explained in detail. Although often referred to, the reader has no chance to understand and reproduce these mechanics. A clear, structured introduction to the carbon mechanics within the GCAM model is required, as well as a clear overview of the possible land-use changes that can be imposed by GCAM (forest harvest, deforestation and transformation into crop land, crop change, etc.).*

We have added significant text describing GCAM, its carbon mechanics, possible land-use transitions (section 2.1) and the model more generally. (That said, GCAM’s algorithmic complexity is similar to that of an ESM, and inevitably we have to point the reader to other references for full detail.)

4. *A clearer discussion of the modelling/design choices is required. For example, stating that some proxies are considered "better" or "best" only makes sense after comparing them for clear predefined criteria.*

We have clarified in the text that our proxy assessment is based on which is least perturbed by LUC in CLM, while faithfully reflecting climate changes. This is what simulations S1-S4 and E1-E2 are designed to test.

Minor comments:

5. *Abstract P1500, 12-4: Quite strange wording which seems incorrect. Human activities do not pose a problem to ESMs as such. Many CMIP5 models were able to run the RCPs, which represent a set of widely varying societal forcing outcomes (in terms of well-mixed greenhouse gases, land-use patterns, and also - to a lesser degree - aerosols). Other variations are possible, but these would also not necessary pose fundamental modelling problems to ESMs. If at all, human activities pose a problem to the ESM modelers and users, rather than to the model itself. The "significant problem" lies at the integrated assessment modelling (IAMs) side. Scenarios are being developed based on simplified representations of the physical and chemical environment and at this point are not equipped yet to handle more complex earth system information. Changing biogeochemical cycles due to human activities is thus mainly a fundamental modelling problem for IAMs than for ESMs.*

We respectfully disagree. Human activities pose a fundamental challenge for ESMs because these activities (i) influence the climate and (ii) are not fixed in the future, but will respond to both climatic and policy factors. Yes, ESMs can accept static (unchanging) LUC or industrial emission scenarios such as the RCPs, but there's no integrated modeling of those emissions. Consider: if the ocean-atmosphere C exchange was specified *a priori*, even though we know it varies with climate, would we not be justified in saying that the lack of ocean modeling poses a fundamental challenge to ESMs? We believe the same logic applies in this case, given how decisively anthropogenic decisions affect climate.

6. *P1500, 115-18: It is unclear to me why LUC effects need to be short-term. To me this seems to be strongly scenario dependent and can thus not be ruled out from the beginning, particularly not with the limited amount of testing available in this manuscript.*

This sentence has been changed and now focuses only on the spatial aspect.

7. *P1500, 118-21: As indicated in major point b (see earlier), this sentence does not reflect what is achieved in this paper.*

This sentence has been slightly changed. Nonetheless, as noted above, we do think it's a pretty fair summary of the work.

8. *P1501, 16: Suggestion: also include a reference to a paper that presents RCPs or their CMIP5 outcomes results.*

We have added several references to papers examining CMIP5 results.

9. *P1501, 18-15: As my earlier comment on the first sentence of the abstract. This problem seems to rather affect the IAM side of this coupling, as currently IAMs generate scenarios that have potentially inconsistent evolutions of society and their environment. For the ESM, there is no problem to run alternative scenarios in which the environment is disturbed under varying policy assumptions, and the RCP experiments (and many other papers) are an illustration of this fact.*

Please see our response to #5 above.

10. *P1501, 119-20: This is an interesting point. While the spatial resolution argument is obviously correct when looking at simple climate models, it is unclear if the authors mean “process fidelity” or “process resolution” (or if they are used here as synonyms). Given that these aspects are provided here as limitations of current approaches, it would be necessary to include a discussion of how the “process fidelity” of the proxy choices and approximations described in this paper compares to incorporating emulations of the ESM instead.*

We actually do mean two different things here: “process fidelity” (the degree of biotic and abiotic process representation in a model) versus “spatial resolution” (how spatially resolved these processes are). It’s an excellent idea to discuss how these aspects are affected by our proxy choices—thank you—and we have done so in the new section 3.4.

11. *P1502, 18-10: The manuscript does not present a study that develops and tests a coupling of GCAM to CESM’s climate and biogeochemical cycles. As far as I understand, the coupling is far more modest: land carbon pool projection changes from the CLM model are fed into GCAM. This manuscript does not describe how GCAM would adjust crop choices to changing climatic conditions in terms of temperature, precipitation and soil moisture.*

That is correct, and we have clarified that the current ms only describes one part of the full ESM-IAM coupling. With respect to how GCAM adjusts crop choices, this would require an in-depth discussion of the economic optimization theory underpinning the model, and we feel that’s much more appropriately confined to the references for interested readers.

12. *P1503-1504, model description: please include a traceable technical description of how changes in carbon pools affect the behaviour of GCAM, either in the main text or in supplementary information.*

We have clarified the entire text, and now include (in the results, section 3.1) two equations specifying exactly how grid-cell level changes in CLM's carbon fluxes translate into changes in GCAM's equilibrium C assumptions.

13. P1504, 110-12: *GCAM simulates carbon emissions and sequestration from changes in land use between modelling simulation periods (P1503, 119). As emissions constraints are part of the possible policy scenarios that can be run with GCAM, I don't understand why LUC effects simulated by CLM, should not affect GCAM's decision-making. The few explanatory sentences following these lines do not necessary help a lot. Does GCAM decide when forests are harvested, or does GCAM decide that a forest is replaced by crop land? In the former case regrowth would cancel out the initial emissions over time, and thus doesn't pose a problem if it were correctly taken into account by GCAM. In the latter case, the effects are long-term and should thus affect GCAM's decision-making. If this is a design-choice in order to keep the CLM-GCAM coupling rather loose, this has to be clearly described. If this is a result of the analysis described in this manuscript, this should come up in the conclusions and not in this section.*

LUC emissions absolutely do affect GCAM's decision-making, but this sentence should have referred to equilibrium C assumptions. It has been corrected.

14. P1504, 121-22: *Without a clear description of the mechanics of the GCAM decision-making processes this is not obvious at all.*

“Obvious” only in the sense that GCAM cares about equilibrium C pools, and thus CLM C pools might provide useful information to change them. This sentence has been changed for clarity.

15. P1505, 11: *Maybe “applicability” or “appropriateness” instead of “feasibility”?*

We agree this was poor wording, and have changed it.

16. P1505, 19- and following: *Is there a particular reason why one doesn't start from a normal pre-industrial control run and then adds variations to them?*

This is in fact what was done, with simulation S1 as the control, and simulations S2-S4 as the single-factor experiments. The purpose of differencing the S2-S4 single-factor results from the S1 control is to remove the influence of interannual variability in the offline climate drivers on the model outputs, improving our ability to detect the single-factor forcing effects. A more detailed description has been added to this section, and a new Table 1 summarizes the simulations.

17. P1505, 122-26: *The end-of-century climate of RCP4.5 is much less perturbed than the RCP8.5 climate. What does this imply of the extendibility of this approach for high- forcing scenarios.*

Our results focused on the lower-forcing RCP4.5 scenario, because (i) the behavior of CESM under that GCAM-generated scenario has been examined closely as part of the CMIP5 exercise, (ii) RCP8.5 was generated by a different IAM and thus would require considerably more work to run, and thus (iii) our first exercise of the full iESM coupling is targeting this well-vetted scenario. It is true that we could gain additional information about the extendibility of our results to higher-forcing scenarios by repeating these experiments with RCP8.5 or other scenarios, and it is a good suggestion for further work in this area. The results here suggest that problems with our approach are more related to disturbance recovery dynamics in regions with low NPP than with non-linearity in scaling to higher-forcing climate, but additional testing is clearly needed in the future.

18. P1506, l19-22: *This would be better placed in the model description section.*

This text has been removed.

19. P1506, l24: *Define “environmental changes”.*

This has been reworded and “environmental” removed.

20. P1507, l23: *CLM’s (“s” lower case)*

Fixed.

21. P1509, l16-17: *This statement makes me wonder whether filtering CLM output in order for it to fit GCAM’s expectations instead of improving GCAM in order to deal with ESM’s noisy carbon pool data is really the best modelling way to go. This design choice, its strengths and weaknesses, need discussion in the manuscript.*

We now discuss why the use of fluxes did not cause this problem, and more importantly, the implications of this design choice later in the new section 3.4.

22. P1510, l20: *It is unclear which criterion has been used to define “best” here, based on the results discussed in the previous sections. When looking at Figure 2, it is not necessarily clear why one proxy is deemed better than others. This needs some clarification and discussion.*

We have clarified this in the text; please see the response to #4 above.

23. P1511, l7-8: *This sentence can serve as a basis for a new title, as it describes more precisely what was done.*

A very good suggestion—thank you.

24. P1511, 19-12: *The presented analysis did not clearly include a comparison/discussion of the robustness of the various proxies. I find this conclusion currently thus not supported by the provided information.*

Figure 2 together with the related methods text in section 2.3 and the results text in section 3.1 present the theory, hypotheses, and quantitative results from our model testing which support this statement in the conclusions. We believe that the reviewer will find it much easier to see this in the revised, clarified manuscript.

25. P1511, 117-24: *These two sentences appear rather repetitive to me.*

We agree; they have been condensed and changed.

26. P1514, 114-16: *The IPCC SAR was published in 1995, not in 2001. In case this is supposed to be IPCC TAR (published in 2001), page 20 contains part of the table of contents, and it is thus unclear to me what the page number is referring to. Page 20 of SAR contains the technical summary on tropospheric and stratospheric aerosols. Please provide a correct reference and a correct page range.*

We apologize for the mistake, and have removed this reference.

27. P1521, Figure 2: *Define all acronyms and use the same acronyms in caption and legend/labels. Please use same acronyms and scenario codes in the figure and text.*

All acronyms are defined in the caption and consistent with the text.

28. P1522, Figure 3: *Please use colors or make differences visible in some other way.*

Figure 3 is now color.