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Interactive comment on "Coupling earth system and integrated assessment models: the problem of steady state" *by* B. Bond-Lamberty et al.

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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, l2-4: 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, l15-18: 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, l18-21: 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, has noted above, we do think it’s a pretty fair summary of the work.

8. P1501, l6: 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, l8-15: 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, l19-20: 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 emula-

tions 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, l8-10: 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, l10-12: GCAM simulates carbon emissions and sequestration from changes in land use between modelling simulation periods (P1503, l19). 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, l21-22: 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, l1: Maybe "applicability" or "appropriateness" instead of "feasibility"?

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

16. P1505, l9- 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, l22-26: 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, l9-12: 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, l17-24: These two sentences appear rather repetitive to me.

We agree; they have been condensed and changed.

26. P1514, l14-16: 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.

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