Reply to reviews of Cook et al.

Review # 2

Comment #1: *Generally, the authors should be aware that "Amazon" refers to river only, not to the basin - for the basin, "Amazonia" or "Amazon basin" should be used. In my opinion the title is not appropriate, because the authors do not really calculate a likelihood.*

Response: Changed title per the comment. The text has also been updated to refer to "Amazonia" instead of "Amazon" throughout the paper.

Comment #2: *p. 64, l. 2: Some recent climate modeling results suggested a possible dieback. The dieback was only found by the Hadley model and this was coupled to a dynamic vegetation model. Please reformulate.*

Response: We updated the text to note that this was just one study that had such an extreme result and not several studies as the original text implied.

Comment #3: *p. 64, l. 5 and p. 68, l. 10: The IPCC-AR4 provides 24 climate model projections. Why did you use only 15? According to which criteria were these 15 models selected? How would the model median change if you would use the 24 models instead of 15.*

Response: We chose to use the better known models. The remainders are not widely used. In particular, some models have never been scrutinized or validated for the region of interest in their climatology and variability. In retrospect, it would probably be more convincing to use all IPCC models. Our results are corroborated by other analysis, e.g., Malhi et al. (2008) who used 23 of the 24 models, and the broad features are similar to the 15 models we analyzed (Compare their Fig 1 with our Fig. 2).We now discuss this in more detail in the revised manuscript.

Comment #4: *p. 65, l. 4: The Amazon forest was replaced by C4-grasses in the study of Cox et al.*

Response: We added text to explain that the forest was replaced by savanna or C4-grasses in the references we list.

Comment #5: *p. 65, l. 14-15: The studies presented by Cowling and Shin (2006) and Schaphoff (2006) are not really similar to the Cox-study. They didn't use a coupled climate/vegetation model. Please reformulate.*

Response: We added text that "although these studies did not use coupled climate-vegetation model, thus not including presumably positive vegetation feedback."

Comment #6: *p. 66, l. 4: The "Amazon" is defined as the Amazon river. I think the authors are correct that it is important to account for regional precipitation patterns and that these patterns vary strongly within the Amazon basin. But the definition of what the Amazon basin is, is rather not a factor that complicates the issue about Amazon forest dieback. Please reformulate.*

Comment #7: *p. 66, l. 5: Sentence not clear, please correct.*

Response: We appreciate this comment and were sorry for the unclear statements. The new text reads:

Another factor that has lead to confusion is that the "Amazon basin" is not defined in the same way across all studies. Process-level mechanisms suitable for one part of the "Amazon basin" are sometimes applied to other parts of the basin without sufficient caution. Several different factors influence precipitation patterns in different parts of Amazonia, and it important to note these regional differences.

Comment #8: *p. 68, l. 1ff: Did you use daily data for the analysis?*

Response: We used monthly output from the GCMs, a common resolution for analyzing climate model projections. The monthly data is interpolated to daily for driving the vegetation model. This information is now added in the revised text (section 2 and appendix).

Comment #9: *p. 69, l. 18: The information about the VEGAS model should be given here and not in the Appendix.*

Response: We gave careful thoughts to this comment and saw both advantages and disadvantages of bringing the model description into the main text. At the end, we decided to leave it in the appendix because the VEGAS forced simulation is a relatively small part of the paper, and the paper flows better this way.

Comment #10: *p. 70, l. 1ff: Wouldn't it make more sense to analyze the change in days with a precipitation lower than 1 mm per day instead of the precipitation during May-September? Then you would also account for shifts in the dry/wet season under future conditions instead of analyzing the precipitation only for these particular three month.*

Response: These are two different ways of presenting the results, with the first more widely used. Because the climate model output is monthly, it seems to make more sense to minimize interpolated daily data (For the same reason, the finding of 11-day lengthening of the dry season has large uncertainty; also see response to Reviewer 1's Comment 6). In practice, they usually point to same conclusion as is the case here. According to figure 6a, the only months that have average rainfall below one millimeter per day are May-September. We essentially did both as Fig. 6 shows the delay while other parts show wet/dry season changes.

Comment #11: *p. 70, l. 22 and Figure 6: This finding is not clear from Fig.6. Additionally, Fig. 6 has a very bad quality, the legend and the y-axis label are missing.*

Response: The 11 day delay was found using a high-resolution hardcopy of the figure. The finding of 11-day lengthening of the dry season has large uncertainty (also see response to Reviewer 1's Comment 6) so that we added 'approximately 11 days' in the revised manuscript. The version of Figure 6 displayed here is an image file. We plan to work with ESD technical staff to see if we can incorporate the postscript file after the paper is accepted. We now also explain the x/y labels in the caption.

Comment #12: *p. 71, l. 21 and p. 77, l. 24/25: This is not clear. Shouldn't it be wet season precipitation that recharges soil moisture? How do you support this finding?*

Response: Our use of the term "recharge" was unclear. We have changed the text to indicate that a greater fraction of dry-season rainfall is absorbed and used by the ecosystem, than wet-season rainfall, a large portion of which is lost as runoff.

Comment #13: *p. 74, l. 17: If this is a robust signal in the IPCC-AR4 models, why do they then differ so widely in their projections of rainfall in the Amazon region?*

Response: It is robust for the dry season SAB, when SAB is part of the subtropical dry zone. They differ widely in the wet season (and total annual rainfall, dominated by wet season).

Comment #14: *p. 74, l. 21-24: This paragraph is not clear. Please reformulate. Please explain the symbols or remove.*

Response: We have updated the text and now explain the mathematical symbols for divergence. The detailed thermodynamic argument is beyond the scope here but is discussed in the cited references, in particular, Neelin et al. (2006) and Held and Soden (2006).

Comment #15: *p. 77, l. 2: The fact that CO2-fertilization is not included in the model should already be mentioned in the model description!*

Response: We now mention this in 'Data and Methodology' section, as suggested.

Comment #16: *p. 77, l. 28: Also Phillips et al. 2009 (Science) show very interesting results for the 2005 drought*

Response: We added text and the reference. Thank you!

Comment #17: *Appendix A: The Appendix is not really necessary. The paragraphs of the Appendix should be moved to the according Methods section.*

Response: We have chosen to leave it in the appendix as discussed above (Comment #9).

Comment #18: *- p. 79, l. 15: Is photosynthesis simulated explicitely? Please describe PS in more detail. The authors mention that the model does not account for CO2-effects, please state here and explain.*

- How does the precipitation change during the dry season affect vegetation in the model?

- *Is the model run in a daily or monthly mode?*

- *Please describe the fire simulation in more detail.*

Response: We have added explanatory text to the model description in the appendix.

Photosynthesis is simulated explicitly as a function of light, soil moisture, temperature and CO2. The CO2 effect was turned off because of the high uncertainly, with uncertainty discussed in Discussion, and now also acknowledge in the abstract. It would be nice to be able to answer the question on CO2 fertilization. Our work is not meant to be the most comprehensive study of the problem. We instead focused on the climatic impact, especially the dynamical mechanisms, which have not been clearly identified previously. We show that the models have relatively robust behavior, backed by the mechanistic understanding. To mix in the highly uncertain CO2 fertilization issue would compound the relatively clear dynamic mechanisms. We therefore chose to set a limit on our scope, while acknowledging this uncertainty. In the revised manuscript, we add further caveat along this line both in the discussion and the abstract.

Precipitation change influences soil moisture, modeled by a physical land surface model SLand that is coupled to the vegetation model. Fire depends on moisture, fuel load (live and dead) and temperature. The VEGAS model was run daily, forced by climate data interpolated from monthly.

Comment #19: *- p. 79, l. 25 "contribution to interannual CO2 variability . Not clear what is meant by this, please reformulate.*

Response: CO2 emissions due to fire is an important contributor to interannual CO2 variability, as discussed in Zeng et al. (2005a).

Comment #20: *- p. 79, l. 29: "coupled"? From the model description I understand that it is an uncoupled vegetation model. Please clarify.*

Response: The VEGAS model is normally part of a fully coupled Earth system model, though here it is run 'off-line'. We now deleted 'atmosphere' to be consistent. The VEGAS is the dynamic vegetation and terrestrial carbon cycle module. It is coupled to the physical land surface model SLand which exchanges heat and water fluxes with the atmosphere. SLand provides soil moisture to VEGAS and interact with VEGAS through evapotranspiration-stomatal resistance interaction. We now clarified these in the revised text.

Comment #21: *- p. 80, l. 7: ": : :has been validated: : :" Were the results of the validation good?*

Response: Reasonably good, in particular on interannual time scales, as in references cited.

Comment #22: *Table 1: If you calculate these kind of probabilities, it would be really important to use the whole model ensemble consisting of 24 models.*

Response: Please see above rationale for choosing the more commonly used models.

Comment #23: *Figure 1: It would be good to have the abbreviations for the regions in the figure. The x- and y-axis labels are missing.*

Response: Thank you for the suggestion. We now added the region abbreviations. We added the description of Jan-Dec in the caption, but not in the figure to avoid cluttering.

Comment #24: *Figure 8: Here you show the median annual rainfall which is increasing. Why don't you show the wet and dry season rainfall?*

Response: Dry season rainfall is also shown (in light blue). Wet season rainfall is not shown because it is similar to the annual rainfall (clarification added in the revised caption). It can also be seen spatially in 2b and graphically in figures 5 and 6.

Comment #25: *Figure 9: a) and b) How can fire flux and LAI be negative? Figure legend is hard to read.*

Response: These values can be negative because the display indicates the change in these variables from the 20th century to the 21st century expressed as a difference. A negative value means this quantity was greater in the 20th century than the 21st. We will provide high-quality ps file for the final production. We now also describe the legend in the caption.