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Biogeosciences Discussions

Interactive comment on "Inferring past land-use induced changes in surface albedo from satellite observations: a useful tool to evaluate model simulations" *by* J. P. Boisier et al.

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Final authors' comments and responses to referees on "Inferring past land-use induced changes in surface albedo from satellite observations: a useful tool to evaluate model simulations"

My co-authors and myself would like to thank the two reviewers for the positive and constructive comments that were made to our manuscript. We will try to account for all of them in a revised version of the manuscript after the publication of this response.

In general terms, referees agree in that the method part of the manuscript needs more clarity and further information. This suggestion will be thoroughly addressed in the

revised manuscript.

Referee #2 gave a number of specific comments. Our detailed response to them can be found below.

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1) In general, the readers would benefit from an elaboration on the LUCID experimental design. Particularly, the construction of vegetation maps and the fact that only land use changes (ie. non-natural vegetation) were implemented needs to be made clearer.

R: Yes, in the current manuscript the reader is invited to revise the published papers of LUCID to well understand the modeling experiment. A better explanation of the LUCID set of simulations, including a more informative Table 1 will be given.

2) Related to above, the methods section is often terse and confusing. Please more carefully describe how the reconstruction of albedos was performed. Additionally, how was the observed / FH calculated? Was the FH term derived from an average of all the models' FH's, or was it directly from the LUCID vegetation maps? If so, what decisions on land use transitions were made?

R: For the albedo reconstructions associated to the various LUCID models, the change in the fraction of herbaceous vegetation (FH) was calculated with the corresponding (LSM) land-cover maps. This analysis is actually not explained in the methods part and will be clarified in the revised version.

3) MODIS albedo has a known bias in high latitudes in winter, when zenith angles are particularly large (see Wang & Zender 2010, 10.1029/2009JF001436). The authors need to address and devise a method to deal with this bias, as these values can cause misrepresentative seasonal albedo patterns.

R: The MODIS snow albedo bias at large zenith angles reported by Wand and Zender (2010) was actually not taken into account in our study. However, our analysis - although performed globally- focuses on regions with changes in land cover from 1870

BGD

9, C7301–C7310, 2013

to 1992, particularly on the temperate regions of the Northern Hemisphere. Hence, few areas from those assessed encompassing high latitudes (> 55°N) are potentially affected with this problem. To correct this bias is beyond the scope of this study, but a proper comment on this issue will be included in the revised version.

4) A large underlying factor for the different model representations presented here is the way in which modeling groups implemented the different vegetation maps, and how this results in substantially different deforestation rates. The authors should give some text as to how the implementations and deforestation rates are so different, as it's not intuitive to readers unless they've read the other LUCID papers. Are there opinions on which implementations are more realistic?

R: The large dependency of the strength of land-use changes (e.g. deforestation) to the way the agricultural data is incorporated into LSMs is a major alert message we pointed out from LUCID studies, as discussed in de Noblet et al. (J Climate, 2012). The contribution to the model uncertainties associated to the land surface forcing is quantified in Boisier et al. (JGR, 2012). It is not a simple task to manage this problem in LSMs since the strategies adopted to incorporate land-use depend on the structure of the models as well as on their natural vegetation maps. Given this uncertainty, we choose in this study to compare several models and their associated vegetation maps, and filter this signal to highlight the intrinsic model albedo sensitivities (parameterization) to land-use changes (e.g., Figure 7). We agree that this point is very important and deserves a better explanation in the revised manuscript.

5) Related to above, another potential source of uncertainty involves the translation/aggregation from individual modeling groups' Plant Functional Types into the 5 land cover types used here. Please address.

R: In general the models' PFTs do not include biome classes with mixed vegetation (e.g. savanna), so the grouping into the five classes assessed is relatively straightforward. Hence, the uncertainties in the partitioning of the five vegetation groups come

BGD

9, C7301–C7310, 2013

principally from differences in the model background maps (e.g., grasses vs. trees) and the strategies adopted to incorporated land-use, as discussed above.

6) Why did the authors choose to use black sky instead of white sky albedo? Albedo within the land surface models should be calculated as a weighted average of black and white sky.

R: Yes. It would be more proper to calculate the surface albedo using both black- and white-sky components, for which a global distribution of direct/diffuse radiation would be needed. We choose to use only the directional hemispherical component for simplicity, as we think that the final conclusions wouldn't change significantly (rather than net value, we are assessing anomalies that quantify differences between land-cover classes). In addition, a weighed albedo calculation wouldn't be more adequate as benchmark to models since not all the LSMs assessed here include direct and diffuse radiation in their surface albedo calculations. This point is also important and our assumptions will be included more explicitly.

7) It would be nice to have some sort of figure, map or otherwise, showing the distance of interpolation needed for grid-cells in the reconstructed albedo maps.

R: The aim of Figure 3 is to quantify the error associated to the interpolation method. Figure 2a shows the distribution of the cells with dominant vegetation per land-cover group that gives an idea of the distances involved in the interpolation. We will evaluate including another metric to evaluate the method.

8) From Figure 8, it seems that some models have significant temperature responses, and that this would change snow distributions between the two time periods. I'd like to see some indication of how influenced the simulations (changing snow cover distributions).

R: The temperature anomalies shown in Figure 8 are normalized, i.e., they give an estimate of the response after total deforestation. The simulated temperature responses

BGD

9, C7301–C7310, 2013

to land use changes are weaker but still important regionally (de Noblet et al., 2012). Therefore, some models show areas with significant changes in snow cover. However, Boisier et al. (2012) showed that this effect does not contribute significantly to the simulated albedo changes. Further, those grid-cells showing large changes in snow cover were excluded in analyses, such as the one showed in Figure 7, in order to not induce misrepresentations.

9) More discussion on uncertainties would be helpful. Sources include the use of only 5 land cover classes, distances for grid-cell interpolation, choice of snow cover datasets, albedo products, the use of the same climatic and snow conditions for pre-industrial and present day, inconsistencies between PFT/LC aggregations, implementations of land cover changes, etc.

R: Agreed.

10) In the discussion, globally-averaged forcings are given. However, these albedo changes are highly regionalized, and will affect those regions strongest. This could be made clearer.

R: Ok.

11) In the discussion, the authors could suggest a further study that quantifies the contribution of natural vegetation changes to climate

R. Ok.

12) Can the authors offer any hypotheses on why different models parameterize albedo within their PFTs so differently, as this is identified as the major source of inter-model discrepencies?

R: The differences should reside in two aspect: the LSMs' albedo parameters (e.g. leaf/steam albedo, snow age dependencies, etc) and how the plant's biophysics is upscalled to the model's grid scale. We will try to discuss and address this more thoroughly. A table comparing the LUCID models in some basic parameters will help

BGD

9, C7301–C7310, 2013

to clarify this and enrich the discussion.

13) In Figure 8c, the intersection of the dashed and dotted line is quite interesting, as it shows the expected temperature response (as predicted by the models) per change in surface albedo (as predicted by MODIS) within the analyzed box. The authors should consider discussing this result.

R. Agreed

14) It would be nice somewhere to be able to compare the forcings from LCC-induced albedo changes to the biogeochemical (released GHGs mainly). Is there a reference for this?

R: This study focuses on the biogeophysical effects of land-use change, and only on one of them (albedo). Hence, we think that the biogeochemical impacts of land-use exceed a bit the paper's context, but anyway it should deserves a comment in the introduction.

15) The authors mention Myhre et al. 2005 in several places, but do not distinguish their study from this or explain why it's novel or a useful extension.

R: Agreed. This study proposes a novel technique that maps geographically-dependent albedo for different vegetation groups. A proper comment regarding this and comparison to e.g. Myhre et al. will be included in the introduction.

Technical Corrections/Comments

[pg 12506, line 19] consider also stating that the models show differences among themselves,ie. there are not many overarching consistent biases

R: Ok.

[pg 12508, lines 2-3] for which time period are the observations? Just saying "modern day"would help clarify.

BGD

9, C7301–C7310, 2013

R: Agreed

[pg 12508, first paragraph] Are the natural vegetation maps the same between the two sets of simulations? From the LUCID experiments, I think so, but they're different between modeling groups. This needs to be made clear to the reader here in the methods.

R: Agreed

[pg 12508, line 8] what are these dominating non-radiative effects? Elaborate a little please.

R: This sentence refers to changes in turbulent heat fluxes. It will be made explicit in the new version.

[pg 12508, line 18] This first sentence is confusing. What do you mean by "difficult to disclose one of the LUCID vegetation' reconstructions"? I'm guessing the authors mean that it is difficult to validate the vegetation reconstructions in general.

R: Yes. As commented above, the problem of implementing "realistic" land-use change in climate models as the LUCID initiative showed, will be more clearly discussed in the revised version.

[pg 12508, line 27] I would imagine the height/LAI parameters of crops matters a lot for albedo as well.

R: Of course. In this sentence we use the term "parameterization" in a broad sense (as how models represent the biophysics of the land-surface), including LAI.

[pg 12509, line 9] change to "LUCID models' albedo sensitivities", as I assume you mean multiple models as indicated by "their"

R. Ok

[pg 12509, line 20] State that the "vegetation" maps only change in crop/pasture dis-

BGD

9, C7301–C7310, 2013

tribution, ie. natural vegetation is the same. Also, how were these vegetation maps derived?

R. This section will be complemented with more information on the LUCID experiments, notably on the models' land-cover maps.

[pg 12510, line 1] again, distinguish between land use and natural vegetation. It's confusing as written.

R: Ok

[pg 12510, line 13] These first sentences are confusing. What does "global interplation" mean here?

R: It refers to an albedo upscaling (globally), by means of spatial interpolation of grid-cells with dominant vegetation.

[pg 12510, line 25] Please describe this technique more clearly. Do you mean that the albedos of grid-cells that had less than 95% of one dominant LCG were derived using interpolation of the pure grid-cells? What exactly is the "spatially nearest value method"?

R: Yes. The "nearest value" method is the simplest way to interpolate spatially, by filling up the albedo in a given coordinate with nearest available value (the available cells are in this case those with dominant vegetation) .

[pg 12510, line 27] I would suggest not saying "degraded", as this has a strong negative connotation. Instead maybe say something like "spatially aggregated", or "upscaled".

R: Agreed

[pg 12511, line 6] change to "We used snow cover data from NISDC instead of MODIS because: : :"

R: Ok

BGD

9, C7301–C7310, 2013

[pg 12511, line 20 - 22] Again, it would be good to give the reader some idea of what these different implementations are, and how they can arrive at such different deforestation estimates from the same LC maps. It's unintuitive if one hasn't read the other LUCID papers.

R. Agreed

[pg 12514, lines 19 - 20] It's unclear what you mean here by "is calculated from the ensemble of grid-cells within the selected region"

R. It means that the MAE considers only the grid-cells inside the box defined in the northern temperate region. To reformulate.

[pg 12517, line 10] Do you mean "LUCID models' snow cover and albedo sensitivities to LCC"? I assume you mean the various models (plural) used in the comparison

R: Correct.

[pg 12519, last paragraph] Please elaborate more on what this means. I believe it relates to the fact that a high snow bias will, on top of the normalized albedo response, bias the models even higher because the starting point for snow is higher, ie. Nonlinearity. Please explain this further if this paragraph is to be included; it's a confusing to the reader as is, and is a relatively minor point.

R. Ok

[pg12523, line 4] Radiative forcing is not the same as surface forcing. The two are comparable, but this needs to be made clear to the reader.

R. Agreed

[Table 1] Spell out "SIC", "PD", and "PI" in last column. Include version numbers in the model footnote.

R. Ok

BGD

9, C7301–C7310, 2013

[Table 4] Include an explanation of what alpha-driven means here, so the table can stand by itself.

R. Ok

[Figure 4] Eliminate "[ ]" from y-axis. “Reconstructed” spelled incorrectly.

R. Ok

[Figure 6] There must have been variation in modeled albedo between years and ensembles. Can you make error bars for the solid lines as well?

R: Yes

[Figure 7] An intermediate panel showing albedo vs. SWE would help interpretation.

R: Agreed

[Figure 8b] Was the averaging done across all snow levels?

R: Yes

Interactive comment on Biogeosciences Discuss., 9, 12505, 2012.