First of all we would like to thank the three reviewers for their time and effort they spent to review our manuscript. It is certainly possible to improve the manuscript based on the detailed and constructive comments.

The first two reviewers are supportive of the paper, acknowledge its importance and we are able to revise the manuscript in such a way that all comments and requests are met. The comments of reviewer #3 have a different nature. We are able to address the more specific comments of reviewer #3, but disagree with the overall comment of this reviewer on lack of relevance of the paper.

Reviewer #1 suggests a reframing of the manuscript from our 'three challenges' and paying more attention to the distinction between land use and land cover or their separate research history, respectively. These are valid points which will help us to improve the manuscript. We intend to resolve this by more clearly describing the framing of the paper. For example, there is room for misinterpretation of the structure in the original manuscript: while we intended to present one important challenge ('main issue') per section, the reviewer identifies the three main challenges already in section 2 (*Provision of spatially explicit, continuous and consistent time series of LULCC*) and thus the other two sections seem to be disconnected. We therefore suggest to include a guiding paragraph at the end of the introduction to clarify our underlying structure as following:

'Each of the following sections thereby presents one of the three challenges we identify to be crucial in future land use – climate interaction research, reviews the issue and its implications for the results of climate change assessments based on previously published literature and in the context of the widely applied land use harmonization (LUH) dataset by Hurtt et al. (2011). Moreover, in sections 3 and 4 we perform additional analysis using data from the CLUMondo land-use change model (Van Asselen and Verburg, 2013) to illustrate our arguments. Section 5 synthesizes the three challenges and provides recommendations to move forward in land use – climate interaction studies.'

Other revisions in line with the comments of this reviewer are outlined in detail below.

Reviewer #2 comments broadly concentrate around hiddenness of the 'ways forward' in the individual sections. We propose to address this in a revised version following the reviewer's suggestions and will include sub-headings, re-organize the (sub-)sections towards a common structure and move the 'ways forward' to a separate section and also slightly expanding on these.

Reviewer #3 concludes that we do not provide 'new insights, synthesis or analysis' and suggests a reframing of our manuscript towards the 'development of new diagnostics for evaluation of global LULCC reconstructions or models'. We do not share this view. The current manuscript is a review, supported by data (e.g., CORINE and NLCD land cover data in section 4) and new analysis (e.g., allocation of managed land as shown by CLUMondo in section 4), of the most dominant issues related to land use and climate modeling. The manuscript brings together, to our knowledge for the first time, prominent challenges in land use – climate interaction studies. This view is shared by the other reviewers. The purpose of the paper is to review the current problems and progress, and identifying possible ways forward for the community. We do agree with the reviewer's assessment that identifying more conclusively the ways forward to overcome existing stumbling blocks will add to the value of the manuscript and will revise the paper to strengthen that aspect.

For a detailed list of how we are aiming to address the individual reviewer's comments and improve the manuscript, please see our following responses inline in blue.

Reviewer #1 (A. Di Vittorio)

The authors examine current practices of providing LULCC data and simulating LULCC effects on the earth system in global models and identify 3 main issues related to reliable provision and use of LULCC data. They then move on to the limitations in data for gross land use/cover transitions, and finally discuss land conversion assumptions in modeling as a source of uncertainty. They also make 3 suggestions on how to improve the provision and use of LULCC data.

I appreciate this paper and am pleased that it discusses relatively overlooked, yet very important, issues regarding LULCC and global modeling. I generally agree with the assessment, but think that the paper needs some reorganization and some additional discussion to fully and clearly make its case. The main issues requiring attention are summarized here, with specific comments/suggestions following:

Response: We thank the reviewer for the kind words and the appreciation of our work. We are pleased that the reviewer generally agrees with our assessment. We will revise the paper to provide a more clear structure as we feel the current framing provided some misunderstanding: we did not aim to first identify the three main issues and then 'move on to limitations in data for gross land use/cover transitions, and finally discuss land conversion assumptions in modeling', but the three main issues are intended to be the three sections following the introduction, namely (1) *Provision of a consistent LULCC time series*, (2) *Consideration of gross transitions*, and (3) *Allocation of managed land in DGVMs/ESMs*. We will refer to this and explain more in the responses to following comments.

1) The paper needs a consistent framing and argument. The three main issues are different between the abstract, text and conclusion. It appears (see abstract lines 28-34 and page 3 lines 5-10) that the point is to show that the 3 main issues are indeed main issues, based on literature, an example, and discussion of two underlying factors (1) gross transitions and 2) land use change to land cover change translation), and provide suggestions for moving forward. But the main issues are not referred to in the later sections, and these two aspects are not introduced up front so that they can be discussed in the context of the main issues. And the suggestions are not related to the issues.

Response: Based on the assumptions the reviewer made regarding the three main issues and the underlying factors, we entirely agree that there is a lack of consistency and disconnection throughout the manuscript. We propose to add a paragraph at the end of the introduction that will clarify the structure (please see the general response for a suggestion). We will also remove the confusing sentences that give the impression that we present the 'main issues' in the first section (after the introduction) and discussing 'underlying factors' subsequently. A revised manuscript will make sure that each section (2, 3, 4) treats one of the challenges outlined in the abstract and picked up in the recommendation section individually, connected by the overarching topic of 'shortcomings/issues in land use – climate interaction studies'.

In conjunction with the changes outlined later on in this document regarding structure requested by reviewer #2, we aim to address the concern raised.

2) It isn't clear that lack of information on gross transitions is a fundamental factor for the 3 main issues. While there is a lot of uncertainty in estimating gross transitions, and there is need to improve related data, this seems more like an example of a more fundamental driver. One thing that cuts through the three issues and incorporates gross transitions is data quality. In fact, that is largely what issue 2 in the

text describes. And ultimately issue 3 as well (initial, present-day data sets for future projections). Maybe there are only two main issues (single historical product with no uncertainty and uncharacterized/large model uncertainty in future land projections) and two underlying factors (data quality and independent land use and land cover implementation). Then the underlying factors provide guidance for the two communities to work together to address the two issues as they apply to both the human and dgv/es models.

Response: We will rephrase the section in such a way that we no longer present the lack of information on gross transitions as a fundamental factor for other issues/challenges. We will make more clear that gross transitions are a challenge in itself, since these additional changes are hardly considered in climate models, but may have a substantial impact, e.g. on the resulting carbon signal. Indeed, it closely links to both of the other challenges, to the harmonization (section 2) in terms of data quality and to the allocation (section 4) in terms of how the DGVMs/ESMs are able to implement the transitions. However, the reframing suggested by the reviewer would make the gross transitions to one out of several 'data quality' issues, which we feel is not appropriate. The topic of gross transitions goes beyond data quality issues due to their scale dependency, i.e. even if we would be able to reduce uncertainties due to technical restrictions (e.g., in remote sensing products) or model assumptions, sub-grid processes would need to be addressed separately. Similarly, the suggested reframing would exclude the uncertainty from present-day land-use (and land-cover) products which different land-use model intercomparison exercises have identified as an important source of variation across land-use models (Alexander et al., 2016; Schmitz et al., 2014). We however agree that challenge 1 (*Provision of consistent time series*) is mainly related to data quality, which we will emphasize in a revised version of the manuscript.

3) The underlying factor of the traditional separation of land use research from land cover research is not addressed until section 4, even though it cuts through the main issues and there is also a main point in the conclusion that land use modeling needs to be integrated with land cover/ecosystem modeling. And one of the suggestions calls for specific land use to land cover conversion information in place of just land use information. You also use land cover products for figure 5, which are not necessarily consistent with agricultural land use data. Furthermore, this separation is not explicitly discussed, with LULCC being a whole throughout the text, even when discussing how each land model has to make land cover conversion assumptions to accommodate independent land use data. You mainly focus on the land cover conversion uncertainty, but the separation of land use and land cover is the underlying source. There is some additional literature addressing this specific issue that would be useful to the authors. I would also be happy to discuss this further with the authors, as I am trying to finish a manuscript looking at how land cover conversion uncertainty affects carbon and climate projections. Look me up if you are coming to AGU in San Francisco this year.

Response: The reviewer has a valid point that we did not carefully separate between land use and land cover throughout the manuscript and especially in section 4. We will check again and change accordingly. As mentioned later on in the response to reviewer #2, we will additionally clarify the structure such that a recommendation section will focus on the short term improvements (i.e., how to tackle the individual issues within the current coupling) and an outlook section will discuss the model integration. We do not necessarily agree that the separation of land use from land cover research 'cuts through the main issues', but it rather applies to our issue 3 (*Allocation of managed land in ESMs and* DGVMs). In addition to this, we discuss the issues arising in the data used for the current coupling in section 2. We will clarify this in a revised manuscript as outlined in the response to the reviewers first general comment.

Specific comments and suggestions:

Abstract

page 1, lines 31-32: this subgrid and gross transition is not on page 5 as a main source of uncertainty. The second main source in the text is inconsistencies of present day data. You do later discuss transitions, and make a statement in the conclusion, however.

Response: See response to this reviewers main comments. The revised manuscript will ensure consistency.

page 2, lines 1-2: I think I know who you mean (providers and users), but it is unclear who is included in the "joint development and evaluation" here.

Response: Thanks for pointing to that. We will rephrase this to clarify as following:

'We propose that LULCC data-provider and –user communities should engage on the joint development and evaluation of enhanced LULCC time series, […]'

page 2, line 12: What do you include as a DGVM here? Some consider any model having vegetation growth in response to environmental conditions as a DGVM. For others a DGVM specifically includes prognostic biogeography (i.e., the extent of vegetation types change according to environmental conditions) and/or successional vegetation processes (e.g., stages of forest stand growth following a forest clearing disturbance).

Response: We agree with the reviewer that the DGVM terminology is misleading in our manuscript. We suggest to replace it with the term *'terrestrial biosphere model (TBM)',* which covers models with both static and dynamic vegetation. Additionally, we will add a short explanation that will make clear that this term is meant to be inclusive of a range of modeling approaches, as basically any terrestrial biosphere model that is able to include land use from an external data set will face the challenges discussed in our manuscript.

page 2, line 26: you should also cite: Meiyappan and Jain, 2012. Three distinct global estimates of historical land-cover change and land-use conversions for over 200 years, Frontiers in Earth Science, 6(2):122-139 (I noticed you cite it later).

Response: Good point. We will include the reference.

page 2, lines 29-30: you should also cite: Di Vittorio et al., 2014. From land use to land cover: restoring the afforestation signal in a coupled integrated assessment-earth system model and the implications for CMIP5 RCP simulations, Biogeosciences, 11, 6435-6450.

Response: We apologize that we have not been aware of this work during the preparation of the manuscript. We will include the reference.

Provision of LULCC

page 4, lines 11-13: The CMIP5 product harmonizes only land use, and as such the land cover (forest, grass, etc.) and how it is altered by land use is determined independently by the DGVMs/ESMs, and can be dramatically different between models for a given scenario (in fact, prescribed scenarios can be substantially altered in ESMs by this, see Di Vittorio paper listed above). The CMIP6 product is also including forest cover in the harmonization, both for the historical period (with reference to satellite data) and for the IAM scenarios (which actually project all land cover).

Response: We agree that the term LULCC is not appropriate in this context. We propose to improve this paragraph by exchanging the term 'LULCC' by 'land use'. Furthermore, we suggest to replace the sentence

'This strategy tries to conserve the original patterns, rate and location of change as much as possible, and to reduce the differences between the models due to definition of land-use categories (e.g., what constitutes a forest).'

by

'This strategy tries to conserve the original patterns, rate and location of change as much as possible, and to reduce the differences between the models due to inconsistent definitions of cropland, pasture and wood harvest.'

page 4, lines 14-15: Only land use is input to and output from GLM for CMIP5, and forest cover is included for CMIP6.

Response: We will revise as mentioned in the response to the previous comment.

page 5, line 12: It isn't clear here that you have shifted away from the harmonization group of models to a more general group providing present-day lulc data for future projections.

Response: It is not really a shift from the harmonization group of models to a more general group, but we are not aware of work that has compared the differences in the starting maps of the IAMs contributing to the CMIP5 harmonization only. The cited Prestele et al. (2016) paper indeed includes additional (non-IA) models, but still three out of four models providing data to the CMIP5 harmonization. These do not agree about initial areas as well. We will clarify as following:

'This uncertainty, however, is represented also in the starting maps of the different land-use change models providing land-use data to climate models, including the IAMs providing data for the harmonization.'

pages 5-6, lines 24-5: Two points here: 1) In the IPCC context, only land use was used, with forest cover coming into play for CMIP6, even though the IAMs project land types for the entire terrestrial surface. This introduces uncertainty beyond just the model structure/assumptions and different input data (see the Di Vittorio paper listed above). 2) The starting point of lulcc determination isn't just about which land-use input data or how processes are implemented. The spatial configuration of these data and the model are key factors in determining model outcomes. And each model has a unique spatial configuration. Gridded models/data do not necessarily resolve this spatial issue because regional values are often just resampled to the grid. See: Di Vittorio et al, 2016. What are the effects of Agro-Ecological Zones and land use region boundaries on land resource projection using the Global Change Assessment Model, Environmental Modelling and Software, 85:246-265

Response:

1) We agree with the reviewer about the missing land cover information being a major constraint in the CMIP5 LUH product and propose to clarify and expand the discussion about the missing land cover information in CMIP5 LUH in the section *Allocation of managed land in ESMs and DGVMs*. We do not

think that the discussion about uncertainty emerging from the fact, that only land use is provided to DGVMs/ESMs and they utilize different implementation schemes on their background land cover, would fit in section 2 where we discuss the uncertainties arising during land-use modeling and the harmonization.

2) We agree that the spatial configuration of the individual models even increases the uncertainty range and appreciate the reference to the Di Vittorio et al. (2016) paper. We propose to include this additional uncertainty issue by rephrasing page 5, lines 26-28 to:

'Land-use change model intercomparisons and sensitivity studies, however, indicate that the uncertainty range emerging from different assumptions in the models, input data and spatial configuration substantially impacts the model results (Alexander et al., 2016; Schmitz et al., 2014; Di Vittorio et al., 2016).'

page 6, line 2: How were the variables normalized? Could the dominance of initial pasture area be due to it just being the largest difference in relation to the other variables? Also, it would be more clear if you were specific in the text and the caption in describing that the "starting point" and "initial" are the pasture area in relation to fao in 2010, and that "model" is actually model type and presumably the spatial resolution/configuration.

Response: We will include a clear mentioning how the data were processed. The areas are indeed in absolute terms, which indeed makes it likely to have larger deviations. However, in the text we will describe the range relative to the average value in more detail to address this point and indicate that the larger variation cannot be explained by the larger initial area. We will improve the caption as suggested.

page 6, lines 6-14: I completely agree! While recent feedback on LUMIP has prompted the provision of LU-forest uncertainty along with the CMIP6 LUH product, it still falls short of the comprehensive approach discussed here.

Response: We thank the reviewer and also appreciate initial consideration of uncertainty along with LULCC data provided to LUMIP, e.g. that additional high and low estimates of historical land use will be included (Lawrence et al., 2016). However, we think a more ambitious approach should be taken into consideration for future MIPs.

page 6, lines 15-22: The separation of land use and land cover is a critical factor omitted from this discussion. While land use and land cover are often said in the same breath and the LULC(C) acronym is widely used, in nearly all cases people are referring to either land use or land cover. Research is clearly split along these lines, and land use data are remarkably inconsistent with land cover data. Land use and land cover need to be studied together, as an integrated process, in order to reduce LULCC uncertainties and inconsistencies between these two groups of data.

Response: We agree with the reviewer that there is a large discrepancy between land use and land cover research and data products and that this will be an important challenge to overcome in future by integrating the research lines. In fact, model integration is one of the key points of our conclusions. We will highlight it more clearly in a revised manuscript by rephrasing our three conclusion points in a 'recommendation' section (for the more short-term improvement potential) and discussing the model integration in an 'outlook' section.

Considering gross land use changes

How does this relate to the three issues in the previous section? Is this really a major driver of the 3 issues, or something along for the ride? It is clearly present in issues 1 and 2 (although the present day isn't discussed, only past and future), while its absence in IAM projection may be the relevant link (as the transitions are determined by a single independent model, which is part of issue 1)

Response: See earlier responses. We will clarify in the text that we do not argue that missing gross transition representation is a major driver of the uncertainty in land-use data sets used for the harmonized time series. Separate from the uncertainties discussed in the previous section we argue that gross transitions are an issue that have not got enough attention throughout the communities apart from shifting cultivation in the tropics.

page 7, lines 21-23: Just a note: You are well aware that gross transition information is highly uncertain, and current work suggests that the CMIP5 LUH data product may actually overestimate gross transitions in the tropical regions.

Response: Indeed, gross transition information is uncertain, especially if it is provided by models that have multiple other sources of uncertainty (as discussed in section 2) or simple assumptions about the spatial distribution of shifting cultivation have to be made (as in the CMIP5 harmonization). For this reason, we call for additional work on this topic, based on empirical data, such as the updated shifting cultivation estimate for the CMIP6 LUH product (which the reviewer probably refers to here) or the recent work by Fuchs et al.

page 7, line 32: there are no land cover categories in CMIP5 LUH, only primary and secondary land. wood harvest is associated with forest or non-forest, but this land cover designation is based on a threshold of a potential biomass model, rather than more commonly used land cover or potential vegetation data sets.

Response: We will remove 'land-cover' from the sentence, as it is indeed misleading along with the CMIP5 LUH data. But even if forest/non-forest land cover is provided for CMIP6, the decision how to derive the transitions (e.g., forest to crop or non-forest to crop) has to be made? We thus believe our main argument in this paragraph about the simplistic assumptions to derive transitions remain unaffected.

page 8, line 14: "…increasingly been captured…"

Response: Will be corrected accordingly.

Allocation of managed land in ESMs and DGVMs

Ah, finally! This aspect of separate land use and land cover information/modeling is a factor in all 3 of your main sources of uncertainty, and as such needs to be mentioned up front and related to these uncertainty sources.

Response: See the general response, previous responses and the response to reviewer #2 how we will clarify the structure in a revised manuscript to address this comment. In short, section 2 will focus on the uncertainties within the land-use and land-cover data provider community ('data quality') and its implications for climate assessments upon coupling to DGVMs/ESMs. Section 3 will pick up an important, but so far hardly considered challenge (gross transitions), while section 4 will discuss the issues arising when using external land-use data in DGVMs/ESMs, including the underlying source of the separated land-use and land-cover research lines.

page 8, lines 27-28: and scenarios and over relatively short time periods (see Di Vittorio et al 2014)

Response: We can remove 'over long time periods'.

page 9, lines 26-27: this is consistent with land cover being studied separately from land use, and your examples also relate to your second main source of uncertainty.

Response: The reviewer is correct that the overview of these studies also show that there is a mix of approaches in studying current land-use and land-cover changes that necessarily leads to some kind of uncertainty. However, this statement was intended to emphasize that we did not conduct a systematic review, but rather use the studies as an illustration to support our argument that the change pattern is spatially heterogeneous.

pages 9-10, lines 30-12: glad you did this! But what determines the source of land use in CLUMondo? It is important to clearly state how this model differs in this selection versus those that use the methods by which you classify the changes. Generally, more info is needed regarding how the different classified algorithms are defined, in relation to how they are implemented in dgvms/esms. The reader should be able to understand what is going on without digging through the supplemental material. Maybe a table of the definitions?

Response: We will include some more detail in the main text (P9L33 onwards; see a suggestion below), add a table with the definitions (from S2.4), and expand the detailed description in the supplementary materials. In the original manuscript we aimed to keep methodological descriptions as short as possible, as we apply the analysis mainly for illustrative purposes.

'CLUMondo models the spatial distribution of land systems over time, instead of land use and land cover directly. Land systems are among others characterized by a mosaic of land use and land cover (cropland, grassland, forest, urban, bare) within each grid cell. The land systems are allocated to the grid in each time step 'based on local suitability, spatial restrictions, and the competition between land systems driven by the demands for different goods and services' (Van Asselen and Verburg, 2013; Eitelberg et al., 2016). Thus the determination of the source land use can be interpreted as a complex algorithm taking into account external demands, the previous time step, local suitability in a grid cell and neighborhood effects. This strategy differs from the one in DGVMs/ESMs in a way that not one simple rule (e.g., grassland first) is applied to each grid cell equally, but accounts for spatial heterogeneity of land-use sources. We reclassified the CLUMondo outputs according to their dominant land use or land cover type to derive the transitions and classified the changes in the simulated data within each ca. 0.5 x 0.5 degree grid cell as either grassland first, forest first, proportional, or a complex reduction pattern (Table 3; see SI for details).'

page 10, lines 5-8: what about the undefined category, which is the dominant category according to the figures (not the complex)? what does it stand for? are you grouping this with the complex category?

Response: Good point. We seem to have missed the undefined category somewhere on the line and will add this piece of information to the text, including a table that will explain the individual categories. In principle, undefined means that it was not possible to detect on of the algorithms (forest first, grassland first, etc.). We are not grouping it with the complex category, as this category basically entails the opposite: all major land-cover types have been available, but there is no priority which one is reduced upon cropland expansion.

page 10, line 16: the IAM community has been projecting land use AND land cover for some time, although not necessarily gross transitions.

Response: Agreed. But it is about gross transitions and transition matrices here and as IAMs/LUCMs provide the transition matrices, they need to derive the exact source category somehow as well. How will this be done? As the reviewer mentioned earlier, there are not many IAMs which actually project land use and land cover at a regular grid scale, but instead resample/downscale aggregated results. Thus, it very much depends on the sophistication of the allocation procedure, how 'accurate' the derived transition matrices will be. Additionally, these downscaled maps are usually not evaluated against observational data. Thus, by using transition matrices (and the ability of DGVMs/ESMs to incorporate them), the issue would be solved for ESMs/DGVMs, but did not disappear and requires further research.

Conclusions and recommendations

page 11, lines 17-20: this is an important point, but it hasn't been clearly demonstrated in the text, largely because the paper generally refers to LULCC as a whole.

Response: We will resolve this issue by applying the revisions mentioned in our previous responses and the response to reviewer #2, i.e. refining land-use and land-cover terminology and reorganization of the sections. Additionally, we will move the discussion about model integration into a 'outlook' section and thus separating it from the more short-term recommendations.

page 11, lines 20-23: while not an individual, agent-based behavioral model, GCAM has been integrated with CESM as the iESM, implementing two-way feedbacks between the human and environmental systems, particularly for terrestrial systems and the effects on land projection. See the oft-noted paper above, which runs the iESM, and: Collins et al, 2015, The integrated Earth system model version 1: formulation and functionality. Geoscientific model development, 8, :2203-2219. There should also be a paper coming out soon on a complete experiment using the iESM to examine the effects of the feedbacks.

Response: We will accommodate this comment in the 'outlook' section of a reorganized manuscript by including a short discussion about efforts of model integration so far.

page 12, lines 1-19: It isn't clear how these suggestions relate to your three proposed primary issues with LULCC (which should also be restated in this conclusion – 1) uncertainty in lulcc data products is lacking due to not enough different products generated, 2) present-day lulc data are inconsistent and thus contain high uncertainty, and 3) uncertainty in lulcc projections is largely driven by initial data uncertainty over other model-specific sources). Issues 2 and 3 appear to mainly consist of data quality issues. Also note that your second main source on page 5 does not refer to gross or subgrid transitions at all, just to inconsistencies in present day data. Please make your conclusions/suggestions more consistent with the theme of the paper.

Response: See previous responses. The main challenges are meant to be (1) *Provision of a spatially explicit, continuous and consistent time series of LULCC*, (2) *Considering gross land-use changes* and (3) *Allocation of managed land in ESMs and DGVMs.* Related to these challenges we suggest to (1) Develop enhanced harmonized time series including uncertainty, (2) supplement the time series with estimations of gross transitions and (3) the development of transition matrices based on sophisticated land-use models and empirical data.

Supplemental material

Figure S1 seems a lot more complicate with more steps than described in the text, which makes more sense.

Response: Please see response to comment pages 9-10, lines 30-12.

Reviewer #2 (Anonymous)

The authors discuss in their manuscript the major uncertainties and shortcomings associated with the implementation of land-use and land-cover changes (LULCC) in climate change assessments. Additionally, three major challenges are identified and the reasons for them are discussed.

General Comment: Generally, I think this paper raises some important issues related to implementation of LULCC in the modelling community. Raising awareness to this issues with the help of an extended literature review will be beneficial in tacking this problems.

Response: We thank the reviewer for the kind words and the overall positive evaluation of our manuscript.

However, I think the manuscript would benefit from not only raising awareness of the issues and the reasons behind it but also providing a 'way forward'. This is sometimes done in the individual sections but I think the suggestions get lost in the wealth of information presented in the manuscript. I therefore suggest adding a separate recommendation section in which the authors clearly state in short and precise form what could/should be done to overcome the challenges and which part of the community they see in a better position to take the lead (if possible), instead of having the conclusions and recommendations combined in one section.

Response: Indeed, the manuscript intends to provide guidance for the involved communities (i.e., IAM, LUCM, DGVM, ESM, remote sensing) on the 'ways forward' based on the different challenges identified. We aimed to do this by discussing each of the challenges within one section and bringing them together in the conclusion section with additional recommendations. However, we see that this could be done better with an improved guidance for the reader throughout the manuscript. We think that the reviewer makes useful suggestions how to reorganize the manuscript in a way that the intended structure becomes clearer. We therefore decided to follow these suggestions and propose to improve the manuscript by:

- Streamlining the discussion within the individual sections into 'description of the challenge', 'underlying reasons' and 'examples'
- Adding subheadings accordingly (e.g., 'Background', 'Underlying reasons', 'Example(s)'
- Moving the recommendations from the individual sections to a separate 'recommendation' section preceding the overall conclusion section, including an indication which part of the community we think is in charge of taking the lead

We feel that this reorganization of the structure will help to simultaneously clarify the main concerns of reviewer #1.

The abstract and P3L5-10 ('overall objectives) do not match. Please make sure that these points are consistent throughout the document.

Response: We will ensure consistency throughout the document by rephrasing the particular part of the abstract and adding a paragraph giving guidance on the structure of the manuscript according to the response to reviewer #1.

This brings me to my general comment; The paper covers a lot of material but very often the structure of the document gets lot. I therefore suggest the authors to go through the document and make sure that the reader can follow the train of thought easily. This could be achieved for example by adding subheading to the sections and having similar structure within each of the sub-sections. This also applies to the Supplements which should also convey a clear structure within the sub-sections.

Response: We will clarify the structure as outlined in the response to the reviewer's previous comments and hope to resolve the issue raised here in this way. Additionally, we will make sure to clarify the structure of the supplementary material.

Specific Comments: In the title of the manuscript it should become clear that the document is about 'anthropogenic' land-use and land cover changes and that the assessments are with regard to 'climate change'. I therefore suggest revising the title.

Response: We can add 'anthropogenic' to the title and exchange 'climate assessments' by 'climate change assessments' to be unambiguous. We suggest to revise the title (in addition to the changes suggested by reviewer #3) into

'Current challenges of representing anthropogenic land-use and land-cover change in models contributing to climate change assessments'

P1L20-23: Suggested to split sentence

Response: We suggest to split the sentence into

'However, the processes and drivers of anthropogenic land-use activity are still overly simplistically implemented in Dynamic Global Vegetation Models (DGVMs) and Earth System Models (ESMs). The published results of these models are used in major assessments of processes and impacts of global environmental change such as the reports of the Intergovernmental Panel on Climate Change (IPCC).'

P2L22-26: I think this is an interdisciplinary journal, it would be beneficial for readers outside of the community, if examples of the uncertainties could be given (e.g. it might not be clear to the readers what 'definition issues' are (see also P5L 16)).

Response: We agree that the 'definition issues' were not sufficiently explained in the text. We will thus add an explanation and examples, e.g.

'Carbon fluxes are understood quite well for some compartments of the global carbon cycle, e.g. fossil fuel combustions and the ocean sink (Le Quéré et al., 2015), but the quantification of LULCC flux suffers from high uncertainties (Ballantyne et al., 2015) due to different definitions of individual land-use categories (e.g., what exactly is a 'pasture'), the definition of the land-use carbon flux (Pongratz et al., 2014; Stocker and Joos, 2015), the simplistic representation of LULCC in models that are used to quantify these fluxes (de Noblet-Ducoudre et al., 2012; Jones et al., 2013; Pugh et al., 2015) as well as the uncertainty about LULCC history (Ellis et al., 2013; Klein Goldewijk and Verburg, 2013; Meiyappan et al., 2012).'

Accordingly on page 5:

'While the rising operational application of remote sensing during recent decades has opened a powerful resource to map land cover on global scale, the exact definition of individual land-use and land-cover categories (e.g., Sexton et al., 2015), and difficulties in the distinction of them from the spectral response

(Friedl et al., 2010), lead to a variety of global land-cover products that do not agree about extent and spatial pattern of individual classes (Ban et al., 2015; Congalton et al., 2014).'

P3L1: which 'sources'? Please specify

Response: The sources are listed in the previous paragraph. We will add a reference to make this unambiguous.

P3L27-31 Please be more specific and elaborate on the dataset

Response: Unfortunately, there is not a single dataset we could elaborate on here and we will add a clarifying statement in the revised manuscript. We argue that the diversity of models produce a tremendous amount of data, which is not necessarily consistent and comparable due to the underlying reasons listed in the preceding paragraph (i.e., two independent land-use change modeling communities (historical, future), different data sources, varying assumptions and drivers in the models.)

P4L6&L9: What does 'LUH' and 'HYDE' stand for?

Response: The reviewer is absolutely correct that we missed to spell out the acronyms. We will include the full terms in the revised manuscript.

LUH stands for *Land Use Harmonization*, a commonly applied acronym for the harmonized land use data set developed by Hurtt et al. (2011). *HYDE* refers to the *History Database of the Global Environment* (Klein Goldewijk et al., 2011), a historical reconstruction of population and land-use activity.

P5L18: 'large differences' in which variable?

Response: We refer to global land-cover products in this sentence, i.e. the variable is land cover or the individual classes (such as forest, grassland, etc.), respectively. We will rephrase to remove ambiguity. See the suggestion in response to comment P2L22-26.

P5L23: what are the 'all relevant processes' to the authors? Elaborate. What is still missing.

Response: This is indeed a reasonable comment and the elaboration on this question would probably easily fill another publication, which we think is out of the scope of our manuscript. To address this comment we will add a summarizing sentence with a list of references that have discussed these relevant processes in detail, e.g. Verburg et al. (2011) and Erb et al. (2016).

Some of the relevant processes might include temporal dynamics based on 'observational' data for non-forest land types, the distinction of 'natural' grasslands vs. intensively used pastures and additional information on land management practices (see Erb et al., 2016), urban/peri-urban development or a consistent change product including the main classes (forest, shrubland, grassland, cropland, pastures, urban,..) in general.

P5L26: It is not clear what a 'marker scenario' entails.

Response: We suggest to add a clarifying sentence and references in this paragraph, e.g.

'A 'marker scenario' entails the implementation of a SSP scenario by one IAM that was elected to represent the characteristics of the qualitative SSP storyline best, while additional implementations of the same SSP, but by other IAMs, are 'non-marker scenario'. See Popp et al. (2016) and Riahi et al. (2016) for details.'

P5L33: 'large variations' in which variable?

Response: The variable is 'pasture areas'. We will rephrase the sentence accordingly to make this clear.

'For example, the projections of 11 IAMs and LUCMs show large variations in pasture areas in 2030 for many world regions (Figure 2, background map).'

P6L5: The differences in output arising from different models the input and calibration etc. is not only an issue in the assessment of LULCC but generally applies to all models…Maybe if you look at other modelling communities and how they quantify these uncertainties.

Response: We entirely agree that these issues are not unique to the LULCC modeling community. However, it is not the purpose of the paper to describe detailed methods/metrics of how the uncertainties could be quantified. But in the paragraphs following this statement, we outline what would conceptually be needed to (1) quantify the uncertainty range and (2) suggestions how it could be reduced.

We argue that little attention has been paid to the evaluation of land use models and the quantification of the uncertainty in their projections. This leads to a situation where often model and input data related differences dominate the scenario related uncertainties. We acknowledge that there are activities starting towards quantifying the uncertainties related to model assumptions (Riahi et al., 2016). However, two main issues are not resolved yet: (1) the 'marker' implementation of SSPS are intended to be used for climate change assessments without providing an error/uncertainty range due to different model interpretations, which would allow to quantify the uncertainty propagation into the final assessments and (2) all 'marker scenarios' are interpreted by a very similar kind of models (IAMs), which – especially when it comes to the spatial pattern of LULCC projections (= input to DGVMs/ESMs) – largely differ from alternative realizations of land-use allocation models (Prestele et al., 2016).

P 6L11: the problem is how to define 'plausible' realisations.

Response: It is indeed a difficult task. However, there are different products available for global land cover only (e.g., the ESA-CCI product, the MODIS product) or integrated with land-use statistics (e.g., Fritz et al., 2015; Klein Goldewijk et al., 2011; Ramankutty et al., 2008), which show substantial variations in the extent and especially the spatial pattern of land-use distribution. To elect one of these to the 'best' might be difficult due to technical and analytical constraints. Nonetheless, we would assume them to be 'plausible' since they are based on sound science (either remote sensing directly and/or remote sensing and statistics) and we will add this deliberation to the manuscript.

P7L 2: Elaborate why sub-grid dynamics 'have been shown important'

Response: We will add a few examples summarizing the results in the referenced publications.

P7L6: what are 'under-determined mathematical systems' in this context?

Response: We will clarify this in the revised manuscript.

We refer to Hurtt et al. (2011) here who describe the derivation of land-use transitions as a 'large, under-determined mathematical system', i.e. the final state of LULC in year t2 depends on the state of LULC in year t1 and all the transitions in between. As usually no or insufficient information is available on the transitions, we have to make assumptions (e.g., cropland expands on grassland in a grid cell) or constrain the system by empirical data.

P7L8: what are ‘minimum-transitions’ in this context?

Response: Minimum transitions entail the changes between LULC categories only accounting for one-directional changes. We will add this explanation to the text.

P7L21-23: Rephrase sentence

Response: We suggest to rephrase as follows:

‘It is thus important that Figure 3 indicates also in large parts of the temperate zone and high latitudes substantial gross changes that may be underestimated by the currently used LUH dataset.’

P10: rephrase ‘allocation issue’ do you mean the shifts between the communities and their perceived responsibilities

Response: ‘Allocation issue’ actually refers to the decision which land-cover type is replaced upon cropland and pasture expansion in the model. We suggest to rephrase to:

‘Providing such transition matrices, however shifts the decision which land-cover should be replaced upon cropland or pasture expansion from the DGVM/ESM community to the IAM/LUCM community.’

P10 Maybe you would like to add to your decision that ‘satellite data’ is also not ‘directly measured data’ but also goes through a mathematical conversion process.

Response: That is indeed a good point and we suggest to add a sentence that explicitly mentions that satellite data is not directly measured data, too. However, we think compared to the modeled LULCC data, satellite data entails a much more ‘directly measured’ component in most cases and can contribute to the evaluation of land-use change models.

P11L24: Can you elaborate what ‘improved communication’ should entail in an ideal case. Additionally, I think it is not only the ‘understanding’ but first the ‘awareness’ of different assumptions and constrains needs to be achieved and before one can understand and tackle the problems.

Response: We believe improved communication eventually entails engaging in model integration as outlined in our conclusion. Simultaneously the individual challenges discussed in the manuscript need to be resolved through joint engagement across the communities in the individual tasks. Indeed, raising awareness of different assumptions and constraints is a first step and a major objective of the manuscript. By reorganizing the manuscript as described in our previous responses, we will ensure this becomes clear in a revised version, including recommendations which part of the community could most likely take the lead.

Figures: P24: add a ‘log’ label to the legend and add colour areas without change in grey to make the light yellow areas better stand out.

Response: We will revise as suggested.

Supplement SP2L7: can you provide more details on the updated version, i.e. reference

Response: We will add reference as far as possible.

The updated version is based on Ramankutty et al. (2008) for the static map of cropland distribution. There was however no additional publication related to the updated dataset. The dataset was available from http://www.geog.mcgill.ca/nramankutty/Datasets/Datasets.html, but the webpage has recently been removed.

SP5: Can you comment on the uncertainties associated with the CORINE data

Response: We can add a short paragraph of uncertainties related to the CORINE data. We do not think that it would add important information to the manuscript, if we elaborate comprehensively on the uncertainty in the CORINE data. We actually only use the change product for illustration in terms of cropland transition trajectories. These aggregated results are probably not heavily affected by uncertainty in the CORINE data.

SP6L8: Can you elaborate on what the 'thematic accuracy' entails.

Response: 'Thematic accuracy' entails the capability of CORINE land cover maps to represent the 'true' land-cover class as compared to an independent validation data set (EEA, 2006). We will add an explanatory sentence and the reference in the respective section of the supplementary material.

Reviewer #3 (Anonymous)

The manuscript by Prestele et al., "Current challenges of implementing land-use and land-cover change in climate assessments", provides an overview of recent publications on interactions among land-use, carbon cycling, and different aspects of climate. First, the manuscript aims "…to identify existing shortcomings of the current LULCC representations within DGVMs and ESMs, reveal the underlying mechanisms and constraints that have hampered improved representations until now, and propose pathways to improve current representations" (page 3, lines 5-7). Second, based on the literature review, the manuscript attributes the lack of progress in including LULCC into climate assessments, to 1) the failure to account for uncertainty in reconstruction and future scenarios of gridded LULCC; 2) resolving sib-grid changes in land-use activities (e.g. gross transitions); 3) allocation of primary lands to managed lands in DGVMs and ESMs. Manuscript reviews a number of studies and discusses a wide range of limitations, specifically in CMIP5 historical reconstruction and future scenario. It has interesting discussion of how to use remote sensing data in improving treatment of LULCC processes in scenario development and its implementation into DGVMs and ESMs.

Response: The reviewer presents a good summary of the objectives of our manuscript. We thank the reviewer for the time spent on our manuscript and we appreciate the positive evaluation of our discussion about implementing remote sensing data in land – climate interaction studies.

For some of the following comments we sometimes split the original comments of the reviewer to address individual points.

However, the title is not appropriate because climate assessments such as IPCC do not implement LULCC – IPCC assessments review literature. CMIPs are not part of the IPCC, although their model simulations provide input to IPCC.

Response: The reviewer has a valid point here. Our current title is misleading, although we did not intend to equate the CMIP simulations with the IPCC assessment. We thus propose (in addition to the changes suggested by reviewer #2) to change the title into:

'Current challenges of representing anthropogenic land-use and land-cover change in models contributing to climate change assessments'

The manuscript has four major shortcomings: 1) While the manuscript reviews and synthesizes a number of recent studies on the development of scenarios of LULCC and of LULCC for climate and carbon cycling, it does not actually provide new insights or synthesis of LULCC implementation in ESMs and DGVMs. The manuscript provides a discussion of how the CMIP5 scenario was constructed and its limitations, but does not discuss differences in land use components of different ESMs or DGVMs. Or how they implemented the CMIP5 LULCC scenario. Table 1 gives 4 examples: 3 DGVMs (2 of which are variants of LPJ model) and a new HadGEM2-Jules ESM. There is no comprehensive analysis of CMIP5 ESMs or TRENDY DGVMs used in the AR5 in respect to LULCC. Thus, the manuscript's first goal is not supported by new insights beyond those previously published in literature.

Response: The reviewer raises a valid point that the wording of our main objectives leaves room for interpretation which requires clarification. It is not the purpose of the manuscript to focus on the technical details of land modules and/or LULCC implementation of ESMs/DGVMs. This has been done in related publications (e.g., De Noblet-Ducoudré et al., 2012) as the reviewer states correctly. Instead, we provide a synthesis of issues arising along the chain of activities regarding land-use in climate change assessments (remote sensing, modeling, scenario development, implementation in DGVMs/ESMs). For that purpose we review the literature and identify the three challenges that comprise our three main sections. For each challenge, we show why it is a challenge, what implications this challenge can have for carbon cycling and climate assessments, and discuss limitations in the current approach to overcome this particular challenge.

In this way, the manuscript provides important guidance to the communities involved, as we bring together the individual points for the first time, including recommendations for potential ways forward. In fact, Table 1 should be regarded as an illustration to support our argument rather than a comprehensive analysis. We feel accommodating changes as outlined in the response to the other two reviewers will better clarify the value of our manuscript.

Additionally, please note: We intentionally did not submit a 'research article' but a 'short communication' type of manuscript, since it is intended as a guidance or perspective for future research, rather than new fundamental research.

2) The manuscript claims that the limited characterization of uncertainty in CMIP5 and CMIP6 LU reconstructions and scenarios is responsible for the lack of progress on LULCC in climate assessments. There is no reason to believe that's true. CMIP is designed to compare climate models and ESMs under a common set of forcings and capture model structural uncertainty. CMIPs never claimed to capture all uncertainty due to input forcing. It's a well-established practice in climate MIPs to provide a standard scenario for all forcings – greenhouse gases, short-lived species, solar, constants, volcanoes and LULCC, particularly over historical periods. Such GCM or ESM simulations are extremely computationally expensive. Permutation of alternative forcings datasets is not likely something that many climate centers will be able to engage and afford. The idea of multiple LULCC reconstructions advocated by the paper for CMIPs is not practical. If some modeling group/center wants to explore uncertainty due to LULCC, there is more than one scenario that is available even from the GLM model: Hurtt et al. (2006) included both scenarios based on SAGE and HYDE datasets. Hurtt et al. 2011 examines different assumptions in the GLM model.

Response: We do not seek to claim that 'the limited characterization of uncertainty in LU reconstructions and scenarios is responsible for lack of progress on LULCC in climate assessments' in our manuscript. We argue that the current characterization of uncertainty is insufficient and errors unaccounted for propagate into climate assessments. For example, LUMIP (contributing to the goals of CMIP6) aims to answer the scientific question '*What are the global and regional effects of land-use and land-cover change on climate and biogeochemical cycling (past-future)?*' (Lawrence et al., 2016), which we think can only be done if there is a sufficient quantification of uncertainty in the land-use forcing data set in place as well.

We agree with the point that CMIP is designed to compare climate models instead of forcing data sets, but it is similarly true that CMIP – due to its highly structured design – acts as a prototype for activities outside of CMIP and its forcing datasets (and as such the LUH) are widely used as a standard outside of CMIP, too. Please note, we do not restrict our arguments to the CMIP comparisons, but use it as an example at several places in the manuscript due to its high impact and pioneer role in the community.

We will make sure to clarify these distinctions in a revised version.

In our view a 'well-established' practice is not necessarily the same as 'best practice'. The fact that climate modeling centers cannot (or in some cases do not prioritize to) explore uncertainty in LULCC does not necessarily imply that it should not be done at all. If not practical in such a comprehensive way as proposed in section 2 of our manuscript, then the communities need to come up with alternative strategies to tackle these uncertainties, e.g. determining a minimum LULCC accuracy required for climate assessments using less computationally expensive DGVMs or offline land surface models (see our conclusion section). Harmonization to a common input for climate models is a major first step to have LULCC included in the climate simulations; in a next step the communities need to find a way to systematically approach the related uncertainties. Here our recommendations and conclusions could provide guidance on how to move forward and we will rephrase them to be more specific.

The main bottleneck for improving LULCC characterization in the CMIP is poor representation of LULCC processes in GCMs and ESMs. Most CMIP5 ESMs or TRENDY DGVMs can't use the information available in CMIP5 or CMIP6 historical reconstructions or future scenarios. For example, most of the CMIP5 models use only information about land use fractions, and not gross transitions provided by the Hurtt et al. (2011) data set. With the exception of very few models, ESMs do not represent shifting cultivation or wood harvesting.

Response: We agree that one of the major issues in land use – climate interaction studies is the 'poor representation' of LULCC in GCMs and ESMs. In fact, we identify it as one of the main issues in our manuscript as well (section 4) and will emphasize it additionally following the suggestions of reviewer #1 and #2. Given the low certainty in inputs of additional products such as wood harvest and shifting cultivation (Erb et al., 2016; Hurtt et al., 2011), inclusion of the processes in ESMs is not necessarily the only bottleneck. Simultaneously, we do not agree that a 'main bottleneck' (e.g., the poor representation) justifies neglecting other important issues (such as the uncertainty in LU modeling and the gross transitions) we raise in the manuscript. Instead, the LU modeling community should clearly communicate these issues as well and take the lead on improving the products.

Another unsupported assumption in the manuscript is that, by making additional ESMs or GCMs with alternative representations of LULCC history, one would get a better handle on the uncertainty in climate feedback of LULCC. It's not necessarily true: most studies with and without LULCC typically find a small difference in global climate and small regions with statistically distinguishable differences in climate characteristics. One would need a large ensemble of such simulations to find differences between the biogeophysical effects of alternative LULCC reconstructions and scenarios, unless they are really different as in future scenarios. Biogeophysical differences should be more pronounced, but the problem is that CMIP5 or even CMIP6 ESMs are incapable of representing major LU processes such as shifting cultivation, wood or crop harvesting..

Response: Regarding biogeochemical effects it has been shown that alternative reconstructions make a large difference to carbon emissions (e.g., Bayer et al., 2016; Meiyappan et al., 2015). From these findings we derive that alternative reconstructions are 'likely to substantially impact' LULCC – climate interactions and propose to determine a minimum accuracy that would be required from LULCC time series to not significantly impact on ESM output. For example, the work of Kaplan et al. (2011) and Fuchs et al. (2013) has shown that historical reconstructions can substantially differ from HYDE, thus we think it is an important scientific question how alternative reconstructions could affect the climate signal. Additionally, the uptake of a high/low estimate of historical land use in LUMIP (Lawrence et al., 2016) after feedback from the community (see the open discussion of the LUMIP paper, Lawrence et al. (2016), in *Geoscientific Model Development*), indicates that the uncertainty in LU products is indeed an important issue that needs to be further explored.

3) The manuscript questions assumptions in CMIP5 Hurtt et al. 2011 reconstruction and future scenario. The Hurtt et al. (2011) effort, for the first time, harmonized historical reconstruction with the 4 Representative Concentration pathways (RCP) scenario and took into account gross transitions between different LU types in both tropics and extra-tropics. The authors are mistaken in their assumption that no-shifting cultivation in the extra-tropics implies no gross-transitions in the extra-tropics; for example, non-zero transitions between pastures to crops and crops to pastures. Furthermore, for CMIP6 (Lawrence et al. 2016), there will be a focus and additional LUH reconstructions available, as well as more details about the relationship between land cover and land use categories. I think a lot of criticism of the CMIP5 LULCC reconstruction and scenario is valid but the authors are overlooking improvements in the new reconstruction for CMIP6, which is publicly available now on the CMIP6 website.

Response: We acknowledge the effort of the Hurtt et al. (2011) harmonization activity and its contribution to enhance LULCC representation in land use – climate interaction studies. But, as the reviewer states later on, there are also limitations in the CMIP5 product. We do not assume that gross transitions can only appear in the tropics due to shifting cultivation (see P7L17-20), but argue that due to the resolution of the minimum transitions, gross transitions, especially in the temperate zone and the high latitudes, might be missed (P7L11ff.).

In terms of CMIP6 and the LUH2 product, we do not overlook the improvements, but explicitly mention the update (P5L6). However, we admit it is extremely difficult at the moment to follow the improvements compared to the LUH CMIP5 product, since documentation of the new products is restricted to a rather generic description (Lawrence et al., 2016) and thus we might miss some details. We are aware that the new historical product is publicly available, but the final dataset does not allow to trace back how the individual processes were implemented. To our best knowledge – apart from the indisputable improvements between the two products (e.g., additional focus on land management, improved shifting cultivation estimate) – some of our main criticisms (e.g., gross transitions in the extra-tropics, derivation of LU transitions) will be untouched even with the new product.

While it's possible to construct more detailed scenarios for recent periods with satellite coverage or for specific countries (e.g., Table 2 in the manuscript), particularly in the Northern Hemisphere, it is difficult if not impossible to develop multi-century reconstructions on a global scale with consistent sets of assumptions. Making simple assumptions in ESM is not an unreasonable approach for global, multi-century analyses. Assuming transitions based on the satellite era for the entire CMIP-style experiments may be problematic, as well, for pre-industrial or future periods.

Response: We agree that making simple assumptions is a reasonable approach to get started with the land-use implementation in DGVMs/ESMs and that the satellite era might not be representative for multi-century analysis as well. However, at least for this era the model assumptions should be carefully evaluated. Based on sufficient transition information for present-day, these assumptions could be, e.g., gradually replaced over time (backward and forward) based on scenario assumptions or regional characteristics (e.g., Fuchs et al., 2015). Sensitivity analysis could provide additional insights how individual decisions affect the land-use pattern, even over long time periods. In such a way the models would account for spatio-temporal variability in land-use transitions.

4) The rationale for including analysis from the CLUMondo model is not clear – it demonstrates how spatio-temporal variations could be different within the grid. It does not show that such patterns will affect climate or carbon cycling. Besides the CLU-Mondo analysis, there is now new analysis in this manuscript. So, there are no new insights/analysis, just a synthesis of other studies, which are already partially covered by the authors in related publications (e.g., Alexander et al. 2016, Bayer et al, 2016, Prestele et al. 2016).

Response: We will add a sentence to our objectives , which explicitly mentions the illustrative purpose of the CLUMondo analysis (see our general response for a suggestion). Specifically, the rationale for including CLUMondo analysis is to show that a simple allocation algorithm (such as forest will be cleared upon cropland expansion) applied globally might not sufficiently account for the spatio-temporal heterogeneity in the change patterns. Previous publications have shown that these decisions can affect regional climate and carbon cycling (e.g., de Noblet-Ducoudre, 2012), and thus our analysis should be taken as an illustration that further research is required on how these decisions affect the ESM results.

As mentioned in previous responses, the manuscript does not aim to present comprehensive new analysis, but rather use illustrative analysis using the CLUMondo model to support our arguments. In doing so, we provide a synthesis of currently untackled, or insufficiently tackled, challenges at the interface of land-use and climate modeling, and try to present guidance for the communities involved.

I think the most interesting part of the paper is the section on remotely sensed data (high and low resolution) in development of new diagnostics for evaluation of global LULCC reconstructions or models. Perhaps the authors can re-frame their analysis and demonstrate how such data can be used to improve or evaluate reconstructions (e.g. the one in CMIP6) or to create new diagnostics to evaluate ESMs and DGVMS.

Response: In our view, our manuscript brings together three major challenges/issues at the interface of land-use and climate modeling, which can serve as a guidance on the 'ways forward' to the communities involved – we therefore do not share the opinion that the remotely-sensed data should be the chief focus of the paper. Certainly there is also a need to develop new diagnostics as mentioned by the reviewer, but this is beyond the scope of this current paper. However, the reviewer raises a fair point and we will add this need to the 'outlook' section of the revised manuscript.

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