

# Responses to reviews for manuscript **Global Transition Rules for Translating Land-use Change (LUH2) To Land-cover Change for CMIP6 using GLM2**

## **To Reviewer #3:**

**Reviewer 3:** The manuscript entitled Global Transition Rules for Translating Land-use Change (LUH2) To Land-cover Change for CMIP6 using GLM2 by Ma et al. aims at recommending a global transition rule for translating LUH2 land-use forcing into land-cover changes in CMIP6 models. The authors simulate land-use induced land-cover changes based on a set of translation rules using the GLM2 model and prescribed LUH2 land-use transitions. Subsequently, emerging present-day forest cover, biomass density, and LUC emissions are evaluated against published estimates. The authors conclude by recommending a rule where all vegetation is cleared upon cropland/pasture expansion and only forest vegetation is cleared upon rangeland expansion.

The paper is technically correct, well written, timely in providing recommendations how to translate land-use forcing into land-cover changes for CMIP6 and the content is generally suitable for publication in GMD. I am wondering, however, if the conceptual design of the study is valid in the context of the large uncertainties that exist in the reconstruction of historical land use. Likewise, I do not think that the main conclusion ('optimal' rule being rule 1) is supported by the results presented. The wording ('optimal transition rule', 'accurate quantification', [...]) is not suitable in the context of large uncertainties. Rather than claiming an 'optimal' transition rule, I suggest a framing towards recommending a 'consistent' translation rule for implementation of land use in ESMs/DGVMs. A consistent treatment in ESMs/DGVMs would eliminate added uncertainty and complexity from different treatment in each model. If this is 'optimal' (or and 'optimal' global rule even exists), is difficult to judge with available data and not shown by the contents of this manuscript. The reproducibility of the analysis presented is questionable, especially as neither the code of GLM2 (though stated in the code and data availability section) nor any documentation of the model is publicly available at the moment.

**Response:** Thanks for your comments which have helped us improve the manuscript. We have revised the manuscript including changing word of 'optimal' to 'recommended' and strengthening the justification of rule determination at result section by adding spatial analysis and regional comparisons. Note that 'transition rule' has been renamed as 'translation rule' throughout the manuscript to avoid confusion with LUH2 land-use transitions.

Regarding the comment of the conceptual design, we agree that large uncertainties exist in current land-use modelling products, and it is difficult or even impossible to propose globally 'optimal' rule to translate land-use change to land-cover change regardless of the land-use dataset. Therefore, we have modified the last paragraph of introduction section to clarify the goal that recommended through our evaluation only guide the implementation of LUH2 (historical part) in CMIP6. Besides, we agree with your point that "globally consistent rule could eliminate

added uncertainty and complexity from different treatment in each model”, we have added this at end of the second last paragraph of introduction section.

**Reviewer 3:** Conceptual design. One of the main (implicit) assumptions in the manuscript is that the land-use transitions from LUH2 are ‘correct’, although these transitions are affected by large uncertainties and necessarily based on many assumptions. Given also the presented results that show simulated forest cover (and mostly also carbon emissions) in the range of previously published results for rules 1-4 (and partly even for the ‘analytical’ rules), I am wondering how valid any conclusions drawn regarding an ‘optimal’ translation rule can be. Without evaluation if any of the reference estimates is ‘better’ than others, I cannot see why Rule 1 is more ‘optimal’ than Rule 2, 3, or 4 (as long as they are all within the range). Moreover, if we can see only this small discrepancy already with assuming LUH2 data as ‘correct’, how do the authors think, would this evolve, if accounted for uncertainties in the land-use reconstruction? For example, how would the LUH2 high and low estimates change the results? How would prioritizing another land-use type in the allocation of land-use transitions in GLM2 (if this still exists like in GLM) change the results? And in conclusion: Does it even matter which of the rules is applied in an ESM/DGVM given these probably much larger effects from the mentioned uncertainties (besides of being consistent across CMIP6 models)?

**Response2:** Given the role of LUH2 as required forcing dataset, our goal was to determine the best translation rules given these data to inform and standardize the use in future modeling studies. To address uncertainty, we now include estimates of uncertainty in key reference datasets. We show that given this uncertainty, it is not technically possible to differentiate performance between some of the possible alternative rule choices. However, here we do confirm that Rule 1 performs among the best through these analyses consistent with the HYDE recommendation, and therefore have increased the confidence in recommending its standard usage. While LUH2 does provide a historical high-low, we focus here on the reference dataset only due to its required usage in model forcing. The Land-Use Model Inter-comparison Project (LUMIP) is organized to compare model performance using this forcing dataset.

**Reviewer 3:** Wording/Framing. Closely related to the comment above, I do not think that the wording in the manuscript is appropriate at many instances. The authors should avoid terms like ‘accurate’, ‘optimal’, etc. in the context of historical land-use change and its translation to land-cover change. As the authors state correctly in their discussions, globally valid transition rules probably do not exist and I would encourage the authors to rather emphasize the underlying uncertainties than trying to hide them behind strong words. Instead of claiming to derive ‘optimal’ rule(s), it would be more useful to recommend a ‘reasonable rule’ that should be used consistently across CMIP6 models.

**Response:** Very good suggestion. We have removed these adjectives where possible.

**Reviewer 3:** Conclusions. The main conclusion (=recommending rule 1) is not supported by the results presented. For all of the proposed diagnostics (forest cover, biomass density, carbon

emissions), all of the rules (1-4) are within the range of the diagnostics (sometimes even some of the analytical rules which are supposed to be idealized/unrealistic), and all of them are far from ‘accurately’ translating land-use change to land-cover change as the authors claim. The fact that on average one of the rules is closer to an averaged reference map does not provide justification that one of the four rules is superior over the others. The authors need to provide more justification why they recommend rule 1 and/or 2 based on the results presented here. Personally, I do not think this is possible without either defining a ‘best/most suitable’ reference map (e.g., based on GLM2 forest definition) or extended (spatial) analysis to identify regional characteristics of the different rules. Generally, the discussion/conclusion section needs to be strengthened, as in its current form it mainly repeats methods/results instead of discussing the findings of the analysis.

**Response:** We largely agree and have re-framed the analysis in response to these concerns. In the revised manuscript, we compare performance to each rule to each reference map as opposed to the average. We also include a treatment of uncertainty in the reference maps. In the end we now show its difficult to differentiate between Rules 1-3 and recommend Rule 1 for both its relatively good performance and underlying prior recommendation from the HYDE.

**Reviewer 3:** General. Please check the whole manuscript for missing whitespaces in front of references and within references.

**Response:** Changes made.

**Reviewer 3:** P1 L15 ‘accurately’ does not seem to be an appropriate wording given the large uncertainties both in climate and land-use modeling. Please remove.

**Response:** We have rephased this sentence to remove the word ‘accurately’.

**Reviewer 3:** P1 L16 I would suggest to use ‘land-cover change time series’ only, i.e. remove the ‘land-cover’.

**Response:** We have rephased it as “*climate models need consistent land-cover change time-series at a global scale*”.

**Reviewer 3:** P1 L18-21 Please include that GLM2 was used for the simulations already here.

**Response:** They are changed to “*Building upon the latest Land Use Harmonization dataset (LUH2), land-cover dynamics, particularly in forest cover and carbon stock, were simulated based on each rule from 850 to 2015 globally by Global Land use Model 2 (GLM2) at quarter degree spatial resolution.*”.

**Reviewer 3:** P1 L23-25 I think ‘optimal transition rule’ is not the correct wording. This sentence is also quite complicated. I would suggest to rephrase, emphasizing that within GLM2 the

mentioned rule turned out to perform best. The wording here indicates that this rule is ‘optimal’ irrespective of the model used, which is not supported (and probably also not intended) by the results of the manuscript.

**Response:** We have reorganized this sentence as “*Examinations at global, country, and grid scales indicate that the recommended translation rule for CMIP6 models is 1) completely clear vegetation in land-use ...*”.

**Reviewer 3:** P1 L26-28 I am wondering if mentioning the detailed forest area is required, while not referring to the carbon density and LUC emissions at all?

**Response:** We have added results for carbon density there as “*According to this rule, contemporary global forest area is estimated to be 37.42 10<sup>6</sup> km<sup>2</sup>, and forest area estimates at global and country scales both stay within the range derived from remote sensing products. Likewise, the estimated carbon stock is in close agreement with reference biomass datasets, particularly over regions with 50% forest cover. This rule also mitigates the anomalously high carbon emissions from land-use change observed in previous studies in the 1950s*”.

**Reviewer 3:** P2 L5-7 I think it is not correct to present this statement as a fact. The numbers are based on a historical LU reconstruction, i.e. model results. Please rephrase to, e.g., ‘Model results show [...]’ or ‘It has been estimated, [...]’.

**Response:** We have rephased it as “*It has been estimated that, during the past 300 years, >50% of the land surface has been affected by human land-use activities, >25% of forest has been permanently cleared, and 10-44 10<sup>6</sup> km<sup>2</sup> of land are recovering from previous human land-use disturbances*”.

**Reviewer 3:** P2 L7 Include ‘amongst others’ as there are many more impacts of LUC and land management on the carbon cycle than deforestation, afforestation, and wood harvest.

**Response:** P2 L8-10 has modified as “*Impacts on the carbon cycle result from several processes among others: deforestation removes natural forest and ...*”

**Reviewer 3:** P2 L12-13 Same as above. Please highlight that this is also an (uncertain) model result, e.g. by including the uncertainty ranges.

**Response:** We have rephased this as “*Cumulatively, models estimate that land-use land land-use change have contributed to a net flux 190±75 Pg C to the atmosphere during 1870-2017 (Le Quéré et al., 2018)*”.

**Reviewer 3:** P2 L13-14 What are ‘these emissions’? Please rephrase in a way that the reference to land-use emissions becomes clear.

**Response:** We have rephased it as “*While emissions from land-use and land-use change only account for 10% of current anthropogenic carbon emissions, they were a dominant contributor to increasing the atmospheric CO<sub>2</sub> above pre-industrial levels before 1920*”

**Reviewer 3:** P2 L13-15 The numbers presented here are probably all derived by using LUH(2) as land-use forcing. Thus, I think it would perfectly fit the storyline to add a sentence that explains that exactly due to these uncertainties a ‘better’ translation between land use and land cover is required. Otherwise, one may ask, why we would need all these transition rules, if we already know about historical land-use impact.

**Response:** We have added uncertain range at the end of 1<sup>st</sup> paragraph of introduction and pointed out at the next paragraph that a globally consistent translation rule is required for ESMs and DGVMs.

**Reviewer 3:** P2 L18-22 What about just saying LULCC reconstructions enter Earth System Models (ESMs) (e.g., Lawrence et al., 2016), Dynamic Global Vegetation Models (DGVMs) (e.g., Le Quéré et al., 2018), and bookkeeping models (Hansis et al., 2015; Houghton and Nassikas, 2017) to quantify biogeochemical and biophysical impacts of historical land-use change.’ I don’t think the details about models and MIPs is required here.

**Response:** We have changed these lines as “*LULCC reconstructions enter Earth System Models (ESMs) (Lawrence et al., 2016), Dynamic Global Vegetation Models (DGVMs) (Le Quéré et al., 2018) and bookkeeping models (Hansis et al., 2015) to quantify biogeochemical and biophysical impacts of historical land-use change as part of historical simulates (DECK and CMIP6 historical simulations), future projections (scenarioMIP), impacts studies (ISIMIP), paleoclimate studies (PMIP), land-use specific simulations (LUMIP), and biodiversity studies (IPBES)*”.

**Reviewer 3:** P2 L18-22 Remove ‘(e.g., HYDE, SAGE)’. Replace ‘Goldewijk et al., 2017’ by ‘Klein Goldewijk et al., 2017’. Add Ramankutty and Foley, 1999 and Pongratz et al., 2008.’

Ramankutty, N. and Foley, J. A.: Estimating historical changes in global land cover: Croplands from 1700 to 1992, *Global Biogeochem. Cycles*, 13(4), 997–1027, doi:10.1029/1999GB900046, 1999.

Pongratz, J., Reick, C., Raddatz, T. and Claussen, M.: A reconstruction of global agricultural areas and land cover for the last millennium, *Global Biogeochem. Cycles*, 22(3), GB3018, doi:10.1029/2007GB003153, 2008.

**Response:** These lines are changed to “*Considerable efforts have been devoted to modelling historical land-use states (Goldewijk et al., 2017; Kaplan et al., 2009; Pongratz et al., 2008; Ramankutty and Foley, 1999) and ...*”.

**Reviewer 3:** P2 L24-26 The manuscript is about historical land use, i.e. the harmonization with future LULCC seems to be an irrelevant information here.

**Response:** The lines are changed to “*In particular, the recent Land-Use Harmonization 2 (LUH2) dataset (Hurtt et al., 2017) has been developed to provide global gridded land-use states and transitions in a consistent format for use in ESMs as part of CMIP6 experiments*”

**Reviewer 3:** P2 L28-33 Remove the reference ‘Shevliakova et al., 2013’ between the sentences and only put it in the end of the paragraph.

**Response:** We have moved the citation to the end of the paragraph.

**Reviewer 3:** P3 L1-3 I would not agree that there is ‘a lack of explicit global rules’. As the authors show later on, it is relatively easy to come up with some. I would rather argue that there is no consistency/agreement on which rule to apply. Apart from that, in this context it is also worth to mention that such ‘global transition rules’ probably do not exist at all [see, e.g., Prestele et al. 2017].

Prestele, R., Arneth, A., Bondeau, A., de Noblet-Ducoudré, N., Pugh, T. A. M., Sitch, S., Stehfest, E. and Verburg, P. H.: Current challenges of implementing anthropogenic land-use and landcover change in models contributing to climate change assessments, *Earth Syst. Dyn.*, 8(2), 369–386, doi:10.5194/esd-8-369-2017, 2017.

**Response:** We agree that there is no agreement on which rule should be used, thus we have changed ‘a lack of explicit global rules’ to “a globally consistent rule”.

**Reviewer 3:** P3 L5 ... ‘and the location where a land-use change happens’.

**Response:** It has been changed as “*the degree of land-cover alteration varies with the types of land-use changes and the location where a land-use change happens*”.

**Reviewer 3:** P3 L5-7 While this statement sounds very intuitive, I wonder if there is any literature supporting these tendencies?

**Response:** We agree that the potential activity is intuitive, but are also not aware of specific literature on it.

**Reviewer 3:** P3 L11-15 Complicated sentence. Please shorten. In my opinion, everything after ‘...not yet provided’ is not necessarily required. What about joining with the following sentence

instead? Isn't it exactly what the authors are aiming at: providing recommendations how to treat these 'new land-use types' in the translation?

Suggestion: 'However, explicit suggestions for land-cover and carbon stock modifications resulting from these new defined land-use types are not yet provided, but are crucial for the translation of land-use change to land-cover change within ESMs or DGVMs. An inconsistent translation will potentially produce very different land-cover dynamics, which will impact the land surface biophysical and biochemical processes.'

**Response:** Great suggestions. these lines are changed as "*However, explicit suggestions for land-cover and carbon stock modifications resulting from these new defined land-use types are not yet provided, but are crucial for the translation of land-use change to land-cover change within ESMs or DGVMs. An inconsistent land-cover translation of these land-use products within an ESM or DGVM will potentially produce very different land-cover dynamics, which will impact the land surface biophysical and biochemical processes.*"

**Reviewer 3:** P3 L18 I would not agree that the approach presented here will reduce any uncertainty. It rather can provide recommendations for consistent treatment across models, if the 'optimal' rule is adapted by the CMIP6 models. But this does not allow any conclusions how uncertainty will be affected.

**Response:** these lines have been revised as "*To recommend a global translation rule for translating historical land-use changes for CMIP6 models, this study investigates the impacts of land-use change on land-cover by proposing several alternative sets of translation rules, which are then integrated into the Global Land use Model 2 (GLM2) model (Hurtt et al., 2017, 2019) to simulate ...*"

**Reviewer 3:** P3 L22 Remove 'other' in front of 'independent'.

**Response:** change made.

**Reviewer 3:** P3 L22-25 Here, too, I recommend not using the term 'optimal'.

**Response:** We have changed 'optimal' to 'recommended'.

**Reviewer 3:** P4 L1-5 Is there a specific reason not to include the ESA CCI land cover for comparison? Remove the details about the comparison dataset here. They are all mentioned in section 2.5

**Response:** the ESA CCI land cover dataset has different land-cover classification schemes with the six satellite-based datasets, and therefore the legend translation (Table S1 from Song et al 2014) may be not applicable for ESA CCI. We have re-organized P4 L1-L5.

**Reviewer 3:** P4 L7-28 As the method section is already quite long, I would suggest to shorten here. I do not think there is a lot of added value to describe LUH2 in this detail for the purpose of the paper. I guess there will be an associated LUH2 publication soon, so it is probably enough here to just describe the key features that are relevant for the analysis in this manuscript.

**Response:** section 2.1 has been shortened.

**Reviewer 3:** P4 L11-13 This sentence doesn't seem to fit in the context here.

**Response:** It has been removed.

**Reviewer 3:** P4 L17 While 'data-driven' is probably not wrong, I think it is misleading as it implies that the constraints used in LUH2 are based on observations. However, to my knowledge most of the constraints are model outputs in some way (be it the HYDE reconstruction or models derived from remote sensing images, etc.). Therefore, I would recommend not to use 'data-driven' here.

**Response:** We have removed 'data-driven' and this revised sentence is "*The LUH2 dataset was generated with the GLM2 (Hurtt et al., 2017, 2019), which like its predecessors (Hurtt et al., 2006, 2011), estimates annual sub-grid-cell land-use states and transitions by including multiple constraints such as gridded patterns of historical land-use from the HYDE database (Goldewijk et al., 2017), ...*".

**Reviewer 3:** P4 L28 Where do the 2 kg C/m<sup>2</sup> come from? How do they relate to other forest definitions? Are there any references that could support this threshold? Some more information would be valuable for the reader.

**Response:** It is difficult to link biomass density to tree density as their relationship may strongly vary with tree species and also locations, and there are many different definitions of forest in the literature. The threshold value of 2 kg C/m<sup>2</sup> potential biomass was used for consistency with prior studies and GLM2/LUH2 (see references below).

*Hurtt GC, Pacala SW, Moorcroft PR, Caspersen J, Shevliakova E, Houghton R, Moore B (2002) Projecting the Future of the US Carbon Sink. Proceedings of the National Academy of Sciences of the United States (PNAS)/ 99(3): 1389-1394.*

*Hurtt GC, Frohling S, Fearon MG, Moore B, Shevliakova E, Malyshev S, Pacala SW, Houghton RA (2006), The underpinnings of land-use history: three centuries of global gridded land-use transitions, wood harvest activity, and resulting secondary lands. Global Change Biol 12:1208–1229*



*G. C. Hurtt, L. P. Chini, S. Frohking, R. A. Betts, J. Feddema, G. Fischer, J. P. Fisk, K. Hibbard, R. A. Houghton, A. Janetos, C. D. Jones, G. Kindermann, T. Kinoshita, Kees Klein Goldewijk, K. Riahi, E. Shevliakova, S. Smith, E. Stehfest, A. Thomson, P. Thornton, D. P. van Vuuren, Y. P. Wang (2011) Harmonization of land-use scenarios for the period 1500–2100: 600 years of global gridded annual land-use transitions, wood harvest, and resulting secondary lands. Climatic Change 109:117–161*

**Reviewer 3:** P5 L1 What are the ‘analytical purposes’ of rules 5-9? For the rest of the manuscript they are mostly used to state that the results with these results are ‘way off’, but this is not very surprising given their idealized/unrealistic character. I would therefore recommend to leave them out, as they rather add confusion.

**Response:** Rules 5-9 are included for model experimental design completeness and for reference to the other cases. In the text, we clearly differentiate them as such.

**Reviewer 3:** P5 L8-12 I agree that the effect of spatial and temporal varying rules is beyond the scope of this study. However, these are very strong simplifications and it would be useful to get an indication of how including this variation would affect the results. Maybe the authors could look a bit more detailed into the country-level and gridded results for the different rules and diagnostics. Can there be seen any patterns, if one of the rules is ‘more likely’ in certain regions than in others? If this is not feasible, the authors should include more detailed elaboration how the results may be affected in the discussion section.

**Response:** We have added regional comparison of Rules 1-3 estimates of carbon density at Figure S5.

**Reviewer 3:** P5 L18-19 It sounds a bit ‘circular’ that the output of GLM2 (i.e., LUH2) is used as input into GLM2 for the analysis in this manuscript. Could the authors provide more explanation how this was implemented? Are these independent model runs?

**Response:** To better explain the model runs, we have rephased these lines as “*In this study, land-cover change is simulated by performing a modified GLM2 simulation in which the computed land-use transition rates (using the same methodology as LUH2) are supplemented with a set of translation rules (Table 1) to track forest cover change and carbon dynamics at 0.25° spatial resolution*”

**Reviewer 3:** P5 L23-24 More detail required regarding the model run (time period, etc.).

**Response:** We have updated the model info such as inputs, time period, environmental factors not considered at the first paragraph of section 2.3.

**Reviewer 3:** P5 L23-29 These are effectively ‘results’. I would recommend to move to section 3.1.

**Response:** They have been moved to section 3.1.

**Reviewer 3:** P5 L31 ff. I do not know about the specifications of GLM2/LUH2, but in LUH1 [Hurtt et al. 2011], choices had to be made about starting date, priority for land-use transitions, wood harvest inclusion, etc. If this still exists for GLM2/LUH2, it would be useful to indicate here, which configuration of GLM2 was used to derive LUH2 to allow the reader to understand how the historical transition rates have been derived. In the discussion, a short evaluation of how changing these assumptions would change the results of the analysis, would help.

Hurtt, G. C., Chini, L. P., Frohking, S., Betts, R. A., Feddema, J., Fischer, G., Fisk, J. P., Hibbard, K., Houghton, R. A., Janetos, A., Jones, C. D., Kindermann, G., Kinoshita, T., Klein Goldewijk, K., Riahi, K., Shevliakova, E., Smith, S., Stehfest, E., Thomson, A., Thornton, P., van Vuuren, D. P. and Wang, Y. P.: Harmonization of land-use scenarios for the period 1500-2100: 600 years of global gridded annual land-use transitions, wood harvest, and resulting secondary lands, *Clim. Change*, 109(1), 117–161, doi:10.1007/s10584-011-0153-2, 2011.

**Response:** For the LUH1 dataset we performed a large sensitivity analysis in which we systematically varied model inputs and decisions. However, owing to the complexity and increased detail of LUH2 we have not performed a similar sensitivity study for this dataset. As such, there are now choices to be made by the user regarding the specifications of the dataset. Of course, this does not imply that there is no uncertainty in the various model factors and changing the inputs and model decisions would ultimately change the carbon accumulated in the land surface, but would not necessarily change the overall recommendation of which translation rule to use.

**Reviewer 3:** P7 L26 Not an expert in carbon impacts of land management. However, I am wondering if management of land necessarily means that there is no further accumulation of biomass in the remaining ‘natural’ vegetation?

**Response:** When natural vegetation remains on managed land there are a range of possibilities – the accumulation of carbon could decrease over time (if for example that vegetation is grazed), it could remain constant (if it is explicitly managed and with the intent to keep it), or it could grow with time (if it is allowed to expand spatially). In the absence of any other information we have chosen a simple, middle-of-the-road, assumption that the carbon in remaining vegetation is explicitly managed and remains constant. Furthermore, we have acknowledged the possible impact of the assumption at the results section that consistent underestimation of carbon stocks in Rules 1-3 may be related to this assumption.

**Reviewer 3:** P9 L20 Instead of only using an average ‘smallest difference’ for the gridded results, looking more into the spatial patterns would maybe help to derive stronger justification for recommending one of the suggested rules.

**Response:** We have added difference maps of forest cover and carbon density (Figure 2, S2 and S3), and zoom in some regions for detailed comparison (Figure S5).

**Reviewer 3:** P9 L29 ‘Higher forest cover’ compared to what? The average reference map? Unclear. In addition, rather than presenting the three forest cover maps in Fig. 2, it would be more useful to show difference maps (rule 1-3 minus reference; rule 4 minus reference). This would facilitate the identification of differences between the maps.

**Response:** “higher” has been changed to “high”, we just want to claim Rules 1-4 could reproduce the general pattern of forest cover such as where has more forest than other places. Furthermore, we have added the difference maps in Figure 2 for better comparison.

**Reviewer 3:** P10 L9-11 Here, the authors emphasize the uncertainty in the definition of ‘forest’, which cannot easily be resolved. Which definition used in one of the reference maps is closest to the forest definition used in GLM2 ( $> 2 \text{ kg C/m}^2$ ). Using the ‘closest’ map to compare the GLM2 results to would probably give a better indication than having a huge range of ‘reference’ maps (where the range partly originates ‘only’ in definition issues).

**Response:** It is good to evaluate rules with multiple independent satellite dataset to reduce uncertainties originating from particular methods or sensors, but also difficult to visualize the comparison between reference maps and model estimates. Here, we still prefer to keep these six satellite datasets, but extend Figure 4 by including comparisons to all rule to each of the six datasets rather than the averaged dataset. The extended analysis still supports our conclusions that Rules 1, 2, 3 outperform Rule 4 in terms of gridded forest cover.

**Reviewer 3:** P10 L12-15 Rule 7 is within the range according to Fig. 3.

**Response:** These lines haven been updated to “*The forest cover based on Rules 6, 8 and 9 is beyond the range of the diagnostics, indicating that these rules underestimate the impacts of land-use change on land-cover and overestimate the global forest existing in the present day.*”

**Reviewer 3:** P10 L16-17 Rule 5 and 7, too (see Fig. 3).

**Response:** These lines have been modified to “*In contrast, Rules 1-4, 5 and 7 produced estimates of global forest area within the range of diagnostics.*”

**Reviewer 3:** P10 L19-20 The statement is not wrong, but also the analytical rules ‘locate’ around 75% of global forest land in these eight countries. This cannot be used as a characteristic of distinction between the rules.

**Response:** The sentence “*Rules 1-4 also produce the same pattern of locating most forest land within these eight countries (Table 4).*” has been removed.

**Reviewer 3:** P10 L27-28 I am not sure, if the mean average delta compared to an average global forest map is a good metric here. The average reference map is a rather ‘artificial’ map, and not necessarily the most plausible one. Does the assessment of ‘smallest’ difference change, if compared to the reference maps individually? Rather than using averages, I think the authors should aim at identifying a reference map that corresponds most with the forest definition within GLM2 and compare to this map. Additionally, showing a map of the differences would also allow to identify if certain rules match the current situation better in particular regions. As several rules are within the range of published forest cover areas, this would allow a better justification for one certain rule and/or regional diversification.

**Response:** Good suggestion to determine a reference map for evaluation. However, it is difficult to find such one which correspond most with GLM definition, as GLM does not has forest cover map before applying the rules of the paper. Instead, we have extended the Figure 4 by comparing rule estimates to each of six satellite-based forest cover maps as well as to the averaged map. We also added difference maps in Figure 2 for better justification.

**Reviewer 3:** P10 L10-12 What happened to rule 4? From Table 5 it can be seen that it reduces the pasture anomaly, too. Why does it not appear any more in the text and Figs. 5/6?

**Response:** Emissions of Rule 4 have been added to Figure 5 and 6.

**Reviewer 3:** P10 L13 Is the difference of 1 Pg C really a ‘significant’ difference?

**Response:** We have rephased this sentence as “*Rule 2 reduces more anomalous emissions than Rule 1 (reduced 6 Pg C in Rule 1 and 7 Pg C in Rule 2),...*”

**Reviewer 3:** P11 L16-20 The differences in the average difference between model and reference are rather small across all rules (except for some of the analytical). Again, are the authors sure that this average difference is a suitable indicator (see also comment above reg. forest area)?

**Response:** The “small” difference across rules is what we anticipated for two reasons. First, Rules 1-3 have same treatment of land-use changes to cropland, but different treatment for those from non-forested land (primary and secondary) to managed pasture or rangeland. Second, the carbon density of non-forested land is also relatively small (usually below 2 kg C/m<sup>2</sup> in GLM2). Therefore, the small difference in carbon density and emission estimates are expected. For better readability of Figure 8, we rescaled the y-axis, we can see Rules 1-3 outperforms Rule 4. And also, we have also calculated the averaged difference over different regions (i.e. four latitudinal bands in Figure S4) instead of averaging over globe.

**Reviewer 3:** P11 L22-25 In Fig. 9 hardly any difference can be seen for the three rules. How large are the differences between the individual rules and the reference? How do the authors conclude from this Fig. that rule 1 and 2 are closer than rule 3? What happened to rule 4?

**Response:** We have modified Figure 9 by calculating the relative difference in carbon stock comparing to two carbon density maps. Rule 4 is excluded as it shows large bias in forest cover in Brazil and also larger averaged difference in carbon density (Figure 3), Figure 9 compares carbon stock of Rules 1-3 and then determine which should be recommended.

**Reviewer 3:** P11 L28-30 Please see major comment ‘Conclusions.’

**Response:** Please see the response to comment ‘Conclusions’ at P11.

**Reviewer 3:** P12 L1-5 These statements are not wrong, but only repeat parts of the results. Please remove.

**Response:** We have reorganized the discussion section, and these lines also have been removed.

**Reviewer 3:** P12 L7-8 How do the authors think that the results presented here can facilitate the reconstruction of historical land-cover change? Please elaborate.

**Response:** We think by combining the LUH2 land-*use* transitions with the suggested rules for land-*cover* changes that could occur during those land-use transitions, it would enable the reconstruction of land-cover changes over the entire historical period. We have re-organized the whole discussion.

**Reviewer 3:** P12 L10 ff. It is still not clear at this point why rule 1/2 are ‘better’ than rule 3/4. Additionally, I wonder what is the added value of the study, if one of the main conclusions is that rule 1 is ‘better’ than rule 2 due to assumptions taken in HYDE.

**Response:** Our analysis takes the suggested rule from the HYDE3.2 paper, and subjects that rule to a series of tests designed to determine if that rule is consistent with multiple datasets and assumptions. This analysis goes beyond that which is presented in the HYDE paper. Furthermore, this analysis is performed within a consistent modeling frame-work, and in particular, with a different underlying map of land-cover/biomass.

**Reviewer 3:** P12 L24-30 While not wrong, I think irrelevant here as it (1) mainly repeats what has been written in the methods section and (2) does not provide justification for one of the rules presented. The authors should aim at emphasizing the reasons why they recommend rule 1 to CMIP6 models.

**Response:** We have re-organized the discussion section and also explained why rule is recommended.

**Reviewer 3:** P13 L6 Not clear why the authors introduce a new model here.

**Response:** we have removed it.

**Reviewer 3:** P13 L14 I do not agree with this statement/conclusion. Several rules presented here lead to similar results (within the range of reference maps) and justification is missing, why one of the rule is better than another. The claim that an ‘optimal’ rule has been determined by the analysis is not supported by the results.

**Response:** We have changed ‘optimal’ as ‘recommended’ and added extra figures and explanation at results section to support the justification of rule recommendation.

**Reviewer 3:** Figure 1 Isn't Fig. 1(b) a binary map (forest/no-forest)? In this case the legend doesn't make sense.

**Response:** No, it is the forest cover map of which most of grid-cells have fractions close to 100% at year of 850.

**Reviewer 3:** Figure 2 Please add difference maps to facilitate the identification of differences between the maps. Only 4 rules (instead of 9 as mentioned in the caption) are shown.

**Response:** Caption is corrected, and also the difference maps are included.

**Reviewer 3:** Table 1 Are the ‘analytical rules’ required for the purpose of the manuscript?

**Response:** Inclusion of Rules 5-9 could be used to interpret individual impacts of cropland, managed pasture and rangeland expansion, also give baseline estimates of resulting forest cover and vegetation for ESMs/DGVMs if similar rules are implemented.

**Reviewer 3:** Figure 3 For the analytical rules the x-axis labels are not centered any more.

**Response:** Figure 3 recreated to address this concern.

**Reviewer 3:** Figures 5-6 Why is rule 4 omitted from these Figs.?

**Response:** Rule 4 is included in the recreated Figures 5 and 6.

**Reviewer 3:** Figure 7 Also here difference maps would help to guide the reader.

**Response:** The difference maps have been added in Figure S2

**Reviewer 3:** Figure 8 Switch Rule 1 – Rule 2 (x-axis).

**Response:** Figure 8 recreated.

**Reviewer 3:** Figure 9 Differences between the rules hardly can be seen. Maybe zooming in for different percentages would improve the readability?

**Response:** We have modified Figure 9 by adding two zooming-in subplots.