General comments

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.

Major comments

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)?

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.

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.

Specific comments

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

Abstract.

P1 L15	'accurately' does not seem to be an appropriate wording given the large uncertainties both in climate and land-use modeling. Please remove.
P1 L16	I would suggest to use 'land-cover change time series' only, i.e. remove the *'land-cover*'.
P1 L18-21	Please include that GLM2 was used for the simulations already here.
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.
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?

Introduction.

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, […]'*.
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.
P2 L12-13	Same as above. Please highlight that this is also an (uncertain) model result, e.g. by including the uncertainty ranges.
P2 L13-14	What are '*these emissions*'? Please rephrase in a way that the reference to land-use emissions becomes clear.
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.
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.
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.

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

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

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 land-cover change in models contributing to climate change assessments, Earth Syst. Dyn., 8(2), 369–386, doi:10.5194/esd-8-369-2017, 2017.

P3 L5 … '*and the location where a land-use change happens*'.

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

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.'

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.

P3 L22 Remove '*other*' in front of '*independent*'.

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

Methodology.

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.

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.

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

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.

P4 L28 Where do the 2 kg C/m² 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.

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.

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.

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?

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

P5 L23-29 These are effectively 'results'. I would recommend to move to section 3.1.

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., Frolking, 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.

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?

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.

Results.

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.

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 kg C/m²). 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).

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

P10 L16-17 Rule 5 and 7, too (see Fig. 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.

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.

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?

P10 L13 Is the difference of 1 Pg C really a ‘significant’ difference?

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)?

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?

Discussion and Conclusions.

P11 L28-30 Please see major comment ‘Conclusions.’

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

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

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.

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.
P13 L6	Not clear why the authors introduce a new model here.
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.

Figures and Tables.

Figure 1	Isn't Fig. 1(b) a binary map (forest/no-forest)? In this case the legend doesn't make sense.
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.
Table 1	Are the 'analytical rules' required for the purpose of the manuscript?
Figure 3	For the analytical rules the x-axis labels are not centered any more.
Figures 5-6	Why is rule 4 omitted from these Figs.?
Figure 7	Also here difference maps would help to guide the reader.
Figure 8	Switch Rule 1 – Rule 2 (x-axis).
Figure 9	Differences between the rules hardly can be seen. Maybe zooming in for different percentages would improve the readability?