General comments

I appreciate that the authors reconsidered the wording throughout the manuscript and added analysis, now presenting a more nuanced discussion of their approach to test different rules for the translation between LUH2 land-use and land-cover for ESMs. However, their main conclusion (=recommendation of rule 1) is not covered by the results of the analysis and sufficient justification for the exclusive recommendation of this rule is still missing. The framing of the manuscript still indicates the opposite, e.g. by the following statement in the abstract:

'*Examinations at global, country, and grid scales indicate that the recommended translation rule for CMIP6 models is 1) completely clear vegetation in land-use changes from primary and secondary land (including both forested and non-forested) to cropland, urban land, and managed pasture; 2) completely clear vegetation in land-use changes from primary forest and/or secondary forest to rangeland; 3) keep vegetation in land-use changes from primary non-forest and/or secondary non-forest to rangeland. This confirms the translation rules suggested earlier in the HYDE dataset underlying LUH2.*'

(1) The examinations across scales do not exclusively indicate rule 1 (instead rules 2 and 3 are equally likely), which the authors also state in the manuscript and in the reply to the reviewers.

(2) The examinations do not confirm the translation rule suggested by HYDE. Instead, the earlier suggestion from HYDE is used as (the main) justification to pick rule 1 instead of rules 2 or 3.

One way out would be to be very clear about the fact that rule 1 is only recommended to achieve consistent implementation in future simulations (i.e., it would require to be a major point in the discussion and also in the abstract) and this recommendation is NOT a result of the analyses in this manuscript (as these show that with the same arguments also recommendation of rules 2 and 3 could be justified).

In this context, the manuscript would also benefit from a more critical discussion about the downsides of a consistent 'translation rule' (which is not necessarily supported by available data). In my opinion, it is reasonable to aim at a standardized translation between LUH transitions and ESM land cover. But such a standardization always comes at the cost of omitting uncertainties, instead of actually reducing them. If, for example, the '*added uncertainty of 43 PgC in CMIP6*' (as stated in the discussions) is avoided by implementing a consistent 'translation rule' this does not necessarily mean that the uncertainty is not there anymore; it might be just not depicted in the ESM results anymore. Only if the authors could show by their analysis that one rule performs significantly better than others, this would be an indication for actually 'reducing' uncertainties.

If the authors do not want to put more emphasis on the consistency aspect and/or highlight the limitations of their results (i.e., basically we do not know about the 'correct' rule), they would need to show with their analysis that rule 1 outperforms the other rules.

In sum, I think it is a useful study/analysis and worth to be published, but requires more nuance in the presentation of results, limitations, and derived conclusions.

Some of the comments from the previous review were poorly addressed (pages/lines refer to the original comments/manuscript).

P3 L1-3	The authors did not address the comment on the (non-)existence of 'global transition rules'
P3 L5-7	What is the basis for this statement, if it's not supported by literature? Some previous analysis? I think without a reference it is a misleading statement.
P4 L1-5	I am sure there are suitable legend translations for ESA CCI land cover as well and it's one of the most up-to-date datasets, but indeed it's not a critical issue.
P4 L28	While I see that it is difficult to link biomass density to tree density as the authors state, I think it would be worth to give an indication which one of the forest definitions in the literature (and also the ones in the reference maps used for comparison) is closest to this 2 kgC/m^2 definition. This definition has the potential to affect the results and deserves some attention.
P5 L1	The intention to include rules 5-9 is still not clear. Although it might be useful for test/sensitivity runs (also for the ESM community), I think it doesn't make sense to include them if the main purpose of the manuscript is to derive a realistic/recommended translation rule (where these rules by definition are not useful). In the results (incl. tables and figures) they are hardly revisited and rather add confusion to some of the results. In my opinion, the authors should decide to either include all rules in all results/tables/figures or stick to rules 1-4. To concentrate on a different set of rules at different sections of the results is confusing.
P10 L9-11	I see the authors intention to include the whole range of currently available forest reference maps. However, it would be still useful to give an indication which one is closest to the GLM2 forest definition. If we would know, for example, that one of the products has a similar forest definition, this could increase the confidence/justification for one of the rules.

Minor comments

Page/line numbers refer to the revised manuscript.

P1 L30	Reference biomass is also close for rule 2 and 3.
P1 L30	Should it be: '[…] regions with forest cover larger than 50%'?
P2 L16	As there is now already a carbon budget update, it might be good to use the latest values/reference. Friedlingstein, P. et al. 2019. Global Carbon Budget 2019. Earth Syst. Sci. Data 11, 1783–1838. https://doi.org/10.5194/essd-11-1783-2019
P3 L7-9	It is not only the lack of a globally consistent rule, but also the fact that the existence of such a global rule is very unlikely and a large simplification (see original comment P3 L1-3).
P3 L21-23	But also obscures the uncertainty from the lack of process understanding and lack of dedicated spatially explicit treatment.
P3 L25	'which **are** then integrated'
P9 L20	'accounted for in bookkeeping **model** based studies'
P9 L29	'should be **close** to diagnostics'
P9 L31-33	It's not 'other criteria, such as …', but the only one that is used in the end to identify the recommended rule.
P10 L17-19	I don't understand what the authors intend to say here?
P11 L2-4	Due to these large discrepancies it would be even more helpful to guide the reader with some information about which forest definition (of the reference maps) is closest to the GLM forest definition. (see original comments P4 L28; P10 L9-11).
P11 L17-18	And are within the range for Brazil, US, Congo, Indonesia, Peru.

P12 L6-14	All the realistic rules (1-4) reduce the pasture anomaly. Is this then just the difference between LUH1 and LUH2 or really a characteristic of the individual rules?
P13 L3-5	On average and globally. The regional and gridded comparisons (Table 4, Supplements) indicate that this might not hold at the country and grid level. Misleading statement.
P13 L6-8	It's actually hard to say if it is 'better' given all the uncertainties in these comparisons.
P13 L14-16	Which is also true for rule 2 and 3.
P13 L23-25	The uncertainty is not really reduced by implementing a consistent rule, as long as we do not know, which rule is 'correct'. It's just omitted from evaluation.