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Comment on essd-2022-245

Anonymous Referee #1

Referee comment on "Harmonising the land-use flux estimates of global models and national inventories for 2000–2020" by Giacomo Grassi et al., Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-245-RC1, 2022

This study represents a step-forward based on previous works by Grassi et al. and the 2021 Global Carbon Budget update. It extends the reconciliation between NGHGIs and bookkeeping models on carbon fluxes over 'managed land' to cover different categories of land use (and land use change) and to cover different nations and regions. It further establishes a framework that allows such reconciliation in the future bearing in mind both methods could evolve. Then future directions for improving the method consistency and confidence in quantifying national achievements in managed land carbon sink are provided.

I appreciate having the opportunity to review this work. This can definitely clarify confusions for those who are not so familiar with this field. I suggest its publication after addressing my comments. Most of my comments are to help enhance clarity. Some comments are rather a little philosophical and the authors can consider them as suggestive to enhance their discussions if they find it useful, but not that they must be addressed in a hard way.

General comments:

- In the methods section: need to explain that DGVMs in S2 simulations did not explicitly simulate secondary forest, but their simulated sinks driven by environmental changes

over the domain of secondary forests from another map was used, to avoid confusions. The underlying assumption is that environmental responses of carbon sink over primary and secondary forests are the same, which is not necessarily true (Lines 366-367 describes something relevant but the results there is just a coincidence I believe. DGVMs do not simulate secondary forest so any difference, if found, can only emerge from segregation of spatial grids. I suggest removing results in lines 366-367 because it makes no real sense).

- The use of Potapov et al. (2017) to approximate unmanaged forest needs to be justified. The fact that it has been used in previous studies (Deng et al., 2022) does not justify its appropriateness. The most important point is, under the very loose definition of 'managed land' in the current IPCC guidelines, the domain of managed land is almost completely up to the nations and completely loses the objectivity with almost no relation to the actual state of the land whether it's managed or not. **Hence the comparisons in the first two columns of Supplement Table 1 can potentially be out of pure coincidence.** Note that the nations have incentives to expand the areas of managed land, whatever their real status, if they have confidence that the concerned pieces of land will be a carbon sink for a reasonably long time in the future (hence the consistency in reporting managed land is a key in NGHGIs). I don't see the advantage of using Potapov et al. compared to an alternative approach where one just uses the fraction of managed vs. unmanaged land to simply distribute the total simulated indirect carbon sink to managed land. Second, there is the conceptual inconsistency between primary forest and the IFLs in Potapov et al. as the authors explained in their paper, and the limit of the 500 km2 minimum size, but this is rather a minor point.
- The separation between forest carbon sink and deforestation fluxes is nice but also comes with uncertainty. The key is fluxes relate to gross deforestation might have not been reported by nations and the area undergoing gross deforestation depend on the spatial resolution of the land cover data used. The discussions and associated uncertainties in this respect need to be strengthened.
- I also noted the lively discussions between Malte Meinshausen and Sandro Federici. While there is perhaps no need to provide the corrections due to indirect effects because readers who understand this paper can easily obtain them by using the supplementary Table 1. But I suggest authors enhance the discussions regarding the potential leakages in current IPCC guidelines. That is, when the pervasive indirect effects of increasing CO2 show as a sink-enhancing term over actually unmanaged land, the nations have incentives to claim these lands as managed lands and the associated carbon sinks as the national contributions to mitigate climate change. But when the same indirect effects show as a carbon source term in forms of growing wildfires or large-scale forest dieback, the nations will have incentives to say these are natural disturbances and are not national liabilities.

Below are some minor comments:

Lines 86-88: we know that these differences reside in their respective concepts and are unlikely resolved by 'new observations and platforms.' Please rephrase.

Line 933-934: “direct human-induced effects” should be “direct land use change effects”, land-use change includes shifting cultivation, harvest and regrowth due to abandonment or afforestation/reforestation. Need to rephrase here.

Figure 1: strictly speaking there are only unmanaged lands if trendy S2 simulations are meant here. Need to also state in the methods that environmental effects on managed land are treated equal as those on unmanaged lands, which is a critical assumption in the paper but not necessarily true.

Line 112: “global models”: do you mean bookkeeping models? As trendy S3 is not used throughout the whole study, it’s better to be specific.

Line 125: this gives a sense that DGVMs simulate secondary forest. But in Grassi et al. (2018), only the spatial map of secondary forests (from LUH2?) were used and applied on DGVM simulated carbon sinks of intact land, no? Please correct to provide a more accurate description.

Line 128: “proxy for managed” => should be proxy for environmental effects on management forest.

Line 178: “natural land cover changes (Sitch et al., 2015)”, I guess it should be “natural vegetation dynamics”. The authors should mean land cover changes that are driven by environmental changes rather than human-induced land use.

Line 179: ‘anthropogenic fluxes’ and line 192 ‘anthropogenic CO2 emissions’. These should be both ‘land use change emissions’, not to be confused with ‘anthropogenic CO2 emissions’ in lines 93 and 95.

Line 191: what do you mean by ‘forest thinning from below’?

Line 283-286: Note that simulated natural carbon sinks over non-forest land account for ~ 1/3 by DGVMs (Table A8, 2022, ESSD; also shown in Fig. 10 in this paper). According to the approach used here, I guess some of this non-forest natural sink is ascribed to secondary forests. This might need clarification. This points needs to be considered also relevant with lines 310-315.

Lines 374-377: the discussions here comparing the adjustments from a single model and from an ensemble of models seem not having a lot of insights for me. The agreement might be just out of coincidence. I suggest removing it.

Lines 384-391: The authors can just explain that the simulated natural sink per forest area was used from DGVMs so that the effects of hypothetical forest areas at preindustrial conditions were filter out, which would be easier to understand.

Line 460-461: I guess there is also the difference in gross vs. net land use change between those accounted for in BMs and by GHGIs, not limited to shifting cultivation.

Line 566: “NGHGIs typically report estimates of gross deforestation” => Are there evidences supporting this? At least this seems not the case for China and India because they do not show any deforestation flux. Reporting gross vs. net changes in forest area in the category of deforestation could be an important source of uncertainty, because gross forest change typically depends on the spatial resolution of the underlying land cover data that are used to derive land cover change.
(https://www.nature.com/articles/s41467-021-22702-2)

Line 559-560: Could it be an option for the land use change segregations used in BMs to move close to IPCC guidelines? A greater disaggregation in BMs will for sure better, but does not necessarily lead to better comparison with GHGIs and allow avoiding the cross-walking like in Table 1. The large discrepancy in the category of ‘other lands’ (Fig. 9) probably points to potential underlying mismatch in land use change category between these two approaches.

Line 586-587: with climate warming as a directional change, the assumption that climate-driven changes or variations in disturbance will cancel to a net-zero effect over time will unlikely hold. This comes back to the discussions between Malte Meinshausen and Sandro Federici (cf. the open discussion online:
https://essd.copernicus.org/preprints/essd-2022-245/). Both being as directional changes, carbon sink enhanced by CO_2 concentration increase are accounted as national contributions to carbon sink, but carbon sources enhanced by climate change (e.g. wildfires) were left out.

Line 590-592: completely agree with this.