ANSWER to Referee #1

(answers by the authors are in red)

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.

Thank you for the positive and very constructive comments. Please find below our replies.

Many thanks 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).

  Thank you. As suggested we added in the method section that DGVMs in S2 simulations "The S2 simulations of DGVMs assume all forests to be natural and do not explicitly simulate secondary forests."

  and later "although the differences in carbon uptake by natural and secondary forests are not considered by these runs, the DGVMs gridded results capture the spatial heterogeneity of the sink in terms of different forest types, soil types, and local climate"

- 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.

On the need to justify the use of Potapov's (and Hansen's) map:

- We clarified that "a previous study (Grassi et al. 2021) found "intact" and "non-intact" forest areas being a relatively good proxy for "unmanaged" and "managed" forest areas in the NGHGIs".

- We only partly agree that "the comparisons in the first two columns of Supplement Table 1 can potentially be out of pure coincidence". While it is true that countries have a large freedom to define their managed area, we think that, in absence of better (country-specific) information, the Hansen/Potapov map represent an acceptable proxy. After all, if relevant unmanaged areas exist in a country, these are far more likely to occur on what Potapov et al. consider "intact" area (e.g.,. far from roads). In Suppl table 1 we also added country-specific information, for example on Australia, to explain/discuss any relevant mismatch between what the NGHGI counts as "managed" and what our study sees as "managed".

- With regard to "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", we think that actually our approach (using Potapov in the absence of better information) has the advantage that the DGVMs gridded results capture the spatial heterogeneity of the sink in terms of different forest, climate and soil types. This is added in the text (see above).

- On the "conceptual inconsistency between primary forest and the IFLs in Potapov et al. and the resolution used by DGVMs (0.5 degree), we agree, but given the data from DGVMs, there is not much we can do. However, in the revised manuscript, we made available the combined global map that we used, at two resolutions (0.5 and 0.05 degrees), to allow other studies to apply even a greater resolution than the one we used with DGVM data.

- To address, at least in part, the reviewer's concern, we developed a specific map of managed/unmanaged forest for Russia, which is one of the countries with the largest areas of unmanaged forest. Although a digital map of the managed forests considered by the Russian NGHGI is not available, the Russian GHG inventory team shared with us specific maps for each administrative region. These maps indicate the fraction of forest and the relative share of managed/unmanaged forests in each administrative region. These maps were used to guide an adjustment of the threshold used in the Hansen's forest map, aimed at obtaining a better match at regional level between our area of managed forest and the one used in the NGHGI. Specifically, the use of a threshold of 10% in tree cover (rather than 20% as for the other countries), combined with the same approach applied for the other countries, allowed us to obtain values of managed forest area which resemble reasonably well the Russian regional maps (see new Supplementary figure 2) and match well the total managed forest area reported for the whole country (666 Mha in our map, 686 Mha in the NGHGI). Although our approach does not produce an exact map of the managed forest used in the NGHGI, we think it represents a step forward in representing the spatial allocation of Russian managed forests. The resulting map for Russia was then combined with the country-

specific maps for Canada and Brazil and the Hansen/Potapov map for the remaining countries, to produce a global map of managed forests. Since Russia, Brazil and Canada represent the three countries with the largest area of unmanaged forest globally, our approach minimizes the impact of using the Hansen/Potapov map. The combined global map is made available online (https://zenodo.org/record/7541525#.Y8WF8ezMJEI) at two resolutions (0.5 and 0.05 degrees), to allow other studies to apply the same approach. Given the increasing interest by the scientific community in verifying data from NGHGIs, we think this new global map represents a useful added value compared to the original manuscript.

- 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.

  Thanks, we agree. As suggested, we strengthened the discussion with regard to shifting agriculture and deforestation, in two points:

  "While the separation of BMs' results into various land categories helps the comparison with the NGHGIs, an important source of uncertainty (especially for NAI countries) is how the fluxes from shifting agriculture are allocated, i.e., if they are placed into forest, deforestation, or other. Specifically, in this study BLUE and OSCAR allocate emissions from shifting agriculture under "deforestation" and any subsequent removals under "forest" (e.g. for OSCAR, this corresponds to +3.5 $GtCO_2$ yr^{-1} under deforestation and -2.5 $GtCO_2$ yr^{-1} under forest for the period 2000-2020); H&N allocates emissions from shifting agriculture under "deforestation" only after the first conversion occurs (this corresponds to about +1.1 $GtCO_2$ yr^{-1} for the period 2000-2020), and thereafter the emissions and removals (overall a small net flux) are allocated to "other fluxes". The quantitative importance of shifting agriculture for CO_2 fluxes is also confirmed by Harris et al. (2021). For NGHGIs, it is often unclear if and under which categories the fluxes due to shifting agriculture are reported. While the above difference may help to explain the larger emissions from deforestation in BMs than in NGHGIs, and also the larger forest sink in the adjusted BMs (i.e. including the natural sink estimated by DGVMs for the managed forest area) than in NGHGIs, the lack of reliable information from most NAI represents one of the biggest sources of uncertainty in our comparison at the level of individual land-use categories"

  "Estimates of deforestation fluxes from models strongly depend on the underlying datasets, including the spatial resolution of the land cover data or statistics used (Winkler et al. 2021). For example, a recent study (Ganzenmüller et al. 2022) concluded that deforestation emissions based on high-resolution activity data substantially lower the previously estimated emissions using the LUH2 datasets (used here by BLUE and OSCAR). On the other hand, some NGHGIs do not report emissions from gross deforestation (for example, China and India)."

- 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.

With regard to the suggestion by Meinshasuen, we now added all our data online (https://zenodo.org/record/7541525#.Y8WF8ezMJEI). This online repository includes, for each country, the CO_2 flux from global models (BMs for each land-use category, and the ensemble mean of the DGVMs with the sink in intact/non-intact forest, i.e., the adjustment applied to BMs) and from NGHGIs (for each land-use category). Furthermore, in the same repository, we made available the detailed protocol to process the DGVMs results and the map of managed/non-intact forest that we used.

With regard to the potential leakages in current IPCC guidelines -Volume 4, Chapter 3 of the 2019 Refinement to the 2006 IPCC Guidelines on National Greenhouse Gas Inventories - we clarified that: "Furthermore, extending the area for which CO_2 fluxes are reported and accounted for could impact the fairness of the mitigation efforts: forest-rich countries could be incentivised to expand the area of managed forests to more easily reach emission reduction targets and carbon neutrality. However, according to the IPCC (2006), when moving unmanaged land to managed land it is good practice to describe the processes that lead to the re-categorization, i.e. countries cannot move lands in and out the NGHGI without evidence of the actual status of the land as well as of the legacy of past events (for this reason, shifting from managed land to unmanaged land is not a good practice, as the legacy effects of past management can continue for long periods). On the other hand, such a choice could imply large (and potentially uncontrollable) compliance risks for the country, associated with, e.g., permafrost thawing, large fires, etc. The concept of managed land has also been designed to reflect the intention to report and account only those fluxes that countries consider manageable. Of course, some unmanageable flux may also occur on areas considered managed by countries, which may also pose compliance risks. Related to this and following IPCC methodologies (IPCC 2019a), countries like Canada and Australia already disaggregate emissions and subsequent CO_2 removals from large natural disturbances occurring on managed land (under the assumption that fluxes compensate over time), with the aim to better isolate the anthropogenic signal on land-use emissions, and to reduce the risk that uncontrollable natural events threaten the fulfilment of the country's climate targets (IPCC 2019a, Kurz et al. 2018). It is important to note that these natural disturbance emissions and subsequent removals are excluded from the accounting, but are reported in the NGHGI. In the future, it is likely that emissions due to natural disturbances will increase under climate change (Anderegg et al. 2020), and that the positive effects of indirect effects on the net land sink will decline (e.g., CO_2 fertilization will likely tend to zero under high mitigation scenarios, Canadell et al. 2021). Due to the associated compliance risk, the application of the second option (report and account for all land) could induce large and unforeseeable political risks in several countries. Yet, quantification of GHG emissions and removals on unmanaged land remains of high scientific relevance and should be encouraged"

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.

Thanks, we slightly rephrased it.

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.

We rephrased as "Bookkeeping models consider as anthropogenic only direct human-induced fluxes from land-use change, such as from deforestation, shifting cultivation, wood harvest, and regrowth after harvest or abandonment of agricultural lands."

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.

As suggested above, we added that:

" The S2 simulations of DGVMs assume all forests to be natural and do not explicitly simulate secondary forests."

and later "although the differences in carbon uptake by natural and secondary forests are not considered by these runs, the DGVMs gridded results capture the spatial heterogeneity of the sink in terms of different forest types, soil types, and local climate"

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.

Thanks, we modified the text and now specify that we refer to Bookkeeping models

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.

Thanks, we changed into "the CO2 fluxes from nine DGVMs were filtered with a map of secondary forests and added to the fluxes from one BM"

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

Thanks, we followed the suggestion

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.

Indeed, we used "natural land cover changes" in contrast to land-use-induced land cover changes. We have replaced this by "natural vegetation dynamics in response to environmental changes" to be even clearer.

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.

We have deleted this sentence in response to reviewer 3's comments, which made us slightly re-write this section.

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

We deleted that, it is not strictly needed.

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.

We now explain that the flux over non-forest land is high because it includes the loss of additional sink capacity at the end of the methods section.

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.

Ok, we deleted the sentence, as suggested

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.

We have consolidated our discussion of the loss of additional sink capacity and moved this part to the methods section. This shortened our explanation overall.

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.

Agree. We added a sentence mentioning gross deforestation possibly underestimated by NGHGIs, see above.

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)

Thanks. We added a sentence mentioning gross deforestation possibly underestimated by NGHGIs (e.g. China and India), see above. We also added ref to Winkler et al.

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.

Thanks. We think that the four categories applied here are already a good step forward, and indeed they have been applied also in the Global Carbon Budget 2022 released after we have first submitted our paper. We have thus substantially shortened this paragraph, acknowledging the progress meanwhile made to move BMs' disaggregation closer to IPCC guidelines' categories.

On "other", we agree that the problem may partly be in the system boundaries: for example, NGHGIs are expected to report "cropland remaining cropland" and "grassland remaining grassland" (this is done by most Annex I countries, but only by very few Non Annex I

countries), while BMs only estimate conversions (e.g. cropland to grassland and viceversa). This is partly already discussed, e.g. when we say that "While this may be partly due to the fact that BMs estimate only land-use changes for agricultural lands (e.g., grassland conversion to cropland and not 'cropland remaining cropland'), the large sinks reported by India (for cropland) and China (for cropland, grassland, and wetlands) are not well documented."

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 CO2 concentration increase are accounted as national contributions to carbon sink, but carbon sources enhanced by climate change (e.g. wildfires) were left out.

As explained above IPCC Guidelines do not allow to exclude from the 'reporting' any emissions estimated applying the managed land proxy from the NGHGI - the exclusion could be made by countries during the accounting.

The assumption that the impact of disturbances average out over time is based on the "equilibrium" principle of ecology. No better alternative was found within the IPCC Guidelines. This however may change in the next future and would require additional methodological work by the IPCC.

Nevertheless, to clarify that emissions are not discretionally excluded from NGHGIs, the following revision is implemented: "It is important to note that these natural disturbance emissions and subsequent removals are excluded from the accounting, but are reported in the NGHGI"

Line 590-592: completely agree with this.

Thanks

Finally, it should be noted that we updated the results to make it consistent with the Global Carbon Budget 2022 (at the time of the original submission, only GCB 2021 data were available). While results changed a bit (i.e., now we have a better match between BMs and NGHGIs at the level of LULUCF, but a less good match for forest land and deforestation), the message of the paper does not change.