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Earth System Discussions

Science Data

Interactive comment on "Carbon Emissions and Removals by Forests: New Estimates 1990–2020" by Francesco N. Tubiello

Marieke Sandker (Referee)

marieke.sandker@fao.org

Overall assessment The topic of this paper is highly relevant and timely. The paper is well written and the comparison with GHG inventory reporting under the UNFCCC is useful as well as the reference in the discussion to the difference between anthropogenic and natural fluxes. This work is trying to fill an important data gap with high uncertainties. The methodology section would benefit from a more detailed description on how the data was obtained and how missing data was treated. Finally, some concerns exist concerning the assessment of removals/emissions in remaining forest land. This may merit further elaboration in the discussion perhaps also comparing with studies assessing natural fluxes (i.e. all fluxes), in addition to the comparison with only anthropogenic fluxes in the GHG inventories.

C1

Overall comments Equation 1 suggests carbon stock changes in forest land remaining forest land are calculated comparing country reported carbon stocks for different years. Are national correspondents aware that the estimates provided here would be used to assess emissions/removals from forest land remaining forest land? Would it not be more straightforward to ask countries directly whether they can report on the change of their country's carbon stocks in forest land remaining forest land (instead of asking them to report carbon stock in different points in time)? For example: if a country has 100,000 ha forest in the year 2000 of which 90,000ha is primary forest and 10,000ha is secondary forest, and in 2010 that country has 95,000 ha of forest if which 90,000 primary forest and 5,000ha secondary forest, the average carbon stock in the forest in time 2 has gone up but it could be incorrect to interpret this as removals in forest land remaining forest land? Could this be a concern?

In addition, many countries will not have multiple assessments and therefore report carbon stock only for one year without this meaning the flux in forest land remaining forest land being zero – on the other hand countries may have estimates for different years using different methodologies making them not directly comparable. Looking into the spreadsheet, DRC's forest land remaining forest land has zero emissions/removals. This seems highly unlikely? DRC's GHG inventory seems to suggest F>F to remove approx. 50 million tCO2eq/yr? How does the study deal with these limitations?

Pan et al 2011 suggests forests globally to on average remove 4.0 bln tCO2eq, this study suggests net emissions of 0.4 bln tCO2eq - that's a 4.4 billion tCO2 gap. Could it be that the above mentioned issues (countries lacking data on evolution of carbon stock + interpretation of stable reported values as zero fluxes) contribute to the explanation of this gap?

What is the data input for deforestation area estimates? Are you using country reported deforestation numbers gap-filled with negative net forest area changes in case countries did not provide deforestation estimates? If so, it would be good to describe this in the methodology. If not, why is this not used?

C2

It seems removals from non-forest land converted into forest land are not estimated. Why not? I have not seen it explained in the article – it would be good to highlight this such that the reader is made aware this flux is missing?

Detailed comments Line 43/44: Estimates of CO2 emissions and removals from forest land were computed following the carbon stock change method of the 2006 IPCC (2006) guidelines, Vol. 2 and 3, at Tier 3, approach 1 (Federici et al., 2015; FAO, 2020a). ïČŸ Is this an accurate description of the methodology applied? According to Jim Penman, the stock change method requires repeated field measurements – very few countries would have this. Any form of AD x EF is considered gain-loss https://www.reddcompass.org/uncertainty?uri=_Toc372288937.html%23_Toc372288937&ve mgd-content Wouldn't the approach rather be a mix of stock change and gain-loss? ïČŸ Wouldn't the assessment be better described as Tier 1 level, or at best Tier 2 since country estimates are used, rather than Tier 3? Perhaps a model was applied here but Tier 3 does also imply an increase in accuracy so perhaps a different type of model is implied for Tier 3. Line 60: it would be helpful if the paper would explain what is meant with net deforestation (net area change? excluding temporary tree cover loss? considering the carbon contents in the replacing landuse?) Line 88: Should this be forest area flux? Line 235: First, the good agreement between the FAO estimates and country reports implies that the definition of forest land use underlying both FAO and UNFCCC reporting was consistent, i.e., all managed and hence the emissions were considered all anthropogenic. ïČŸ Can this truly be concluded? As aggregate values the reported emissions/removals were less than 15% different but what were the differences at country level, were these not much larger? Even if they were comparable I still wouldn't conclude from it they are both managed. In theory with the FRA forest area you calculate using the full forest extent without making a distinction between managed and unmanaged so why conclude this? Editorial: Line 51: as show in more detail below > as shown in more detail below Line 77: . . .on forest land proper, and from deforestation. ïČŸ I don't understand this Line 90/91: over or underestimates > over or underestimated Line 93/94: (which is the primary variable measured, from

which total carbon stock is obtained by are multiplication) ïČŸ I don't understand this Line 225: For Indonesia, the new FAO estimates (as well as those based on the FRA 2015) had greatly overestimated country reported data for 1991-2000 ïČŸ Do you mean to say FAO estimates were much higher than GHG inventory reported data? Harmonize the 1991-2020/1990-2020 period annotation