

Review of Tubiello et al. v2

The manuscript has been greatly improved with regard to: (i) a more detailed methods description, (ii) a clearer discussion of data limitations and known issues (see details below), and (iii) a more disaggregated comparison with other global datasets, at both global and country levels. In addition, the results now include all carbon pools, not only carbon in living biomass as in the previous version. I commend the authors for these important improvements made.

However, some aspects are still worth improving.

I previously recommended a “basic evaluation at country level” before presenting aggregated results, using the examples of DRC, USA and Italy. The DRC case (constant values of carbon density over time, representative of about 90 other countries) has now been adequately discussed.

The case of the USA should be better discussed. The USA’s FRA report mentions that “No forecasting was used. 2022 data were applied for FRA 2025 reporting.” However, to my understanding, FAOSTAT computes the CSC for 2020–2025. I proposed an adjustment to the calculation, or alternatively: “If the authors choose not to adjust the calculation method to reflect this explicit information in the FRA report, the implications of this methodological artefact should at least be very clearly explained and discussed in the manuscript”. The implications include an artificial decline of the sink in the affected countries (not only the USA, I think). While it is now written in Tab.1 that “The USA had not yet reported for 2023”, to my understanding the implications explained above are still not clear to the reader.

For Italy, I had noted a clear inconsistency in the original country report to FRA. Specifically, “while total forest biomass is reported to increase from 2020 to 2025, total carbon stock is reported to decrease over the same period. Given the close and well-established relationship between biomass and carbon, this discrepancy strongly suggests an error in the compilation of the carbon stock table.” The authors replied that “Italy’s values had been wrongly read in a first part of the work (prior to Oct 2025) from the FRA original dataset and have been recalculated.” However, when checking the Zenodo file or the FAOSTAT online dataset, the problem I noted for Italy remains. It is a significant issue for Italy (ca. 75 MtCO₂/y) and for any analysis of FAOSTAT data for the EU. I understand that the issue originated from Italy’s original submission to FAO FRA.

If the authors cannot resolve the cases shown for the USA and Italy, I recommend that these specific cases be mentioned in the paper (e.g. in Section 2.3) as examples of data limitations that may affect the results.

Additional points:

Table 3: please check Russia. The latest Russian NGHGI gives 739 Mha of managed forest for the year 2023, not 900 as reported in the table – the corresponding text should also be adjusted.

Please add another digit in Tables 2 and 3.

Line 152: “boundary issues related to smaller reported managed forest area compared to total forest land area were limited to a few countries,” and line 256: “We suggest that, based on actual country data comparisons, the boundaries of FAO estimates and UNFCCC data were in fact greatly aligned.” This may be true at the global level, but for the affected countries the difference in area is a non-negligible factor explaining the difference in C fluxes. Please consider this.

Section 2.3 on strengths and limitations greatly improves the paper. I suggest adding, among the limitations, the fact that FAOSTAT provides 10- or 5-year averages and that data are updated only every 5 years, i.e. when new FRA reports become available.

Section 3.1.1: The decline of the FL sink over time can be partially attributed to incomplete reporting of C stocks in more recent years (e.g. USA). I think this is worth noting.

Table 4: to help readers, please explain in the table caption what FAO models mean and define CSC, NFC and FL. Also check the numbers: CSC should be $NFC + FL$, but this does not always seem to be the case (even considering rounding).

The difference between FAO models is occasionally very large. For example, for FL NAI, FAO1 is -1.4 and FAO3 is -2.9 , suggesting that half of the forest sink is in non-biomass pools. This is worth noting in the discussion, also in relation to the gap-filling applied for soils (line 281).

Figure 3a: please explain LMTD.

Figures 3b and 3c: please spell out AI and NAI.

Line 467: Melo 2026 uses “not plausible” rather than “not credible”, based on the level of sink per unit area in three countries (Mali, Malaysia and Namibia), i.e. a subset of those in Table 8. Please clarify this.

Figure 5: terminology needs some adjustments. I suggest using “deforestation” (instead of NFC) for GCB and JRC, and explaining in the table caption that NFC is a proxy for deforestation. “FL GCB” is actually FL GCB “translated” or “NGHGI-aligned” (using methods described by Melo et al. 2026) – this is crucial information to add. Inversions do not measure “net land carbon stock change” but rather a “net C flux”; there could be differences from the CSC approach (e.g. due to lateral fluxes). These clarifications do not undermine the value of the comparisons done, but help avoiding over-simplifications or erroneous interpretations.

Line 514: I would emphasize that “less important” applies to global-level comparisons. For specific countries (e.g. Brazil, Canada), the difference in area is important for explaining the differences in fluxes.

Line 520: GCB “translated” (Melo et al. 2026). This is crucial to avoid misunderstanding.

Line 527: I guess the “LULUCF data hub consortium” refers to the “JRC LULUCF data hub” (Melo et al. 2026).