

Response to Reviewer #1

Note: line numbers refer to those in the track change version of the edited manuscript

1. 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.

ANSWER: We thank the reviewer for this positive feedback and for many useful suggestions that helped further improve the manuscript.

2. 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.

ANSWER: Noted with thanks wrt DRC. For USA and Italy see below.

3. 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

ANSWER: We appreciate the suggestions received and have expanded text in the “limitations” section of the manuscript accordingly. We now highlight in a separate paragraph (see new lines 479-482) the additional uncertainties that are generated by the use of FRA data, highlighting two cases. The first relates to the USA issue reported above, which we amply discuss, providing the USA as example. We also explain that we do not engage in “correcting” FRA data based on additional information that we may

have. The second case relates to possible errors in the input data. In terms of also highlighting the case of Italy, see next comment. Conversely, the comment made by the reviewer wrt Tab.1 is not relevant to this issue, as the text citing USA reporting for year 2023 referred to their reporting to UNFCCC, not the FRA. Nonetheless, this comment highlighted the fact that the note at the bottom of Table 1 was confusing. We have revised the relevant text and moved it directly within the body of the manuscript

4. 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.

ANSWER: We thank the reviewer for staying on this point. We have once again reviewed the data for Italy and are now similarly convinced that there is a mistake that needs correction. The issue however is reversed with respect to what highlighted above: the FRA data for biomass C decreased, while the soil C increased. The result is the same as highlighted: a gap of roughly 75 MtCO₂. We have consulted with our FRA colleagues and decided to change the country input carbon stock data, limited to the FAOSTAT estimates. Changes were made based on the biomass DM time series, which was in line with the country’s NGHGI, showing increases during 2020-2025.

In summary, we now discuss explicitly the cases of the USA in the limitation section and corrected the data for Italy. We preferred not to name the country affected directly in the manuscript—although we may publish a country note in FAOSTAT detailing the above changes. We have instead inserted general text about the possibility of errors in the input FRA data (Lines 482-485). At the same time, we have fully revised the Zenodo data with a revision of data for Italy—by carrying forward the 2020 fluxes on removals on forest land and associated carbon stock change, to 2025. This corrected previous discrepancies of roughly 75 Mt CO₂ for FAO1 (noted

by this reviewer), and about 100-110 Mt CO₂ for the FAO2 and FAO 3 estimates. The reviewer was indeed correct that this was necessary, as it greatly affected disseminated data for 2025 for the country of course, as well as for all relevant aggregates of which Italy is part in FAOSTAT, including southern Europe, Europe, EU27, OECD, Annex I—which have now been revised accordingly.

5. 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.

ANSWER: We appreciate the input and have checked, but note that the revision of the Russian data happened within a NGHGI submission dated Jun 25 2026, way after our general cut-off date of Mar 15 2026. We prefer to keep the manuscript with all country data similarly treated, i.e., same cut-off date. We now indicate this cut-off explicitly in the manuscript, see line 150.

6. Please add another digit in Tables 2 and 3.

ANSWER: We appreciate the suggestion but, as stated in the methods section on limitation, 50% uncertainty implies that estimates can be communicated only with one, maximum two significant figures. We use the latter when providing full ranges across FAO models but keep to one significant figure for single results.

7. 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

ANSWER: We agree with the suggested modifications, highlighting however that based on our analysis, it is not only an issue of global signals, but one that the feature does not have extensive global application—regarding a small set of countries. While we highlight that the number of countries with different FAO and UNFCCC forest area is small, we now stress however for those countries it is an important one. See modified text on line 422-425 in methods and lines 609-614 in results.

8. 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.

ANSWER: Thank you for this suggestion. We commented on the timeliness of future updates in the same section, lines 379-383—adding information that the new FRA platform already allows countries to perform updates in their FRA data as needed—so this will reduce the above limitations. At the same time, we note that we had already discussed that the FAO estimates are 10 or 5-year means rather than annual data. Following suggestions by this reviewer, we have made sure in the revised manuscript that this information is well conveyed right after the relevant equations (lines 309-3011), in addition to lines 228-229 and lines 86-287. At the same time, we do not see this issue as a limitation, considering the scatter of annual country data, the underlying measurement cycles in national forest inventories, as well as the underlying uncertainties (lines 405-410).

9. 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.

ANSWER. We are happy to highlight this issue (see line 707-710). We also highlighted that the decline is not statistically significant (line 803-805). In fact (not included in manuscript text), the separate AI/NAI comparisons shown in Fig 3a,b, suggest that both FAO and UNFCCC show a decrease in FL sink strength over the last decade, with the trend difference at global level driven by NAI countries, for which UNFCCC shows a decrease and FAO a flat line. This appears to be an issue linked to the discrepancies of a group of 10-15 countries, already amply discussed in the results section, rather than only a result of the 2020-2025 FAO issue—which as discussed in the limitations, can lead to both under- or over-estimates of sinks.

10. 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).

ANSWER: We have expanded the legend accordingly and adjusted numbers in the table where appropriate.

11. 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)

ANSWER: We have included a sentence on the uncertainty of the soil data in limitations (line 452) and results (lines 956-963)

12. Figure 3a: please explain LMTD

ANSWER: Thank you. We have edited the figure legend for increased clarity.

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

ANSWER: Done

14. 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.

15. ANSWER: thank you for this clarification. We have edited the text c (see lines 870-872).

16. 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.

ANSWER: Many thanks for these clarifications, which have been incorporated in the directly in Figure caption (lines 921-930), as well as additionally described directly in methods, see lines 190-199. in coordination with GCB scientists.

17. 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.

18. ANSWER: Noted and modified text by highlighting the importance for comparisons at country level (see lines 946-949)

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

20. ANSWER: Added specification, see above.

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

ANSWER: Thanks you; adjusted accordingly (line 961).

Response to Reviewer #2

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General Comments

1. This version has much more detail but content is often in unexpected sections. For example, some Results are in the Methods, some Discussion (limitations) is in the Methods. Overall, this manuscript needs heavy reorganization. The amount of material in unexpected places makes it hard to follow the manuscript. Numerous examples are below.

ANSWER. We appreciate this reviewer's feedback. We have significantly re-organized the structure of the manuscript following his/her suggestions.

2. The use of the three FAO estimates is not introduced well and is applied inconsistently. Sometimes you use one, sometimes all three. I understand wanting to use a specific set of carbon pools for different purposes, but which version is used where and why is generally not explained. In some ways, it might be easier if you say in the methods that you calculated these three versions but stick with showing the results for just one of them, except for specific cases where you think another one is more appropriate. Furthermore, I don't think you ever explain how these three versions relate to what is on FAOSTAT or in the FRA.

ANSWER. Based on the good comments received we have decided to use only one version of the FAO models in the manuscript (FAO3, corresponding to all pools), with exceptions where useful. Results from all three models remain available via Zenodo and are also provided as Supplementary Information, where a better description of data availability is provided to the reader.

At the same time, in terms of how the three FAO models were introduced and described, we think that the reviewer was perhaps confused by the original structure of the manuscript, where results provided directly within methods 2.1 section were followed by equation descriptions in section 2.2.

In the current version of Methods 2.1, input data is described at the outset, clearly indicating that carbon stock and carbon stock density data are sourced from the FRA and listing the relevant pools (lines 144). The next Methods 2.2 section emissions immediately follows. In it, the first paragraph provided information on the five pools used in the computations, also indicated in equation (1). Whereas the

three models are clearly described in the last part of this section. We have included this last description as a new paragraph, to ensure readability.

3. I appreciate the comparison with the Global Carbon Budget, but it still leaves much to be desired. At the very least, the specific components of the GCB that are being compared to in lines 494-498 need to be explicitly stated (Eluc, Sland, a component of Eluc, or something else). Even a sentence or two about differences in scope between the GCB and FAO would help. For example, do the GCB estimates you're comparing to include peat emissions and other non-forest CO2 fluxes?

We appreciate the suggestions and have further edited the text accordingly. To this end, we have expanded text in Figure 5 caption and worked on labels, in coordination with GCB scientists. See also additional dedicated text in methods, new section 2.3). Peat emissions and non-forest fluxes were not included (see same section).

Specific Comments

1. Overall, this paper assumes that readers are familiar with FAO data and the FRA that may confuse or deter readers. I encourage the authors to try to lay out the relationship between country submissions to the FAO (and UNFCCC) and what the FAO team does with country data more thoroughly in the abstract, introduction, and methods.

a. The abstract should more clearly articulate the relationship between the FRA and the values presented in this paper. In general, the abstract would be clearer if it started with one sentence of background about the FRA or FAOSTAT.

b. For example, is it correct that countries submit forest area and carbon stock to the FAO (per line 71), but the FAO is the one that calculates emissions and removals (per line 70)? Please clarify which party is doing what.

c. What are the main differences in scope and objective between the FRA data and the NGHGs?

ANSWER. We appreciate the comments and have modified specific section accordingly as follows:

a. The abstract now starts with a description of the FRA/FAOSTAT process (See lines 18-32).

- b. This clarification is provided directly in the abstract (see above). The beginning of the introduction was also edited accordingly (lines 45-53), also clarifying that the forest emission/removals are produced by FAO within its larger efforts to document agrifood systems emissions (lines 65-67).
- c. We appreciate this point, which underscores the need to add a bit more info to the reader that is not working in this field. We have edited the text to describe briefly the process and clarify that countries report to UNFCCC independently of FAO (abstract line 34; introduction, lines 70-75)

2. “the FAO results indicated strong removals on forest land, ranging from -3.6 to -6.0 Gt CO₂ yr⁻¹—with lower and upper boundaries reflecting inclusion of stock change for living biomass only or for all pools. The forest removals were partially counterbalanced by emissions from net forest conversion of 2.8 to 3.8 Gt CO₂ yr⁻¹”: These are net removals and gross emissions, right? Can the difference between them be presented as -0.8 and -2.2 Gt CO₂ yr⁻¹? If so, why not present that range of net values in the abstract? Also, see above comment about how the three FAO estimates are not conveyed clearly. The range of values here (e.g., 2.8 to 3.8) appears to be purely due to the inclusion of different carbon pools. You could potentially present the results as X emissions from biomass, Y from litter and dead wood, and Z from soil, because that’s essentially what the range of values is providing.

ANSWER. We have edited the abstract to present a more structured description of the results, in line with suggestions by this reviewer, i.e., by starting with the net overall signal and then breaking it down into its two main components of fluxes on forest land and net forest conversion. We have eliminated references to the three models in the abstract, in line with suggestions. At the same time we’d prefer not to break down our estimates into pool component contributions, given the overall uncertainty in the underlying data. Rather, as discussed in methods, we use the range of results across the three FAO models as a heuristic indication of uncertainty.

3. Explain why you created your own dataset of country submissions to the UNFCCC rather than using the JRC LULUCF Data Hub (lines 93-100)? What is the advantage of creating a new dataset instead of using the existing, more comprehensive one from the Data Hub?

ANSWER. We appreciate this comment. We would like to highlight that FAOSTAT has disseminated official UNFCCC data across AFOLU categories, including forest land, by country, since 2015. This part of the FAOSTAT emissions database was implemented as a service to user interested in comparing FAO and UNFCCC data across several use cases, including support to NGHGI compilers (see FAO, IFAD IPCC 2015 publication referenced in the main paper). We have slightly modified the initial paragraph in material and methods to clarify this issue. The provision of UNFCCC country data in FAOSTAT is designed as a service to users who might need to compare difference sources or simply as a QA/QC tool for NGHGI compilers. The newer source of compiled (and augmented) UNFCCC data within the LULUCF hub is in fact the JRC NGHGI dataset. We have expanded text to explain why FAOSTAT disseminates UNFCCC data rather than sourcing it from the JRC (lines 155-160).

4. Much of the material in section 2.1.2 (Forest land area) should go in the results, e.g., similarity between FRA 2025 and FRA 2020 (with associated graph) and changes in Argentina's data. Everything between Tables 2 and 3 should be in the results or discussion. (Also, more generally, is it correct that the correlation between forest area submissions to the UNFCCC and forest area submissions to FAO aren't a concern of FAO carbon estimates specifically? This comparison is relevant to FAO in general, not just this paper.)

ANSWER. We have moved this section to results, as suggested.

5. Similarly, much of the material in section 2.1.3 (Forest biomass and carbon stock change) should be in the results in a section that could be called something like "Comparison of FRA 2020 to FRA 2025". What I would expect to see in section 2.1.3 is where the non-biomass data (litter, dead wood, and soil) came from this time, since you said that those other carbon pools are new to FRA 2025. This is covered in a later section but could at least be introduced here.

ANSWER. Explanation of the source of carbon pool data was edited for clarity in the methods section.

6. What are “direct reports” (line 85)? This is an example of this paper assuming reader familiarity with the FRA process.

ANSWER. We agree and have changed the language to indicate official data received directly by the countries—as opposed as gap-filled by FAO (line 114).

7. Table 1 needs all acronyms explained.

ANSWER. The revised manuscript has all acronyms defined within figure captions and in accompanying text.

8. The 1:1 line in Figure 1 doesn’t look like it’s going through (0,0) because it’s intercepting the x-axis and not the bottom-left corner. Maybe this is a trick of log-log axes. If that po indeed (0,0), the figure should have a note confirming that. (I’m likely not going to be the only one confused by it.)

ANSWER. We have drawn a better figure.

9. In Table 2, I don’t understand what the Global-FAO and Report to both FAO and UNFCCC-FAO columns represent and why they’re different. Add a note explaining this.

ANSWER. We apologize for lack of clarity. We have expanded the text in the Table legend and edited graphically the table to improve understanding.

10. Not all terms in equations 1-6 are defined. For example, I don’t see a definition for ΔB . Δp is defined much further after the equations (line 226), complicating interpretation of t

ANSWER . We greatly appreciated this comment and agree that the description of the equations deserved significant restructuring, to improve clarity and readability. For this reason, we separated the text describing the computation of carbon stock change (equations 1-3; see lines 265-305) from the one describing the estimation of emissions removals (equations 4-6; see lines 308-335). In this way, we could expand

the description of all parameters and assumptions used in deriving each set of equations much more clearly, addressing the reviewer's concerns.

11. Why is section 2.3 (Limitations and uncertainty) in the Methods? This would traditionally be in the Discussion.

ANSWER. We preferred to discuss limitations and uncertainty before presenting results, for transparency. While we recognize that some manuscripts may put this section after results, we are likewise familiar with many papers, including in ESSD and especially previous versions of the FAO emission/removal data, which have followed the template implemented herein.

12. The fact that the 2025 data in the FRA is projected by countries needs to be clarified up front in the methods.

ANSWER. We have addressed this issue within the limitations and uncertainty section, in line with similar concerns expressed by reviewer #1. See lines 383-395.

13. "The FAO data showed increasing carbon stocks, hence negative carbon stock change fluxes...", i.e. "The FAO data showed increasing carbon stocks in forests (i.e. net carbon inc

ANSWER. We have edited the text accordingly, see lines 478-487.

14. What FAO scenario number (1, 2 or 3) is associated with -1.8 Gt CO₂ yr⁻¹ on line 331? Table 4 indicates that value is from FAO 1, but the scenario associated with every statistic where the number is used.

ANSWER. We thank the reviewer for these comments. We have checked the text accompanying Tab. 4 and found it adequate, without need for further changes. We believe that further explanations added in methods and the introductory text within the result section now make the interpretation of this table and accompanying text clearer.

15. “the CSC fluxes were consistently negative”, meaning they were consistently sinks? Rather than referring to the flux by sign (positive or negative) in writing, say whether it’s a sink

ANSWER. Thank you for these suggestions. We have edited accordingly, i.e. discussing results in terms of sinks or sources, rather than using signs of CSC fluxes.

16. As with pretty much all other figures and tables, Table 4 needs explanation of the abbreviations used, including what is included in each FAO model and what CSC, NFC, FL, AI,

ANSWER. Explanation of acronyms included in the Table legend—also addressing similar comments from Reviewer #1.

17. There are too many colors for too many lines in Figure 3. All panels of this figure are very hard to understand. Some possibilities include: separating into more panels; dropping from the figure (maybe the less relevant FAO models); categorizing the legend entries more clearly (with category headers); otherwise distinguishing the legend entries better by color thickness. As it is, I can’t tell what this figure is saying

ANSWER. We have re-drawn the figure 3 (a-c) accordingly into one single panel figure, with all acronyms explained within the Figure caption.

18. You sometimes cite Friedlingstein et al. 2025 and sometimes Friedlingstein et al. 2026. Which version of the GCB are you using?

ANSWER. Thanks for catching this. The GCB 2025 paper went from ESSD discussion version (2025) to published (2026) in the course of our manuscript submission and first revision. We have used 2026 throughout.