Earth Syst. Sci. Data Discuss., author comment AC1
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Reply on CC2

Richard A. Houghton and Andrea Castanho

Author comment on "Annual emissions of carbon from land use, land-use change, and forestry 1850–2020" by Richard A. Houghton and Andrea Castanho, Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-351-AC1, 2023

We very much appreciate the thorough reading that Francesco Tubiello gave our manuscript and the many insightful comments he made. Those comments, as well as subsequent exchanges we had with Francesco, helped improve the accuracy and clarity of the paper, and they helped advance my own thinking about a number of issues.

Rather than describe here how we responded to each of approximately 100 comments, we can summarize by saying that we tried to incorporate nearly all of them.

In response to his major points, we noted the two different approaches to estimating emissions from LULUCF and clarified which one we used.

We added a section to the Methods section describing the bookkeeping model, as other reviewers also requested. Readers no longer have to go back to previous papers, especially Houghton and Nassikas (2017). The paper is now nearly 60 pages in length. It had been about 45.

We took pains to distinguish between data from the FAO and "data" we inferred from working with their data. Indeed, we appreciate that a much better paper might result from a more formal collaboration between our carbon modeling and the FAO staff. Such a collaboration might reduce uncertainties considerably. For the purpose of this paper, however, as well as our previous ones, it should be clear how valuable and important the FAO data are.

We added a fourth interpretation of the conversion of forest to other land --- an interpretation that recognizes the possibility of errors in reporting rather than a real change in land use.

We clarified wording throughout the manuscript.

We included a number of additional references.

The paper now has a broader perspective, and is, hopefully, clearer than the one first submitted. Nevertheless, there may still remain some ambiguities and assumptions that Francesco and others disagree with, or even some new errors we've introduced in the revised text. It would be surprising if differences in understanding didn't persist.

Again, these comments from Francesco Tubiello are very much appreciated.

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Reply on RC1

Richard A. Houghton and Andrea Castanho

Author comment on "Annual emissions of carbon from land use, land-use change, and forestry 1850–2020" by Richard A. Houghton and Andrea Castanho, Earth Syst. Sci. Data Discuss., https://doi.org/10.5194/essd-2022-351-AC2, 2023

RC1: 'Comment on essd-2022-351', Anonymous Referee #1, 01 Dec 2022 reply

This manuscript describes a dataset of carbon emissions from historical land use, land-use change, and forestry, presented nationally, regionally, and globally. The dataset is an update to a previously published dataset, and incorporates new data from FAO, along with new model assumptions and processes. The paper is well-written and presented logically.

In addition to describing the specific details and improvements of the dataset, the manuscript has a great description of the many sources of uncertainty in datasets of land-use and related emissions, both generally and for this particular dataset. Specifically, the authors explore how to use and interpret the data from FAO that indicates the area of tropical Forests Converted to Other land (FCO). They employ three alternative interpretations of FCO, with shifting cultivation being the default assumption, and compare the modeled results of each assumption. This is a valuable discussion, along with the discussion of uncertainty in land-use emissions that arise from limitations of limitation of satellite data, and uncertainty that arises from alternative definitions of land-use change emissions and which processes are included.

Overall, this manuscript provides a good dataset description and also provides an excellent discussion of land-use emissions and their related uncertainty. I have a few comments and suggestions for the authors (listed below) but otherwise recommend it for publication.

- The apparent mismatch between net loss of tropical forest and the net increase in cropland and grazing land is described as “forests converted to other lands”. However, perhaps it is also possible that the forest, cropland, and grazing data provided by FAO are just not consistent with one another, and that the differences between net forest loss and net agricultural gain represent the uncertainty or error in the reporting of land-use data to FAO? The reporting of data to FAO also happens infrequently (I think every 5 or 10 years) so it is also possible that the timing of changes in the various land-use categories are not being reported on the same time scales. Has this been considered by the authors?

Author’s response: Yes, indeed. We have added a fourth interpretation for what we call

Forest Conversion to Other land (FCO), an interpretation that considers the possibility that the apparent conversion is really a statistical error in reporting. We discuss two possible errors but argue that one is much more likely than the other.

- Why were changes in fire management considered in the model for only the USA and not for other countries?

Author's response: We have long-term wildfire statistics for the USA but no similar data for other countries. We identify this gap as one that might be filled with further research.

- Why did the area of secondary land used for wood harvest increase in this version of the model code? (Section 2.3.1 and Section 3.1). I can see that this would make a difference to the recovery (and gross uptake of carbon) but it wasn't clear in this section why this change to secondary wood harvest areas had been made in the first place.

Author's response: We have revised the text to try to answer this question. It is really a question of harvest intensity (m^3 of wood harvested per ha). Originally, we had used the same harvest intensity in both primary and secondary forests, but this intensity was unrealistically high for secondary forests and we lowered it.

- Line 406: the cited publication (Friedlingstein et al. 2022) is for the 2021 Global Carbon Budget not the 2020 global carbon budget.

Author's response: corrected.

- Fig 1: the lines are very thin and hard to read

Author's response: redrawn.

- Table 1: what is meant by the "peat-2020" column?

Author's response: we have edited the Table headings to make this clearer.

- Fig 2: the gray lines and black lines and mostly indistinguishable on my screen

Author's response: Figure redrawn for better clarity.

- Fig 3: are the region labels used in this figure (and elsewhere in the manuscript) defined somewhere?

Author's response: we have spelled out the regions and added an appendix that includes the abbreviations as well as the countries in each region.

- Fig 4: lines are too thin, black lines look gray

Author's response: Figure redrawn for better clarity.

- Figs 1, 2, and 4 present similar information, although each is slightly different in order to compare specific changes to model assumptions or data. It would be helpful if the figure captions could more clearly capture or describe the intended message that each figure is trying to convey, to help separate these figures from each other for the reader. In addition, some of the lines in these figures have the same legend labels (e.g. "this study FAO 2021 recovering") but show different data. Is that because some of these figures include emissions from peat, while others don't? Or is there another reason for these differences?

Author's response: we have edited the Figure legends to make clearer the underlying messages.

- Table 2: the caption indicates that emissions for 2 different time periods are included (2011-2020 and 2011-2015) but the later doesn't appear to be shown

Author's response: We have edited the Table and now include only one time period.

- Figure 9: the figure shows emissions from different types of land-use. However, it is not clear if "crop" or "pasture" are emissions from the use of that land or the land-use conversion from some other land to cropland or pasture (which would be a land-use change).

Author's response: we have edited the key in the Figure (and elsewhere in the text) to make clear what the emissions refer to.

- Figure 10: font is too small to read

Author's response: we've increased the font size.

- Table 4: I have the same question as above r.e. types of land use. Also why are some (many) numbers bold?

Author's response: we have clarified the meaning and deleted the bold font.

- Figure 13: the lines are very fine and difficult to read.

Author's response: we have redrawn the Figure to improve it's clarity.

Earth Syst. Sci. Data Discuss., author comment AC3
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Reply on RC2

Richard A. Houghton and Andrea Castanho

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Reviewer #2

This work updated the emissions made by bookkeeping models by Dr. R. A. Houghton and colleagues by using updated land use data from FAO by considering three alternative explanations of excessive forest loss compared with increase in agricultural lands by FAO.

This work is without doubt worthy for publication in ESSD as it is a core dataset underlying the annual global carbon budget made by the Global Carbon Project. But at the same time, I have quite some comments and thoughts which I take chance to discuss with the authors here, which I hope the authors can consider while making revision.

Major comments:

- The bookkeeping approach has been well developed. Because it is a bottom-up approach based on the idea of quantifying anthropogenic effects only, there are no large-scale independent observations to validate this quantification. Despite this is an update that the authors tried their best to make, however, it is also the improvement against which we have no objective standard to benchmark that gives us a chance to verify such an improvement. I think this point needs to be made clear in the paper so that people understand this.

Author's response: we have added text in the introduction to capture this point.

- Related to the comment above, I think the core contribution of this paper is to enhance clarity and provide a one-stop information hub for this dataset and the associate methods, datasets used. The bookkeeping model has a long history (the earliest version was in 1983?). I think it would be useful to make this current paper long but it can be used as a full reference to understand the details of this dataset, so that people don't have to check very often Houghton & Nassikas (2017) and when they refer to that paper, the readers were furthered referred to some earlier papers. Some information, for example, the carbon densities used for different vegetation types, their response curves, could be repeated in the Supplement but they will be useful. It is very

important to have this kind of one-stop information hub for this method.

Author's response: we have added considerably to the methods section to make this a stand-alone paper. We describe the bookkeeping model in more detail, including the response curves. Elsewhere in the text we have been more comprehensive and not simply referred to Houghton and Nassikas (2017) or earlier papers. As a result of these and other additions, the paper is now longer than it was originally.

- For the sake of clarity and traceability. An exact of cross-walking table from land use types in FAOSTAT to the land use types in this study and the associated assumptions need to be provided. The authors simply cited FAOSTAT, 2021. But when readers go to the webpage, it is really unclear which information has been used and how the land cover types were interpreted into the land use types used here.

Author's response: we have added a paragraph that makes clearer the distinction between the FAO data on land use and the rules we used to infer *changes* in land use from these data.

It has to be made explicit that all land use transitions reported by FAO are considered anthropogenic although this assumption is very difficult to verify.

Author's response: we have made the assumption explicit and added a paragraph that considers the consequences if the assumption is not valid.

- The nomenclature or terminology used is sometimes confusing and vague. The most vague one is 'degradation'. The whole block of discussion centering on 'forest degradation' (section 4.2.2) is based on an *undefined* term of 'forest degradation'. If the terminology cannot be clearly defined and made consistent among different studies, comparing different studies won't make things clearer.

Author's response: for the purposes of this paper, we define degraded (whether forest or not) as simply "lower stocks of carbon". We make this explicit and have tried to avoid confusing, vague, and undefined terms.

- What made the authors believe that the adjustments made in section 2.3.1 are necessary and indeed are close to the real world?

Author's response: we have re-written this section to explain the rationale for the improvements to the model.

- Will the authors consider making available the underlying data, e.g., areas of different land use types, carbon densities for different land use types, key parameters for response curves? I believe there are wide interest in using this information from the community.

Author's response: we will make much of the data available at the site indicated. However, there are over 100 countries, 170 years, 20 types of native ecosystems, and response curves for every combination of ecosystem and land-use type. So, for data that are not available at the site, we encourage investigators to contact us with specific requests.

Minor comments:

There seem many of them, but most are demanding clarifications.

Line 36: So what is the difference between LULCC and LULUCF? The readers are expecting explanations but then there are no further explanations. Perhaps lines Line 475-477?

Author's response: Agreed. We have changed the text either to elaborate and clarify or, in this case, to delete this discussion.

Line 50–52: wood harvest is a form of land use but not land use change. If this is the case, then it is not the difference between LULCC and LULUCF? I think the authors are free to choose the name they like but without the explanations that are expected in my previous comment, confusion starts to arise here.

Author's response: Agreed. We have changed the text to elaborate and clarify in another section.

Line 74-76: I don't understand the reason exactly. The reasons listed in the brackets are still vague. Could the authors clarify on this?

Author's response: Agreed. We have elaborated.

Line 114-115: I understand FRA is published every 5 years and the author used FRA2020 to derive change rates for 2015–2019 but they assumed the same rate for 2020. Detailed descriptions would be better. If my understanding is wrong then it justifies the need of detailed descriptions.

Author's response: Yes, this understanding is correct, and we have elaborated the point in the text.

Line 110: Since FAOSTAT 1960 every year the national areas of forest were reported but in lines 62-63 it says the forest area information was not used until 1989. Is my understanding correct? Maybe explain the reasons so that readers won't get confused.

Author's response: Agreed. We have changed the text to elaborate and clarify.

Line 143-145: I would suggest putting this information much earlier than here and maybe in the Introduction. This is a fundamental aspect that the readers need to keep in mind while reading the paper. Its scope is not limited to the title of this section. DONE.

Author's response: Done.

Line 149-151: do these lines continue describing 'degraded lands'? they are not easy to understand. What is the difference between 'permanent croplands' and 'cropland' introduced previously? Or you are describing 'degraded croplands' which seems in a transitional state between cropland and forest? (abandoned but not fully recovered to forest yet). 'Degraded croplands' should have higher carbon density than 'normal cropland' or 'permanent cropland'? I think 'permanent cropland' is the most confusing term here.

Author's response: we have edited the paragraph to provide better clarity.

Line 162: the thing described in this paragraph was called 'shifting cultivation' in Houghton and Nassikas (2017)? I am not sure I am correct. But if I am, then this is in conflict with the next paragraph?

Author's response: the reviewer is not correct. Houghton and Nassikas (2017) did non

include shifting cultivation, per se, but interpreted the conversion of forest to other land through the "recovering" interpretation described here. We've re-written this paragraph to clarify.

I have another question here: In Houghton and Nassikas (2017, GBC) it says "We did not attempt to account for shifting cultivation in this analysis ". So would be nice to explain that you accounted for this because in Table 1 there is such information. In your paper on GCB on negative emissions there it was clearly said shifting cultivation was included. So I guess either it's a wrong citation or you just need to explain. Given the wide influence of your work in the carbon cycle community and the wide usage of this data, it would always be nice to be traceable as far as it allows.

Author's response: Houghton and Nassikas (2018) included shifting cultivation; Houghton and Nassikas (2017) did not. We have tried to clarify in the caption for Table 1 and elsewhere.

Line 166: So what is the standard be counted as 'deforestation'. If it is measured by decrase in carbon density, by my understanding the first class 'degraded croplands ' seem having a lower carbon density than 'shifting cultivation' and hence, conversion from forest to 'degraded croplands' should qualify 'deforestation' as well? CLARIFIED.

Author's response: We have re-written the text to clarify this confusion.

Line 163-173: Does this mean shifting cultivation has a strict rotation length < 5 years?

Author's response: this comment no longer applies to the revised text.

Line 181: what is this FCO?

Author's response: Revised so that FCO should be clear now.

Line 182: a small note explaining that the area of fallow of shifting cultivation was provided in FAO/UNEP (1981) and whether their definition is consistent with yours can be helpful for readers here.

Author's response: Done.

Line 207: Here the fallow lengths are between 2 and 15 years but in Line 163-164 the fallow length is limited to 5 years by definition?

Author's response: Revisions to the text have eliminated this question.

Line 210: need to describe what do you mean by 'traditional shifting cultivation', after all these lengths of descriptions of 'shifting cultivation'.

Author's response: Done.

Line 163: Need to warn the readers about its importance in this paper (i.e. the third interpretation) and briefly explain why, and then warn the readers that a lot descriptions will be devoted to this type so that the readers can be well prepared for the paragraphs that follow and will have better understanding (I hope). I say this basically because the words used for each type of interpretation are highly disproportionate.

Author's response: We warn the reader that this issue is a major thrust of the paper.

Line 187-190: this comparison is really nice and informative. Thanks.

The title of section 2.3.2: might be useful saying directly which is the 'new data from the FAO' by being less ambiguous.

Author's response: Done

Line 227: "the results of the four steps" => the effects of the four steps on the results ?

Author's response: Done

Table 1: Explain the last column so that the table can be independently understood.

Author's response: Done

Figure 2: its caption is confusing with the caption of Figure 2. Do they really have the same caption? At least the lines are quite different. Pls check.

Author's response: Done

Figure 3: 'appeared' in the caption is not exact. What do you mean by this? Is this figure a 'direct' treatment of FAO data, or it is after your interpretation detailed in section 2.3.3?

Author's response: We have changed the text to clarify this ambiguity.

Line 268: "The qualitative results from the three alternatives were as expected if run to equilibrium" => this sentence needs to be expanded to enhance clarity. RE-WORDED.

Author's response: We have deleted this paragraph.

Line 276: higher compared with what?

Author's response: Text has been edited so that this comment is no longer applicable.

Line 268-276: the discussions here needs improving in two aspects: (1) what are the expected carbon densities for the three interpretations ? This information is already needed in section 2.3.3. Without this we don't know what are the expected comparisons among the three interpretations of FCO. (2) based on point 1, what is the expected ranking? This is unclear. Hence it is unclear what has been compared with when stating "expected ranking held" in line 274.

Author's response: We have edited the text and added a Figure to help clarify this issue.

Line 280-284: how is the equilibrium determined? How long these emissions can last? The whole point is about extended emissions when moving into equilibrium but the paragraphs ends on uncertainty, which is confusing for me.

Author's response: We've deleted this point and the comment is no longer applicable.

Table 2: again, explain the last column.

Author's response: Done

Section 3.5: a better more descriptive section title is needed.

Author's response: Done

Figure 6: stacked area figure is not appropriate when there are negative values. Confusing. I would just use multiple panels for different regions. A least it is clearer. Maybe think to combine figure 6 and figure 7 using simple line plot rather than stacked area.

Author's response: We do not agree with this suggestion. While stacked curves may be confusing when the values are negative, simple lines are more confusing because they are not easily distinguished.

Line 330: this justifies my suggestion to merge Fig 6 and 7.

Author's response: We've deleted this point and the comment is no longer applicable.

Section 3.5.2: a more descriptive title could be considered. Of course the whole paper is on emissions by land use.

Author's response: Agreed and done.

Table 5: I don't understand why there is no gross sink in wood products. Not all wood products lead to immediate emissions so there will be some cumulative carbon increase shown as wood product pool compared with a world without any wood harvest. This should be counted as a gross sink, no?

Author's response: Whether slash and wood products are sinks or not depends on definition. Yes, wood products accumulate carbon, but their only effect on the atmosphere is to add carbon. We have added text to make this point explicit.

Figure 9. The legends are confusing. The readers have to guess the meaning. I suppose 'Pasture' and 'Cropland' should represent deforestation or more specifically, forest conversion to cropland, and forest conversion to pasture. But then what is 'fire'? Should we use 'afforestation' or 'plantation'? I guess 'plantation' means there is a net transition in land from agricultural land to forest, but the reason could naturally abandoned cropland. 'afforestation' would be a better name. Here there is no 'forest degradation' which I also have some comments there. If 'forest degradation' cannot be mapped to one of this usages which are relatively clear, are there good reasons we must use this term? For me the land use types shown here are much less ambiguous. AGREED.

Author's response: Agreed. We've changed the key to better reflect the source of emissions.

Line 409: I think BLUE and OSCAR are based on LUH? Because in HYDE there are changes in areas of cropland and grazing land but BLUE and OSCAR need land transitions and these were only available in LUH data.

Author's response: We've added text to make this clear.

Line 423-424: I get confused. Should Feng et al. be compared with net emissions here or gross emissions ? I would argue for net emissions. I suggest the authors clarify on this first before making such comparison.

Author's response: Interesting point. We had assumed that net and gross emissions from deforestation were identical. But it depends on whether the rates of deforestation are net

or gross, and the two studies (Feng versus FAO) might be using different rates of deforestation. We have expanded our discussion to include this possibility.

Line 465-467: all reported changes by FAO are counted as anthropogenic is a key assumption of this paper and should be stated at the early beginning. My second question is, is this assumption valid and why? This question is also relevant to the whole paragraph of 489-497.

Author's response: We agree with this comment. We have edited the text, both to make the assumption explicit and have considered the consequences that it is not valid.

Section 4.2.2 what does the term 'degradation' mean here? Is it wood harvest? Selective logging? Or unknown reason for forest loss or decrease in forest biomass? Is this what is called 'degraded land' in section 2.3.3? are authors in line 579-585 talking about the same thing by using 'degradation'?

Author's response: We have defined what we mean by degradation (lower carbon stocks). And we note that it might be caused by either direct (management) or indirect (environmental change) effects.