

Interactive comment on “The full greenhouse gases budget of Africa: synthesis, uncertainties and vulnerabilities” by R. Valentini et al.

R. Valentini et al.

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Authors' Reply to the Anonymous Referee #2 (please note that the updated manuscript, tables and figures are uploaded in the final comment)

Referee's General Comment

Some sections are more detailed and verbose than others and the information presented in the tables and figures does not always connect well with, or reinforce/support, the information in the text. In addition, when new methodologies are used to estimate fluxes, these methods are not clearly described. The paper would benefit from revision that works to streamline the text and information presented. In addition, the tables can be modified to better support the text and present information across the methodolo-

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gies examined.

Author's reply

The entire paper's content has been revised. Relevant sections have been shortened and relevant tables and figures revised (see the updated manuscript). The explanations of the used methods have been improved (or even added) at the beginning of the relevant sections. See the updated manuscript.

Referee's General Comment

1. The writing style is not consistent throughout the manuscript (i.e., each section reads as if it was written by a different author). The manuscript should be revised to have to ensure a consistent writing style and a better packaging/flow of material.

Author's reply

Once the final version of the manuscript (currently with all the changes tracked) is accepted, the writing style will be revised by an editing professional service.

Referee's General Comment

2. The figures and tables should be revised to better support the information presented in the text. For example, it would be beneficial if, for each method and/or flux presented, the authors included a table that summarized the range of estimates across methodologies and sources. Some tables do this (e.g., Table 5), while others do not (e.g., Table 9).

Author's reply

Some of the figures and tables have been revised according to the suggestions of both Referees. Summary tables were included whenever possible. Nevertheless, in some cases this has been not possible. For instance, for Tables 8 and 9 it is not possible to have satisfactory tables which could include the summary of range of estimates across methodologies and sources. In fact, scale up studies of biological sources and sinks

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of N₂O and CH₄ for specific African ecosystems are not available at the level of detail we present here. Some global modeling exercise has been done at pan-tropical level for humid forests, and more specific studies are available for South American tropical areas. There is a clear lack of scaling up studies for the African continent. Hence we could provide very few values in additional columns, which probably would not be worth. The same situations applies for other carbon-related estimates in Africa.

Referee's General Comment

3. Some Tables and Figures need revision. They are poorly formatted (e.g., large amount of decimal places shown in Tables 1 and 2); the unreadable legend in Figure 2; and the format of Figure 3 (two maps are different size, not clearly labeled, etc.).

Author's reply

As said above, relevant Tables and Figures have been improved (see the updated manuscript). In particular: the decimals in Table 1 and 2 have been reduced; the legend and format of Figures 2 and 3 have been improved.

Referee's General Comment

4. The manuscript is structured so that each section presents results from a different flux (e.g., land use emissions) and/or methodology. However, the level of detail within each section varies. For the most part, but not always, each section starts with a motivating paragraph (or couple of paragraphs) stating the importance of a particular flux in the carbon budget or a basic introduction to a particularly methodology (e.g., atmospheric inversions). However, some sections contain rather lengthy introductions (e.g., Section 3.5), while others are lacking an introduction (Section 3.8).

Author's reply

The manuscript has been updated according to this comment. For example, Section 3.5 was shortened, while 3.8 lengthened by adding few lines of introduction.

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Referee's General Comment

Linking back to comment (1) above, the manuscript should be revised so that each section provides a similar amount of background and detail, presents the results across methodologies/ sources in a similar manner and with a similar level of detail, and that fluxes are presented in consistent/comparable units (e.g., right now the fluxes are presented in units of PgC/yr, tC/ha/yr, gC/m²/yr, TgC/yr, etc.). There needs to be more consistency and continuity across sections. Also make sure that fluxes are presented using a consistent sign convention. Right now fluxes presented for the inversion-based estimates have an opposite sign convention as those presented from the DGVMs. Sign convention should be consistent throughout the manuscript.

Author's reply

In the updated manuscript the consistency and continuity across sections has been improved. For what concerns the units, in most cases we have used Pg C, but in some case (i.e. the land use section) this would have implied too many decimals (0.00000, etc!) so we preferred to use Tg or Mg were relevant. Please note that we have never used t (= tonnes) but just multiplies of grams (P, T, G, M). We do not consider them as different units, they are indeed an international standardized way to use the same units avoiding at the same time too many zeros. In any case, in the last two integrating/summarizing sections (N.5 and 6) we already used Pg only. As for the flux sign convention, we have followed the RECCAP convention: positive correspond to sources to the atmosphere, negative is a land sink. To match this, Fig.5 has been updated. In the previous version NBP was showed using the land sign convention (positive is a sink), but to avoid any contrast with the inversions method, the atmosphere sign convention (negative is a sink) is applied now.

Referee's General Comment

5. When new methodologies and approaches are used to obtain fluxes, these methods need to be better described or articulated within the text. For example, it is not clear

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how land-use emissions (Section 3.6) were derived.

Author's reply

The methodology has been better explained in the text. For further details see the specific replies below.

Referee's Specific Comments

P8346 (Introduction): better articulate how this paper provides an improved estimate of the carbon balance for Africa (third paragraph). In the second paragraph, the authors mention other studies that combine various sources of data and methodologies to get at independent estimates of the African carbon balance. How does this effort improve on these past synthesis efforts? P8347 (Last paragraph of Introduction): State what types of new methodologies are applied in this manuscript.

Author's reply

The results presented in this paper are based on the most recent and comprehensive datasets, derived both from observational and modeling efforts, developed and/or made available under the framework of the REgional Carbon Cycle Assessment and Processes (RECCAP, <http://www.globalcarbonproject.org/reccap>) initiative of the Global Carbon Project (GCP) (Canadell et al., 2011). This is clearly stated now in the last paragraph of the Introduction of the updated version of the manuscript.

Referee's Specific Comment

P8347-8 (Section 2): The title of section 2 suggest that this section presents GHG emissions and removals previously reported by the UNFCCC. However, the first sentence starts with "we have estimated: : ." It is unclear what results this section is presenting. Is it summarizing the results presented in the manuscript or is it summarizing previously reported estimates from the UNFCCC? It is also unclear where the estimates in Table 1 come from. This section (and Table 1) should be better framed within the context of this manuscript.

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Author's reply

Section 2 and Table 1 present a synthesis of the GHG emissions as reported by countries to UNFCCC (http://unfccc.int/national_reports/non-annex_i_natcom/items/2979.php; this link is included in the updated version of the manuscript). In particular we have calculated the GHG emissions on the basis of the national communication to the United Nations Framework Convention on Climate Change (UNFCCC) updated to December 2011. So the original data are provided by countries, but we made the calculations needed to derive the data presented in this paper and we have regrouped them into the different African subregions, etc.

Referee's Specific Comment

P8349 (Section 3): In the last sentence of section 3, the authors state that “a complete and accurate greenhouse gas account for Africa is not yet available.” However, Section 2 appears to present a full GHG account for Africa. This links back to the comment above and the need for better framing of the results presenting in Section 2 and how these results link to and motivate the need for this work.

Author's reply

This sentence refers to the fact that there were not enough data (from both observational and modeling efforts) to derive a GHG budget for Africa with its associated (and reliable) uncertainty. The estimates provided in Section 2 are of some value, but are not based on an independent experimental method. They are national estimations, self-made, so biased by national interests and based on inventories, statistics, emission factors, etc., that give somewhat not very precise estimates. They can give an idea, but cannot be used to “affirm” the Africa's C-balance! We have added one sentence in the updated manuscript, that reflects this explanation.

Referee's Specific Comment

P8350 (line 1): unclear where the “aggregated accuracy” of the land products of 66%

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comes from. This needs to be better explained and defended.

Author's reply

Has replied to the Referee#1, that sentence has been improved in order to clarify the aggregated accuracy: "The accuracy level varies from 67.1% to 70.7% depending on the different land cover products. Details on the methodology to assess the accuracy are given in the Validation Reports". The following links have been added in the updated manuscript:
http://dup.esrin.esa.int/files/p68/GLOBCOVER_Products_Description_Validation_Report_I2.1.pdf
http://due.esrin.esa.int/globcover/LandCover2009/GLOBCOVER2009_Validation_Report_2.2.pdf

Referee's Specific Comment

P8351 (lines 5-9): Unclear how land cover map and aboveground biomass map were integrated (links back to comment 5 above).

Author's reply

The following clarification has been added in the manuscript: "Integration implied the resampling of the data to the higher available resolution and the calculation of biomass statistical values for each land cover class." Together with the other clarification provided to respond to the Referee#1, the general methodology of Section 3.2 should be clearer now.

Referee's Specific Comment

P8352 (lines 8-12): If the biomass from class 40 is significantly different from the other forest classes, then why group 40 with the others?

Author's reply

Class 40 is a closed to open forest class, and has been aggregated with other forest classes as it is still a forest. The aim of the analysis in this paper is to explore the variability inside land cover classes, not to produce a new classification with subclasses.

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Referee's Specific Comment

P8352 (lines 24-25): Unclear how the aboveground biomass of 77.9 PgC is derived.

Author's reply

To clarify it, the relevant sentence has been updated to: "According to this analysis (Table 4), by multiplying the area extent of each class for its mean, it has been possible to estimate a total tropical African aboveground ground biomass of 77.9 Pg C, with different coefficients of variation according to the different land cover classes considered (Table 3)."

Referee's Specific Comment

P8353 (line 6, line 12; etc.): rethink the choices of the use "surface" station here. To some readers this could imply a flux tower measurement rather than an atmospheric CO2 measurement.

Author's reply

The term 'atmospheric' has been coupled to 'surface' in order to clarify that we are talking about the Atmospheric Surface Observations, that differ from the Atmospheric Upper Air Observations. This is part of the overall update of the Section 3.3 as described in the following reply.

Referee's Specific Comments (on section 3.3)

P8353-4 (Section 3.3): This section is difficult to follow. It may be better to put a large amount of the information presented here in a Table (rather than paragraph form) than presents the results from different inversions [e.g., name of inversion, citation, flux estimate with uncertainty, prior used, types of obs used in inversion (e.g., # of towers), atmospheric transport model used, and type and resolution of inversion].

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P8354 (lines 1-5): Better articulate which 5 inversions you are referring to, which 4 solved for grid-based fluxes, and which two were variants of the same system. As written, very vague. Can use the table suggested in comment above to support information presented here.

Author's reply

The whole section has been improved (and a table added) according to the referees comments. See the updated version of the manuscript.

Referee's Specific Comment

P8362-3 (Section 3.6): Not clear how primary forests and land use is documented and determined in this study. Also not clear how carbon emissions from land use is estimated. No clear methodologies are presented.

Author's reply

Primary forests and different land uses were determined according to the national definitions, criteria and assessment methods. The content of Section 3.6, including the methodology, has been improved in order to be clearer and more explanatory.

Referee's Specific Comment

P8364 (lines 10 -12 and lines 25-26): The authors say “we have studied: : ;,” however the results presented are references to other manuscripts. It is unclear what results are new as part of this study and what is from previously published works. This needs to be clarified.

Author's reply

These two sentences are based on the recent results of some co-authors of this paper (including the first author), from the same group, that have still to be published in two different scientific papers (Cazzolla Gatti et al., in review; and Grieco et al. in preparation, to be submitted in the next days). Therefore they are not part of previously

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published works. We have changed the relevant sentences in the manuscript to better clarify the situation.

Referee's Specific Comment

P8364 (lines 16-18): The authors state that "It was observed that the levels of biomass in forests subjected to selective logging are significant lower than those of primary forests.." It is not clear, however, how this was observed. Please clarify.

Author's reply

"African tropical forests biomass was estimated from dendrometric data collected in 150 plots of 500 m² each, subjected to different management (unmanaged/old growth; selectively logged; clearcut). Then they compared the normalized mean level of biomass (divided by area and number of individuals within each plot) of primary forest towards those of selectively and clearcut one. It was observed that even if the levels of biomass in forests subject to selective logging are significantly lower than those of primary forests." This sentence has been added in the manuscript.

Referee's Specific Comment

P8365 (line 16): Reconsider the use of the word "compete".

Author's reply

The entire sentence has been changed to "Fires are additional to heterotrophic respiration and herbivory in consuming carbon that has been sequestered by photosynthesis."

Referee's Specific Comment

P8366 (line 6): What parks?

Author's reply

"Parks" was referred to the protected areas studies in a mentioned paper. The sentence has been changed and the meaning is clearer now.

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Referee's Specific Comment

P8369 (lines 15-16): unclear if the results presented are measured or from modeled estimates. If modeled, how to they compare with observations from gauge stations or from other estimates of water discharge for this region?

Author's reply

The presented results are a combination of using river gauges data (Fekete et al., 2002) and modeling. This has been specified also in the updated manuscript.

Interactive comment on Biogeosciences Discuss., 10, 8343, 2013.

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