

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

Anonymous Referee #2

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Review of “The full greenhouse gases budget of Africa: synthesis, uncertainties, and vulnerabilities.” By R. Valentini et al. bg-2013-39

General comments:

This manuscript reviews the full greenhouse gas budget of Africa synthesizing information from a number of estimation methods including process based models, in situ measurements, and atmospheric inversions. In general, the paper is interesting and addresses relevant scientific questions that fall within the scope of Biogeosciences. It provides more of a review of current and past research and a synthesis across a number of different sources and methodologies rather than the presentation of new methodologies or products, but such a comprehensive collection or review is needed for Africa.

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Given its intent and scope, the paper contains a large amount of data and information and is rather long in length. 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 methodologies examined.

I support publication of this manuscript given that the suggested revisions discussed below are addressed.

Some overarching comments that should be addressed in the review are: 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

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). This would help streamline the text and allow the authors to devote space to comparing fluxes/methodologies rather than presenting lengthy lists of results from various sources/methodologies (which could be done within the tables). See specific comments below.

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.).

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

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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). 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.

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

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 students 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.

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 sen-

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tence 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.

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.

P8350 (line 1): unclear where the “aggregated accuracy” of the land products of 66% comes from. This needs to be better explained and defended.

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

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?

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

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 a atmospheric CO₂ measurement.

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) that 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].

P8354 (lines 1-5): Better articulate which 5 inversions you are referring to, which 4

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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.

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.

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.

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.

P8365 (line 16): Reconsider the use of the word “compete”

P8366 (line 6): What parks?

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?

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

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10, C4517–C4521, 2013

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