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Biogeosciences Discussions

Interactive comment on "The Sub-Saharan Africa carbon balance, an overview" *by* A. Bombelli et al.

A. Bombelli et al.

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Author Reply to the Referee#1 General Comments: This paper “under-utilize” non CarboAfrica results because this is not a review of all current available data on the Sub-Saharan Africa (SSA) carbon cycle. This paper aims at presenting a new overview of the SSA carbon cycle using mainly preliminary CarboAfrica results, and comparing them with previous results and UNFCCC national communications, and sometimes using also non-CarboAfrica results where CarboAfrica results still lack. Therefore we tried to use CarboAfrica data as much as possible. In the abstract the aim of the paper has been re-written as below: “This study presents a new overview of the carbon balance of Sub-Saharan Africa (SSA) by using first results from the project CarboAfrica (namely net ecosystem productivity and emissions from fires, deforestation and forest degradation, by field and model estimates) integrated by available external data, and comparing them with a new synthesis of the data from national communications to UNFCCC.” The focus of the paper has been better

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specified, and the writing improved. The introduction has been simplified and partially rewritten and the logical flow improved as requested by the Referee #1 Technical Corrections 3. The paragraphs sequence now follows this scheme: 1) general short introduction on the carbon cycle and climate change; 2) Africa’s role in the global carbon and climate systems; 3) Land use change and carbon stock and fluxes in Africa; 4) Current gaps and need for new improved data in Africa; 5) why CarboAfrica; 6) aims of this paper. See the Author Reply to the Referee#1 Specific Comment 1 about the reliability of models used. Concerning the uncertainty of fluxes estimates: the NEP estimates come from two different sources: model estimates and in situ data of carbon fluxes or biomass changes. Both model estimation and in situ data are taken from “CarboAfrica” and “non-CarboAfrica” published papers. In this paper we have considered as a major assessment of the uncertainty the variability (standard deviation) across different data sets for each category (model and in situ) rather than the intrinsic uncertainty associated with each single estimate, that come from published peer reviewed papers. For the intrinsic uncertainty one can refer to the individual papers that describe in details their methodology. For what concerns statistical data and their scaling up, it is difficult to assess the variability. However we used as an additional constraint the comparison with previous published data, such as paper of Houghton (2003) for a consistency check of deforestation, and the paper of Lewis et al. (2009) for a consistency check of forest NEP. See also the Author Replies to the Referee#1 Specific Comment 5 and the Referee #2 Specific Comment 13.

Author Reply to the Referee#1 Specific Comment 1: The new biogenic sink value calculated using also the last available UNFCCC estimates is now 0.16, therefore very similar to the previous value of 0.15. We agree that the huge C-sink obtained using the modelling approach is overestimated. These are the probable reasons, that are also stated in the text of the paper: 1) most of the used data are still preliminary 2) the models used were developed mostly for other continents and are not validated against site data for the annual carbon balance of SSA and have not been used in-depth for savanna ecosystems up to now 3) numerous processes that can impede the

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carbon sink are not adequately represented in the modelling (e.g. nutrient limitation, herbivores, indirect effects of fire, etc.) 4) probably the land surface considered as Savanna is overestimated Therefore these models results are highly uncertain and probably overestimated. However all the data we used lead into the same direction: carbon uptake. We are aware that there is a strong need for improving the used field data and the models in particular, and the CarboAfrica project is especially conceived to improve these issues. However, considering that all the 3 different approaches used in this paper go toward the direction of a C-sink, we believe it is worth to show these first results, stimulating the general discussion about the possible role of Africa as C source or sink.

Author Reply to the Referee#1 Specific Comment 2: 0.316 Tg C y-1 is the value for the whole African continent, while 251 TgC y-1 is the value only for SSA, so we use this last value for the SSA budget. This has been better clarified in the revised version. As specified above (see the Author Reply to the Referee#1 General Comments), our estimates are not necessarily better, but - when possible - we used exclusively CarboAfrica results to derive the final C-budget.

Author Reply to the Referee#1 Specific Comment 3: The burned area product used is the L3JRC. The L3JRC is believed to be the best available data set that has been validated and corrected for underestimation for burnt area (Lehsten et al., 2009) and include any estimation of emissions from different vegetation types including cultivation and forest fires (Tansey et al., 2008). The L3JRC product is derived from direct observations of the burnt area rather than relying of proxy evidence from flaming fire detections. Therefore our results might actually be more realistic than previous emission estimates, that have been computed with input data that has been derived by proxy (i.e. active fires) without direct observation of the burnt surface. Moreover, as specified above (Author Reply to the Referee#1 General Comments), we tried to use as much as possible CarboAfrica results to derive a new SSA C-budget. Fuel wood emissions are believed to be not as important in this study and we address the other factors

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raised (deforestation and cultivation). Lehsten, V., Tansey, K., Balzter, H., Thonicke, K., Spessa, A., Weber, U., Smith, B., and Arneth, A., 2009, Estimating carbon emissions from African wildfires, Biogeosciences, 6, 349-360. Tansey, K., Grégoire, J.-M., Defourny, P., Leigh, R., Pekel, J.-F., van Bogaert, E., and Bartholomé, E., 2008, A new, global, multi-annual (2000–2007) burnt area product at 1 km resolution. Geophysical Research Letters, 35, L01401, doi:10.1029/2007GL031567.

Author Reply to the Referee#1 Specific Comment 4: Forest recovery is not included in the models, as no forest degradation due to anthropogenic disturbance in terms of agroforestry or land conversion is included (see also the Author Reply to the Referee#1 Specific Comment 11). The models consider forest recovery in terms of burning, but can be excluded in natural systems as not a feature of forest ecosystems in the tropics. However, emissions from forest fire are included in the L3JRC model used for fire emissions estimates (see the Author Reply to the Referee#1 Specific Comment 3). However the NEP sink of African forest has been recently confirmed by the study of Lewis et al. (2009) published on Nature. They consider also the possibility of a fertilization effect by anthropogenic additions of CO2 to the atmosphere, that is likely to explain the observed enhanced growth. In any case, as explained in the Author Reply to the Referee #2 Specific Comment 1, the value of forest degradation have been reconsidered.

Author Reply to the Referee#1 Specific Comment 5: We are aware that the best way to derive a regional NEP is to use models duly validated by ground data. However, at the current state of the art, models are not ready to reliably represent the SSA C-balance. Therefore we have used the simple scaling up of ground NEP data as a cross check with the current model estimates. Indeed the results show that this upscaling (even if rough and not representative of the full spatial variability of the African landscape) is more realistic than the current first model estimates. This suggest the need for improving the model, that can be used, however, to give an indication about the C-balance trend. This comparison gives also an indication about the uncertainty of current estimates. Nevertheless, to increase the reliability of our results, and to better

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take into account of the spatial variability, we have now re-calculated the NEP of SSA savannas considering each major SSA eco-regions, as defined by Weber et al. (2009) and already used to scaling up model data. As requested in the Referee #2 Summary Comment 3, we have now provided in Figure 1 “a better map showing the vegetation classes used in area-weighting to estimate Africa-wide stocks and/or fluxes”, and we have used it to weight also each of the single in situ NEP data according to their relevant ecoregion.The average NEP value of the three field sites included in the South African grasslands (SAG) ecoregion have been weighted with the SAG area. The same has been done for the NEP values of other ecoregions. Since no field sites are in the Horn of Africa (HA) ecoregion, we have used the average of all sites to extrapolate the HA value.

Author Reply to the Referee#1 Specific Comment 6: We agree that 3.5 Pg C y-1 is a huge value, probably overestimated. Please see the Author Reply to the Referee#1 Specific Comment 1 about the possible reasons. However, even if very high, the models results go towards the same direction of the other approaches, that is a carbon sink. Therefore we believe it is worth to show these first results, stimulating the general discussion about the possible role of Africa as C source or sink. About the the Sahelian recovery, its importance cannot not be supported by the models. Throughout the Sahelian zone no model reports for a positive NEP trend. Positive trends are reported by a couple of models for southern Africa and central tropical Africa.

Author Reply to the Referee#1 Specific Comment 7: We have changed the name of section 3.2 from “1.1 Estimates of NEP of terrestrial ecosystems in the SSA carbon balance” to “C stocks and NEP of SSA terrestrial ecosystems”. The C stocks analysis is relevant to the paper, and can be included in the same section of NEP. Moreover paragraph 6 is preparatory to the following one. Figure 6 (now renumbered as Figure 4) – and the relevant paragraph – have been removed. Section 3.3.1 (including the related text in the methods and results sections) and Figure 5 have been removed.

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Author Reply to the Referee#1 Specific Comment 8: We have added the precipitation term in the equation and explained why we have disregarded them in the text.

Author Reply to the Referee#1 Specific Comment 9: We have now included fossil fuel emissions in the new balance. A comment on the difference between the biogenic SSA C-balance and the total one is now in the Discussion and Conclusions sections.

Author Reply to the Referee#1 Specific Comment 10: We agree and we have produced a new final carbon budget taking into account of all the referee comments!

Author Reply to the Referee#1 Specific Comment 11: In the previous sentences of the abstract we state that the SSA carbon balance is a sink, therefore we believe implicit that it refers to the SSA total net exchange. However we have clarified the sentence. With the sentence “Excluding anthropogenic disturbance and intrinsic episodic events” we meant that the savanna and forest carbon uptake terms are separated from the disturbances (namely fire) one, that is included in the fire terms. We have now simplified the text as below: “The carbon uptake by forests (0.98 Pg C y−1) and savannas (from 1.38 to 3.48 Pg C y−1, depending on the used methodology) are the main contributors to the resulting sink”. The forest recovery is not included in the models, as no forest degradation due to anthropogenic disturbance in terms of agroforestry or land conversion is included. The models consider forest recovery in terms of burning, but can be excluded in natural systems as not a feature of forest ecosystems in the tropics. However, as already explained in the Author Reply to the Referee#1 Specific Comment 4 above the NEP sink of African forest has been recently confirmed by the study of Lewis et al. (2009) published on Nature and can be the result from a fertilization effect by anthropogenic additions of CO2 to the atmosphere. Also a decrease in mortality rates could contribute to this, while an increase in continental-scale disturbance-recovery cycles is less likely.

Author Reply to the Referee#1 Specific Comment 12: We have simplified the relevant section in order to avoid misrepresentation.

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Referee #1 Technical Corrections

1) Author Reply: Text has been shortened to avoid too much technical details as required by the referee.

2) Author Reply: the comment is correct and we have eliminated this sentence from the paper.

3) Author Reply: As explained in the Author Reply to the Referee#1 General Comments, the introduction has been simplified and partially rewritten and the logical flow improved. The paragraphs sequence now follows this scheme: 1) general short introduction on the carbon cycle and climate change; 2) Africa’s role in the global carbon and climate systems; 3) Land use change and carbon stock and fluxes in Africa; 4) Current gaps and need for new improved data in Africa; 5) why CarboAfrica; 6) aims of this paper.

a) Author Reply: we have deleted the sentence.

b) Author Reply: the sentence between brackets has been deleted!

c) Author Reply: this sentence has been cancelled, and also the sentences relevant to the following points d), e) and f) have been cancelled and the structure of this section of the introduction was reconsidered on the basis of the Referees comments.

d) Author Reply: as stated above we have cancelled this (and other sentences mentioned in the points e) and f) below) because not very relevant to the paper purposes.

e) Author Reply: we agree and we have cancelled the sentence. As stated above we have simplified the relevant section in the introduction.

f) Author Reply: as stated above we have cancelled many sentences and reconsidered the organization of the whole introduction and in particular of the section relevant to the comment c), d), e) and f).

g) P. 2089, lines 10 - 15, Vulnerability is not well evidenced by strong sensitivity of

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NEP, and stocks to climate fluctuations insofar as the reservoirs of carbon may simply resonate or fluctuate with the variability. It would be nonlinear or threshold responses in stocks / fluxes as stimulated by variability that would evidence vulnerability. Loose, casual use of the term vulnerability is unhelpful. Author Reply: the referee is right, thus we have deleted this sentence that was inaccurate and not very useful to the paper purposes.

h) Author Reply: the sentence has been changed and the citation removed!

i) Author Reply: We agree that indeed the outcome of rain pulses in savanna ecosystems do not simply results into GHGs pulses, but what we see and need to analyse is an integrated ecosystemic effect, which indeed has not been addressed in this paper and is being addressed only in part in the project. To have a good understanding of the dynamics of GHG pulses, in particolar of N2O, at ecosystem level, and not simply making laboratory tests, the optimal condition would be to have a continuous sampling system for GHGs, soil humidity, temperature, and a frequent analysis of key soil characteristics which influences the variability and magnitude of these pulses, at each Eddy station. Such a coordinated approach requires a significant amount of time and money to be dedicated to each site, however, given that no-CO2 GHGs were for this project a minor task, at least in this first stage of the research, it was not possibile to invest so much effort on this topic. Given that the introduction point out only interesting questions, which are not all necessarily approached in the experimental part, we left the sentence, however modified as following to include the concept expressed by the referee: the frequency and intensity of GHG pulses following rain events and their contribution to the overall annual GHG budget.

Interactive comment on Biogeosciences Discuss., 6, 2085, 2009.