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Biogeosciences Discussions

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

Anonymous Referee #1

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General Comments: The overview paper by Bombelli et al. (BG Discussions) seeks to utilize CarboAfrica results to advance quantitative understanding of Africa’s role in the global carbon cycle. In so doing, their carbon flux accounting under-utilizes non-CarboAfrica results and does little to explain why CarboAfrica results differ from past work and lacks a critical element, namely uncertainty assessment. The paper also lacks focus, presenting material poorly connected to the goal of synthesis. Furthermore, the writing is imprecise in places and some reported values are internally inconsistent.

Despite these concerns, a revised work has the potential to make a valuable contribution by offering new estimates of some of the major terms in an African carbon cycle budget. The paper is relevant to BG and presents novel synthesis, but needs to consider the uncertainty of flux estimates as well as work by other groups to develop conclusions that can be adequately supported. As it stands, the large sink estimate

appears to primarily derive from bottom-up models in which we should place little faith.

Specific Comments: 1) The final budget comes up with a much larger sink than has been previously estimated (0.43 to 2.53 Pg C y-1 compared to 0 to 0.15 Pg C y-1). Why is this so? The authors do not provide a satisfactory mechanistic explanation. There seem to be a few things contributing to this, as explored in comments that follow.

2) The authors seem to suggest in section 3.4 that deforestation emits 0.316 Tg C y-1, but their final budget uses a different figure (0.25 Tg C y-1). In addition, the final budget ignores the other main estimate, Houghton 2003, of 0.37 Tg C y-1, and does so without justification for why their estimates are better. Shouldn’t all of the available data be used unless one identifies a significant concern about procedure or source?

3) This paper’s fire estimate addresses savanna fires only, ignoring fuelwood emissions, shifting cultivation emissions, and deforestation emissions. The latter two may be appropriately omitted if already accounted in other budget terms (unlikely), but fuelwood emissions should be taken into consideration. Past estimates of savanna fire emissions range from 1.09 to 1.67 Pg C y-1, and fuel wood emissions range from 0.08 to 0.24 Pg C y-1 (see Williams et al. 2007). With this in mind, why are the CarboAfrica emissions only 0.79 Pg C y-1, and why are these other estimates discarded? A number closer to 1 Pg C y-1 seems more justifiable.

4) What drives the NEP sink in forests derived from bottom-up models? If much of this is driven by recovery from disturbance and forest degradation, shouldn’t these balance, at least over a large area, long time scale? In the budget they do not balance, with an NEP sink of 0.98 Pg C y-1 and forest degradation of 0.77 Pg C y-1. Is the residual 0.21 Pg C y-1 a fertilization or climate change effect in the bottom-up models? Furthermore, why do the removals exceed the deforestation and forest degradation terms? This may derive from methods rather than processes but there is not enough information and analysis in the manuscript for readers to be able to assess this question. It also raises suspicion about whether forest fire emissions are

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truly accounted in this budget. Also, if the models do not adequately include nutrient limitation, herbivores, indirect effects of fire, etc., are we really ready to have as much confidence in the budget as is purported by the authors?

5) The upscaling of Africa flux towers to a continental NEP flux based on the average across towers in savanna seems foolish (P. 2098 second paragraph). These site-based measurements are doubtful to be representative of the spatial variability.

6) A 3.5 Pg C y-1 sink in African savannas as suggested by the models is very hard to imagine, even if we remove 0.8 Pg C y-1 by savanna fires, which of course is unreasonable in principle because modeled NEP would respond through decreased respiration and/or increased productivity. What processes in the models are causing this huge sink estimated for African savannas? The result is not consistent with vegetation index from satellites, except possibly considering the Sahelian recovery since the 80s. Since so much of the provocative overall net sink derives from this term, explanation is unsatisfactorily absent.

7) The following sections are poorly connected to the synthesis theme / budget calculation and should be either cut or better woven into the broader work: -Section 3.2, paragraphs 3, 4, and 6, and their associated Figures (3, 4, 6) and Table 4. These do not lead to NEP estimates. -P 2107 lines 7 to 13 -Section 3.3.1 and Fig 5

8) The carbon budget equation misses precipitation inputs of carbon that essentially offset and balance the river discharge part of the LT term (see Williams et al. 2007). Both can be disregarded here but if LT is included, so should be precipitation.

9) Why does the budget ignore fossil emissions (˜0.3 Pg C y-1) and only consider emissions from savanna fires (missing fuelwood and forest fire emissions)? Even if it is partly correct to omit some of the forest fire emissions and fuelwood emissions that may be accounted for in other terms (possibly the direct forest fire emissions are in forest degradation and deforestation terms), this should be openly stated and the validity / effect of such assumptions should be discussed.

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10) If the proposed corrections to the budget as mentioned above were included in the manuscript, the new results would follow as:

NEP(F) + NEP (S) + D + FD + EFS + A + Fossil = CB 0.98 + 3.48 -0.32 -0.77 -1 -0.12 -0.3 = 1.95 0.98 + 1.38 -0.32 -0.77 -1 -0.12 -0.3 = -0.15

11) Abstract, “… are the main components of the SSA sink effect.”: The term ‘sink effect’ is misleading in that it is unclear if this refers to the Africa-total net exchange or the sink terms only. How can the exclusion of anthropogenic disturbance and episodic events be reconciled with the sizeable forest sink? Isn’t much of this forest sink the result of recovery from forest degradation? If so, greater clarity is needed here. Otherwise, what explains the forest sink?

12) The introduction misrepresents the fraction of interannual variability in global atmospheric CO2 mixing ratio that is attributed to Africa. This value should be 25% of the variance, where the authors seem to have misunderstood the relationship between standard deviation and variance.

Technical Corrections: 1) page 2093, lines 5 – 10: Too much detail on the N2O and CH4 observations compared to the level of detail on other techniques.

2) Section 2.1.3 Modeling: It seems out of place to mention that … “ For the LPJ-DGVM model, the Pearson correlation coefficient for …”

3) The introduction needs to be logically tightened:

a) Introduction, 2088, lline 25: This last statement about drivers of fire is unclear or imprecise.

b) p. 2088, line 28: the long parenthetical (… particularly with enhancement…) is not helpful and interrupts the flow.

c) Is the anthropogenic impact on irrigation, fertilizer use, agriculture, logging, and fires

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really small across all of Africa? Surely not in the case of modern logging and fire dynamics.

d) P. 2089, line 5: Cite something supporting the claim that anthropogenic pressure on African natural resources is expected to increase.

e) P. 2089, line 6: the logic is weak here. Increased pressure on natural resources is not a core reason for why it is important to quantify the productivity of natural ecosystems and its variability in trends

f) P. 2089, first paragraph: The last sentence is very weak in its logical flow from the previous statements and the next statement. What is the relevance here?

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

h) The use of Scanlon and Albertson 2004 citation is oddly placed.

i) P. 2090, line 5: Arguably we do not need to know about the magnitude and length of the GHGs pulses which follow rain events in dry areas, but rather the integrated effect. Now, if pulses change in frequency or intensity, then the story is different, but this argument has not been made here.

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