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## ***Interactive comment on “Towards multi-tracer data-assimilation: biomass burning and carbon isotope exchange in SiBCASA” by I. R. van der Velde et al.***

### **Anonymous Referee #1**

Received and published: 28 February 2014

I. R. van der Velde et al. present in their manuscript ‘Towards multi-tracer data-assimilation: biomass burning and carbon isotope exchange in SiBCASA’ the further development of the terrestrial ecosystem model SibCASA to account for carbon fluxes from biomass burning and for a full cycling of  $^{13}\text{C}$  in the simulated terrestrial carbon cycle.

With this version of SibCASA the authors address the questions of the variability in the isotopic discrimination and in the isotopic disequilibrium flux. Both are important quantities, which need to be known accurately if measurements of the isotopic composition of atmospheric  $\text{CO}_2$  are used for the inference of oceanic and terrestrial sources and sinks of  $\text{CO}_2$ . The focus here is rather on the disequilibrium than on the discrimination.

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The variability in the discrimination has been investigated in a previous publications by the same others. In order to model the disequilibrium flux it is important to include all relevant processes in the modelling set up. This is one of the reasons for the implementation of the biomass burning process. In general the manuscript is well written but I believe some revisions are necessary for publication in BG.

One of the issues with the manuscript concerns exactly the relevant processes changing the turnover time of carbon in the pools and therefore affecting the disequilibrium flux. The authors quite rightly include biomass burning as one of the processes bypassing the 'normal' way of respiring carbon back to the atmosphere by heterotrophic respiration. Another process, which the authors do not mention at all, is land use change. Land use change emissions are indeed caused by biomass burning, usually clearing of forested areas for agricultural purposes, but a non-negligible part of the effect of land use change is the removal of carbon as products as well as the mobilisation of soil carbon after forest clearings. These processes are not accounted for here in the disequilibrium flux.

Another concern is that the title of the manuscript promises too much compared to the actual content. From reading the title I was looking forward to read the whole manuscript to learn how the authors are going to develop a multi-tracer assimilation system but unfortunately I was heavily disappointed. There is nothing written about a multi-tracer data assimilation system and no real pathway, roadmap or guidelines are provided in the manuscript towards such a system. There is one paragraph in the introduction highlighting the intention to use information provided by the further developed SibCASA model in a multi-tracer data assimilation but this paragraph would be much better placed at the end of the manuscript in an outlook section.

Specific comments:

P 108, L 16: The authors most likely mean plant 'physiology'.

P 109, L 5: The year of publication for the Peters et al. reference is 2012.

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P 109, L 26: Please explain in the manuscript what you mean by 'older carbon'.

P110, LI 10-12: It seems that one of the major findings of the van der Velde et al 2013 paper is that the size of the gap in closing the atmospheric  $^{13}\text{C}$  budget largely depends on the network used to calculate the global  $^{13}\text{C}$  growth rate, at least to the same amount as variability in the biosphere (assuming you mean terrestrial biosphere).

P 112, LI 21/22: Is this a static map of C4 plant fractions? And if so, how do you deal with interannual variability in the extent of C4 plants by e.g. annual grasses?

P 113, LI 8-10: This means you can have changes in LAI which are not supported by the actual amount of carbon in the leaf pool, i.e. the leaves can either become very thin or thick. How does this effect the isotopic discrimination and how large are the errors arising from this inconsistency?

P 115, LI 12-17: Is there any observational evidence to support your hypothesis?

P 118/119: How do you deal with crown fires, which only burn the leaves and do not kill the tree? Quite a substantial amount of the global fires are actually only crown fires.

P 119, LI 9/10: Does that mean you use always the same random year? Please clarify this in the manuscript.

P 119, sec 2.5: How do you spin up the isotopic carbon pools? Are they also in equilibrium in 1851 at the start of the simulation? What are the initial conditions for these pools?

P 123, L 14: Please provide the references for the 'other studies'.

P 124, LI 12/13: Where do the remaining 5% originate from, is this the fraction caused by biomass burning?

P 124, LI 10-15: It is rather interesting that the disequilibrium in the tropics (Itropics) is smaller than in the boreal forest (Iboreal) but the disequilibrium flux in the tropics is more than 4 times larger. Maybe you could elaborate on this.

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P 125, L 15: Which way older or vice versa? Please clarify.

P 128, LI 1/2: Is there sufficient knowledge to include these processes? Are these combustion parameters known well enough to vary them in space and time?

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**BGD**

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