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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.

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First of all we would like to thank the editor and the reviewers for their time and helpful comments. Their thorough analysis of the paper, the comments, and suggestions has helped us to improve and sharpen this manuscript considerably.

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 13C in the simulated terrestrial carbon cycle.

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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 CO2 are used for the inference of oceanic and terrestrial sources and sinks of CO2. The focus here is rather on the disequilibrium than on the discrimination.

We agree with the referee that both discrimination and isotopic disequilibrium are important terms in the atmospheric budget. In our previous study we estimated 45 PgC permil/yr flux caused by direct terrestrial discrimination and 25 PgC permil/yr by terrestrial disequilibrium. The SiBCASA model gives us the opportunity to investigate the full spectrum of terrestrial 13C exchange: the sink part (discrimination) and the source part (disequilibrium, respiration and fires). Because the discrimination is already described by Suits et al. (2005) we focus in this manuscript on the latter part. In the revised manuscript we paid extra attention to the apparent lack of variability in discrimination and in the isotopic signals in respiration. We performed additional sensitivity tests where we enhanced the soil water stress and humidity stress to improve the match with observed respiration signatures. We also extended our analysis on the fast component of C13 exchange. We implemented an additional storage pool in the same manner as proposed in the original manuscript (see Page 6-7, line 25 till line 17, and section 3.3.2).

The variability in the discrimination has been investigated in a previous publications by the same others. In order to model the disequlibrium flux it is important to include all relevant processes in the modeling 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 disequlibirum 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.

The referee makes a fair critique on the absence of land use change (LUC) in this manuscript. We acknowledge that long term LUC can have a quite a large impact on the mean disequilibrium flux, but would mainly affect the long term mean value of disequilibrium rather than year-to-year variability because LUC is often a long term gradual process of emerging C4 vegetation and increase of C4 crop production. Scholze et al. (2008) and Van der Velde et al. (2013) found that discrimination and year-to-year changes in C3 and C4 GPP are the main drivers of IAV in disequilibrium. Therefore, the difficulty to close year-to-year atmospheric budget of 13C in inversion studies (e.g. Alden et al., 2010 and Van der Velde et al., 2013) will probably remain even if we add LUC to our framework. We added an additional paragraph in the Discussion section to highlight the influence of LUC, the removal of carbon for products and we acknowledge that it deserves more rigorous treatment in the future (Page 23, line 7-17). We want to stress that SiBCASA does implicitly account for long-term soil and dead-wood carbon releases following disturbances (by fire).

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 muli-tracer assimilation system but unfortunately I was heavily dissapointed. 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.

We apologize to the referee that high expectations were not met. Originally we had in mind this manuscript would be followed by several other papers where SiBCASA is used in a multi-specie data assimilation system. To avoid further disappointments we changed the focus of this manuscript. It intends to introduce those new additions to the model, including new improvements and sensitivity tests to match observed quantities better. Also we would like to highlight the importance of 13C observations to assess and improve biochemical models like SiBCASA, especially regarding to the allocation and turnover of carbon and the responses to drought. As such we changed the title to: ‘Terrestrial cycling of 13CO2 by photosynthesis, respiration and biomass burning in SiBCASA’.

Specific comments:

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

Yes, thank you for catching that.

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

Thank you. It is corrected.

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

We changed the sentence. Older carbon refers to carbon that has been stored as plant matter being released back to the atmosphere.

P110, Ll 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 13C budget largely depends on the network used to calculate the global 13C growth rate, at least to the same amount as variability in the biosphere (assuming you mean terrestrial biosphere).

This is an important point for which we don’t have a definitive answer. Van der Velde et al. (2013) discusses multiple potential sources that help to close the C13 budget. The atmospheric network is one of the possible sources that hold quite a large amount of uncertainty. However, this uncertainty can go both ways as it can also enlarge the gap in variability budget. Another likely source is isotopic discrimination in the terrestrial biosphere since its interannual variability is still uncertain due to the possible underestimation of drought response in models.

P 112, Ll 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?

We used static C4 map from Still et al (2003). This map represents the C4 plant growth typical in the 1990s. The only variability comes from year-to-year changes in C4 GPP as a result of changes in environmental conditions. These are likely to be a much larger component of isotopic IAV than large scale changes in C3/C4 land-use, as the latter tend to average out as different crops are rotated over different fields.

P 113, Ll 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?

The predicted LAI is always proportional to the amount of carbon in the leaf pool. The leaf pool is allowed to vary freely in response to environmental conditions that affect growth. This prevents the occurrences of negative storage pools (and thus violation the mass balance) as the annual GPP decreases below a minimum to maintain the remotely sensed LAI. However, the magnitude of the leaf pool is constrained by different scaling factors to stay near the values observed in the NDVI record. For example, these scaling factors allow only leaf growth when remotely sensed dLAI/dt is larger than 0. This ensures leaf growth only when observations also indicate leaf growth. Thus making this a semi-prognostic canopy. We changed the sentence in the manuscript to avoid confusion. Since predicted LAI follows closely observed LAI we do not expect a different outcome for discrimination.

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

We guess the referee is referring to page 116, not 115. We agree that the statement is somewhat ambiguous and therefore we dropped it from the manuscript. We meant that the model could not do a smooth transition at dawn and dusk. Such transition was hypothesized during a personal communication between colleagues and was not based on observational evidence.

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.

Similar to Van der Werf et al. (2010) we do not make the distinction between ground

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fires and crown fires. It is true that for the North American boreal region the fires tend to be more characterized as crown fires than for boreal Asia, however such dynamics are not included in the model, and thus an extra source for uncertainty. We mention this in the fires methodology section (Page 12, line 10).

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

No, for each year in the period 1851-1996 we picked randomly a year from the 13-year available dataset. So for instance in 1851 we used meteorology, NDVI and burned area from 2004. The next year, 1852, we used the data from 1999, and so on. We changed the text in the manuscript to clarify this.

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

Yes, the C13 pools are in equilibrium similar to the total carbon pools. We set the time derivatives of the carbon pool equations to zero and algebraically solve for steady state pool sizes given turnover rates for each pool.

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

Fixed.

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

Yes indeed, biomass burning and a small fraction from autotrophic respiration.

P 124, Ll 10-15: It is rather interesting that the disequlibirum 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.

Isotopic disequilibrium flux is the product of 2 quantities: (1) respiration flux and (2) isotopic imbalance between carbon release and uptake. This isotopic imbalance mainly depends on the turnover rate of the soil carbon pools and is especially large in the boreal regions. However, due to more intense plant growth and biological activity we see that the respiration is the most defining factor that determines the magnitude of the disequilibrium. We included a global map of the disequilibrium flux along side the disequilibrium coefficient to highlight the spatial contrast between the two quantities (see Fig. 5a and 5b).