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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.

While this paper is interesting, it feels more like a progress report than a novel scientific contribution. The model development is clear, if not totally novel since there are already other models that include isotopic discrimination and biomass burning, but the application of these improvements to answer scientific questions is less well developed.

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The title promises too much. This research may well be working towards multi-tracer data assimilation, but that is not what is presented here. Better for a title to state what is actually delivered in the paper than what you intend to deliver at some later date. The abstract is also focused too much on what is motivating the questions that aren't answered in the paper than on reporting what actually is.

We find it very unfortunate that the referee was put on the wrong track by the title. The critique on the title is certainly justified. Originally this manuscript was intended as an introduction for several future papers where SiBCASA, with the new model developments (C13 isotope exchange and biomass burning), would play a central role. We changed the focus of this manuscript, which now only intends to introduce those new additions to the model. As such we changed the title to: 'Terrestrial cycling of 13CO2 by photosynthesis, respiration and biomass burning in SiBCASA'. To improve the scientific content we expanded our analysis on the fast component of C13 exchange. We not only propose changes to the model framework as discussed in the original manuscript, we now actually implemented those changes and analyzed them. We also highlighted the importance of 13C observations to assess and improve biogeochemical models like SiBCASA, especially regarding to the allocation and turnover of carbon and the responses to drought. This improved the manuscript and made it all in all a more elaborate study. The abstract is modified to make sure it reflects the contents of the manuscript.

I am not sure that the two parts of this paper come together to form a coherent whole; instead it seems like two separate model improvements that are only somewhat related. It is true that the disequilibrium fluxes are different between the ISOVAR and ISOVARNF experiments, but the same could well occur by, say, changing the turnover time of CWD in the absence of fires. Do you have any observations that could demon-

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strate that the fire fluxes lead to different inferences of the overall C fluxes? The red and green lines in fig. 6 look to have a consistent offset, so there isn't a lot of new interannual variability, in the disequilibrium flux anyway, coming from the fire model. Overall, I would like to see more analysis of what we learn from the model developments here than is currently in the manuscript.

We agree with the referee that in the way it is presented the two model improvements may appear as two separate items, without much mutual association. To overcome this we changed the order of the Results section. In our analysis we split the carbon and isotope exchange in three parts: 13C signatures in atmosphere-biosphere exchange (discrimination), 13C signatures in biosphere-atmosphere exchange (biomass burning and disequilibrium) and evaluation of the model with observations.

A change in turnover of the pools could indeed change the disequilibrium flux, and we do acknowledge that uncertainty in turnover can be high. In our isotopic mass balance study (Van der Velde et al., 2013) we found that the disequilibrium flux could be in reality 20 to 30% larger in order to close the long-term mean budget. If you only slow down the turnover rate, you increase the age of the respiration and therefore increase disequilibrium, but by decreasing the turnover you are also decreasing respiration. So it must be that you need to simultaneously decrease turnover rates and increase pool sizes to increase the disequilibrium flux. However, we want to stress that the turnover rates of the different pools are not simply static prescribed numbers. In SiBCASA they are allowed to vary freely in response to environmental conditions and carbon demand derived from satellite data and meteorology. Comparison to observations showed that predicted wood biomass in SiBCASA lies favorably within 30% of the Forest Inventory Analysis (FIA) (Schaefer et al., 2008). We were reluctant to change any of these turnover parameters because of the complex interplay between pools and fluxes; they have a cascade effect. Changing turnover rates or conversion efficiency may improve the 13C exchange and disequilibrium, but will also alter biomass and fluxes throughout the model, sometimes adversely. Instead we focused on how much fire emissions affect the isotopic exchange. Especially, because fires were never before implemented in SiBCASA and because Scholze et al. (2008) found in the LPJ model a very large change in the disequilibrium of about 10 PgCpermil/yr as a result of fires. The fact that we don't see such a change in our model is mainly because fire emissions from our model are much smaller (and more realistic we believe) than estimated in the LPJ model.

We tried to add more analysis by making a comparison with Ameriflux observations at fire-affected sites (such as Anaktuvuk River, Alaska) to see how fires can affect the other carbon fluxes. Unfortunately, these fires were very local and significant signals did not appear on our 1x1 burned area map.

Based on page 115, line 25, it seems like the C3 and C4 fluxes are combined via a weighted averaging and sent to a single set of C pools for each gridcell. But this only works if the C3 and C4 plants have similar turnover times and responses to time varying perturbations. In practice this would seem to be a bad approach, if that is what is going on in the model, since the first assumption will be violated where C4 grasses and C3 trees coexist, and the second is likely to be violated everywhere given (a) the differing responses to moisture limitation and (b) the long-term differing response to CO2 fertilization, if any, between C3 and C4 vegetation. Moreover, does the overall photosynthetic assimilation differ between C3 and C4 plants in the model?

The referee is correct that the current implementation of a combined C3/C4 carbon pool structure is not ideal when C3 and C4 coexist. However, considering the scales we investigate (globally on 1x1 degree), such violations would only appear strongly where C3 and C4 appear with more or less the same fraction. However, only over 4% of the global land area this is an issue. At most gridcells either C3 or C4 is the most dominating plant type. In addition, there are several biome types (short vegetation), which are designated as C4 biomes, which have specific C4 properties, responses and turnover rates. Over these biomes the C4 fraction is most often the dominating plant type. Lastly, the model includes separate C3 and C4 assimilation parameterization as pointed out in Section 2.1. To be sure whether such combined C3/C4 pool structure is allowed we did a test where we assumed 100% C3 coverage (including C3 responses and turnover rates) at gridcells where C3 fraction exceeds 0.5. Similarly, we assumed 100% C4 coverage where C4 fraction exceeds 0.5. It gives a slight shift towards 4% less C4 GPP than in the combined C3/C4 gridcell simulation. Effects on the isotopic disequilibrium are also small. Mean disequilibrium flux over the last 17 years fluctuated ±1.2 PgCpermil/yr around a mean value of 25 PgCpermil/yr. Very similar to the original simulations.

Page 119, line 27. Need to be clear on terminology. The definition of NEE excludes fire fluxes already, so this compensating offset should apply only to NBP, not NEE.

Thank you for catching that. It is now corrected.

Page 124, line 8-9. Quantify this; I can’t see it in the figure

The variability of the different experiments is given on Page 17 line 14.

Page 126, line 16-23. This seems to be an important implication, that the model is not able to capture variability of C flows at the fast end of the spectrum.

In the new manuscript we expanded the analysis of C and C13 allocation through the storage pool. As explained in detail on Page 6-7, lines 25-17, we made changes in the storage pool in order to capture the fast transfer of isotope signatures during leaf, root and wood growth, together with the fast return transfer of 13C signatures in autotrophic respiration. We added an additional time series plot (Figure 8 in the new manuscript) of VPD, discrimination and d13C of respiration where we compare the old storage pool configuration with the new storage pool configuration. The improved results are now also presented in the VPD-respiration scatter plot (Figure 9b in the new manuscript)

Page 126, line 25 - page 127. I don't follow this argument. What happens to the other 90% of the C?

The 10% - 90% ratio in the old storage pool refers to the sugar portion (the 10%) of the storage pool that is available for plant growth. The other 90% is assumed to be in the form of starch, which is much less readily available for growth. Because the sugar fraction can vary between 5 to 30% depending on the plant species and time of year, an in between value of 10% is chosen based on measurements of Oak trees (see Schaefer et al. 2008). In the new storage pool configuration we separated the sugar storage from starch storage by defining two storage pools. The total amount of storage, the amount of biomass in the pools, and the fluxes remain identical but signals in 13C/12C ratio are now moving much faster from the fast turnover sugar storage to the leafs, roots and wood. This further explained in detail on Page 6-7, lines 25-17.

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Page 127, line 7. Could the inability to match observations in fig. 8b, given the decent match in fig. 8c, be due to too low allocation to quickly-respired leaf C versus other pools?

Inability to capture the fast respiring signals is a byproduct of the way the SiBCASA storage pool is originally designed. Because we assume only 10% of the storage is readily available for growth, the effective turnover rate of the storage pool is not 7 days but is actually 10 times larger, a turnover of 70 days. Of course this smoothes out all the high frequency changes in the 13C/12C ratio of respiration as a result of changes in environmental conditions. The resulting match in canopy respiration (fig. 8c in old manuscript) is because by definition canopy respiration assumes the same isotopic ratio as newly assimilated carbon. This figure is now left out in the new manuscript.

Page 127, lines 17-25. This reasoning is circular. Since you use GFED for the burned areas estimates, they ought to be similar, with only differences between the fuel types available between CASA and SIBCASA as a possible difference. What is the point you are trying to make here?

Although indeed the use of GFED burned area suggests that our fires will be similar to CASA-GFED, we note that total biomass burning emissions also depend strongly on available fuel (biomass) and the assumed burning characteristics (such as combustion completeness which in turn depends on moisture conditions). SIBCASA because of its different photosynthesis model, higher time resolution, and different energy and moisture balance has carbon pools and combustion completeness that are quite different from the monthly CASA model, which is mainly satellite driven. Furthermore,

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as mentioned in the manuscript several other simplifications were introduced in the fire framework we adopted. Because SiBCASA with fires will become our main terrestrial biosphere model for future data-assimilation studies we find it appropriate to present here its performance in comparison to another well-known model.

Fig. 1: This figure would be more useful if it contained mean values of the turnover times of each of the pools, and the respired fraction and transfer coefficients of each of these fluxes.

It is difficult to present the turnover rates in the manuscript for two reasons. 1) The reference turnover rates are not only pool-specific but also biome specific. So in total we have 13x13=169 different turnover rates. 2) The actual turnover rates are determined with the reference turnover in combination with scaling factors for temperature and moisture. As a consequence, the actual turnover rates are not constant but vary in response to environmental conditions and grow demand. For the same reasons we left out transfer coefficients. We note though that the source code for SIBCASA with such details is freely available upon request.

Fig. 4 and 5: Could you show maps of the differences of these values between the ISOVAR and the other two simulations?

Differences appear to be very small between the different experiments. The ISOFIX experiment is dropped all together from the manuscript because it adds only little value to the paper. The only differences we see are increased disequilibrium fluxes in fire affected areas in the ISOVAR-NF experiment. But those differences are already illustrated in Figure 6.

Fig 6: Why are ISOFIX and ISOVAR-NF only shown for 1990-2008 period? Were they run for the whole transient period or only for the latter part?

All experiments were run for the whole period. We present here only the last period to highlight the final period 1991-2008 over which we calculated the interannual variability.

Fig. 7: What would this look like for the ISOFIX experiment – how much is the fit actually improved by the developments on isotopic discrimination here? Also, if you ignore the C4 site, it seems that the main result here is that the SIBCASA is generally underestimating the 13C variability relative to the observations.

In the ISOFIX experiment all simulated signatures are confined in much smaller range from -27 to -26 permil (for C3). The only remaining variable factor that determines the isotopic signature of plants and respiration comes from the atmospheric signature at the moment of assimilation. In the new manuscript we have chosen to drop all the results from the ISOFIX experiment because it does not give much new information. We do underestimate variability in discrimination as mentioned in the manuscript. This is especially apparent in Figure 9, where the correlation between VPD and d13C in respiration improves if we enhance the humidity response (see also Section 3.3.2).

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