Biogeosciences Discuss., 11, C151–C153, 2014
www.biogeosciences-discuss.net/11/C151/2014/

Biogeosciences Discussions
Open Access

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 #2

Received and published: 1 March 2014

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.

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.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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 ISOVAR-NF 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 demonstrate 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.

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?

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

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

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.

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

BGD
11, C151–C153, 2014

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?

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?

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.

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

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?

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.

Interactive comment on Biogeosciences Discuss., 11, 107, 2014.