Biogeosciences Discuss., 8, C2084–C2086, 2011
www.biogeosciences-discuss.net/8/C2084/2011/


Biogeosciences Discussions



# *Interactive comment on* "Novel applications of carbon isotopes in atmospheric $CO_2$: what can atmospheric measurements teach us about processes in the biosphere?" *by* A. P. Ballantyne et al.

**Anonymous Referee #2**

Received and published: 19 July 2011

This manuscripts extends an earlier study (Ballantyne et al., GBC 2010) of (almost) the same authors. The earlier study is, however, not discussed. It is mentioned exclusively to reference that NWR is a good background for the northern hemisphere.

So I have not understood the contribution of the current manuscript. The authors have to work out clearly the novelty of this work compared to the earlier study.

Additionally there are a number of points that should be clarified:

1. It is not discussed that the source signature is a mixture of assimilation and respi-

ration. This is astonishing given the finger-wagging section in the Miller & Tans (2003) paper which is repeatedly referred to.

It is also essential because the disequilibrium will change over season as well, not only humidity.

2. I wondered about the humidity calculations. It should be assimilation weighted in order to do the regression. It should also be in the footprint of the station. What is the footprint of Point Barrow? Especially of the filtered time series for maritime background air? Where and when did you take the humidity in the model?

3. I am really appreciating the method to determine delta_s quasi-continuously. And one extension of the current study compared to the earlier one is the greater number of stations. So it should be warranted that Niwot Ridge is still a good background for Ulaan Uul, for example. This is not discussed. NWR might be o.k. on the same latitude if you consider delta_s to be a monthly mean. But the flying carpet on the NOAA website shows a nice north-south gradient and gradual shifts in the seasonal cycle maxima and minima with latitude.

I would also add caution to the statement that the Miller & Tans (2003) method is an alternative to the Keeling plot. It is an extension. A very good one, though, but it adds a varying background, not more. The rearrangement of the equations does not add anything extra, I think.

4. I think that the intuition in section 5.3 might be misleading. It might come from the implied time scales. Already in the introduction is the funny sentence that stomates "may respond to changes in atmospheric water vapor within weeks". They respond within in minutes, which is the basis of Ball-Berry and Leuning :-)

But on another time scale, humidity goes down over summer leading to soil water stress and reduced stomatal conductance. So you seem to think rather in this time scale. A summer signal is indeed building up in weeks to some month, exactly as in your delta_s.

Plants try to keep their Ci/Ca ratio rather constant if they have enough light and are not water-limited. So it probably goes down with soil water status rather than directly with atmospheric humidity.

A few minor things are:

a) Use a per mil sign in the plots.

b) It is d13C source in the figures but delta_s in throughout the rest of the text.

c) The CO2 compensation point should have star, i.e. gamma* Without the * it does not include leaf/dark respiration.

d) Please be precise that you talk only about NOAA/INSTAAR data. In the introduction are a few paragraphs that make think that Scripps and CSIRO had never existed.

e) What is the line in Fig. 4?

---

Interactive comment on Biogeosciences Discuss., 8, 4603, 2011.