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Biogeosciences
Discussions

Interactive comment on “Use of the isotope flux ratio approach to investigate the $C^{18}O^{16}O$ and $^{13}CO_2$ exchange near the floor of a temperate deciduous forest” *by* E. Santos et al.

Anonymous Referee #2

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This study shows high-frequency observations of stable isotopes in carbon dioxide in a forest canopy for two month. It tries to infer the isotopic composition of soil respiration from these measurements with three different methods. Finally, the study compares the preferred method with some model estimate of the isotopic flux signature.

My major concerns are the following:

1. The three methods are best, i.e. most reliable, at different times of the days or under different micrometeorological conditions. There is no discussion of the strengths and weaknesses of the different methods (there is admittedly a bit about IFR). For the WT method for example: one can safely say that dispersion theory inside the canopy

might be o.k. for neutral and perhaps for unstable conditions; we have not understood it for stable conditions, though. At least this is what Finnigan and colleagues say. It is therefore questionable to apply the WT method under stable conditions, exactly when KP works best. There is no prove that IFR gives actually the right answer under turbulent conditions. The perfect result would have been if IFR compares favourable with KP at stable conditions and it compares well with WT in neutral conditions. u* therefore seems not the right indicator. Why not using L or z/L?

So what to do now? If I am an experimenter: do I use IFR also if I have no laser, i.e. with flasks? Or is IFR only good with a fast laser system? And is IFR good under all conditions? I only have to measure close enough to the ground to have a gradient between the two heights?

2. The comparison with the d18O_R model is quite unsatisfactory. There are so many things that are not discussed but well in the literature. Examples are:

Why is there almost no diel cycle with fCA /= 1? In Seibt et al. (2006) you could well see the diel cycle of almost 10 permil, with CA activity. Do you use a diurnal cycle in temperature?

The soil water d18O, and therefore one of the major inputs, is hard to understand. How comes that DOY 211 is so much lighter at the top? Did you get a very light rain? Why was it so different to DOY 198, which had also a rain event before? What is the isotopic composition of the rain? How good are your fits? Did you check your soil water isotopes with a model? There are quite a few: Isolsm, Musica, Sispat, Soil-Litter-Iso, ...

There is quite a discussion in the literature if the simple model you are using is actually valid or if one has to use a multi-layer model in order to understand the d18O in CO2 signal. The discussion was under the topic "setting point depth" between Riley, Miller, Yakir and others.

So I think the conclusion that fCA was variable during the evaluated period is not very robust, to say the least.

Miscellaneous comments (unsorted):

1. What was modified at Keeling plots? It looks to me that regular Keeling plots are done. The time scale differs from the one in Pataki et al. (2003) and the recommended range is differently. But Keeling did not say anything about it. I find the term "modified Kelling Plot" very misleading.

2. How do you come from 0.035 ppm 13CO2 and a VPDB of 0.011 to a CO2 mixing ratio of 5 ppm? 0.035/0.011 is close to 3.2?

3. Please explain better the observed diel cycle of atmospheric isotopes.

4. Why is z1=1.4 m? The measurement is at 1.4 m and it is not representative for 0-1.4 m. It should be more direction z1=2.8 m (or less).

5. You give error bars for the Keeling estimates but not for the other methods? Any error estimates?

Minor comments are (unsorted):

1. Be consistent with O=16O, C=12C.

2. I think that CA for carbonic anhydrase is well established and there is no need for CAE as further abbreviation.

3. The short names IFR and mKP denote a method while WT denote two authors. WT should be replaced to something like LD to also reflect a method rather than names.

4. Symbols in the appendix are not defined or defined in the main text. This makes it hard to read.

5. Please follow CIAAW recommendations: delta definition equations should avoid extraneous numerical factors, such as the factor of 1000.

6. Griffis et al. (2005b) is no good reference for VPDB-CO2 of 18O. You should emphasize, though, that you use the VPDB of Griffis et al. (2004) for 13C. This is not the "usual" VPDB.

7. Already at the time of Griffis et al. (2004), I was wondering why there is no credit given to Langendorfer et al. (2002) who used a gradient method in a forest with 18O isotopes.

8. Zoom into 13C-plots. Does not have to be the same as 18O.

9. Each plot states that it is Borden, ON, Canada. This might be slightly redundant.

10. Why are the days different in Fig 4 and Fig 5?

11. The scales in Fig 6 and 7 should eb the same for each isotope, i.e. why is Fig 7 going up to +10?

12. Something is fishy with Eq. (4) and I do not know where I need it in the manuscript.

13. What are the numbers -14.64, -7.36 and -1.8 in Fig. 8?

Interactive comment on Biogeosciences Discuss., 8, 7671, 2011.

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