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Biogeosciences
Discussions

Interactive comment on "Diel variations in the carbon isotope composition of respired CO_2 and associated carbon sources: a review of dynamics and mechanisms" *by* C. Werner and A. Gessler

Anonymous Referee #1

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General comments:

The authors reviewed the diel variations of the carbon isotope composition of respired CO2 at plant-organ and ecosystem levels. The manuscript is well written and summarizes the recent developments on the understanding of metabolic origin of respiratory discrimination in plants (but plant and ecosystem are missing in the title) and its diel changes. This review is thus of high relevance for publication in this journal, regarding the importance of this topic in ecosystem carbon partitioning studies. However, the authors should revise the manuscript taking into consideration the reviewer's comments with special attention to 2 major comments below:

1- Although the corresponding literature is mentioned in the manuscript, when reading the manuscript it's sometimes not clear which authors are the first contributors to the recent developments of the mechanisms of respiratory fractionations. In the abstract, where the authors state that "in this review we examine. . . . . .and develop mechanistic explanations. . . . . ", the verb "develop" should be replaced by "discuss" (see the comments below), and the term "mechanistic explanations" is linguistically awkward because explanations are not mechanistic. All the mechanisms discussed in this review are already developed and discussed in the literature, and the present manuscript is a synthetic review. So, the authors should be careful in wording. For instance, in page 2193 (line 23), "we have a conceptual framework" is confusing because the concepts are already published. In many parts of the manuscript, the references should be replaced or added.

2- The hypotheses taken for the equation 1 (model) are not mentioned, the terms of the model are not explained, the same for equation 2. It's not clear why in the equation 1, only one part of the equation is divided by (f1+2f2) and why the f3 does not appear. These equations and the hypotheses of the model and different terms/steps should be clearly defined and explained. The equations 1 and 2 are contradictory, mainly because of the confusion between fractionation factor and isotope effect. The references for different fractionation factors should be indicated in the text or reference to the figure legend where they are given added.

Other remarks:

Page2185-Line 15: Duranceau et al (1999) measured the 13CO2 res at the beginning and at the end of the night period under normal and drought conditions and showed parallel changes in 13CO2 and 13C of leaf sugars during night time (covariations). This should be added to the manuscript in this part (also should be added to the Table 1). Page 2186 – line 2 : . . . .fractionation in particular,. . . . . Page 2187-line 12: one "that" should be deleted. Page 2188-line 18: Initially, Duranceau et al (1999) and Ghashghaie et al (2001) showed short term changes in respired 13CO2

during drought stress. Page 2191-line 16: Add Tcherkez et al (2010) who showed changes through the day. Page 2191-line 25: Add Tcherkez et al (2004) Page 2194-line 19: add “of”:. . . . .in utilization of organic acid pools. . . . . Page 2195-line 1-2: Add Tcherkez et al (2005) to the sentence just after " (KC) are strongly inhibited ". Same page-Line 8: “positive delta13C” is not correct: the authors should say “high delta13C” or “less negative delta13C”. Same page: The findings of Hymus et al (2005) on the relationship between the amount of photoassimilates during the day and the 13CO2resp should be underlined here. Same page Line 23: The verb “focussed” is too strong here! Page 2196-second paragraph: The maximum extent of intramolecular 13C variation observed by Rossmann et al (1991) in glucose molecules (sugar beet) is around 12‰ (not 6‰. Same page-lines 20 and 25: “New” is redundant for NMR analysis mentioned here. Change the wording. Page 2197: Equation 1: d13C-1 should be d13C1 (1 as subscript). Page 2200-line 15: It should be added that earlier, linear relationships between respiration rate and respiratory fractionation were observed during both leaf ageing and drought (Ghashghaie et al, 2003) and with growth (Ocheltree and Marshall 2004). Tcherkez et al (2003) should be added for linear relationship with temperature (line 16). Page 2201-line 26: Please replace “in our example above” by “in Rossmann et al 1991”, otherwise it could be misunderstood by readers. Page 2205-line 2: delete “,” after “new indications suggesting. . . . . ” Page 2206-line 4: delete “s” from components so it should be : component fluxes Figure 1: The total water soluble fraction contains metabolites other than potential substrate for respiration. If some of them are 13C depleted this could explain the dampening of signal. This is not discussed. Figure 2 legend: add “;” before PAR. Brown color of the arrows is not easy to distinguish on this figure ! Part 1 of figure 2: The environmental factors at the top of left side: it’s not clear if the conductances are only affected by VPD and soil water or also light, T etc. The same for A, respiration and photorespiration. All these environmental factors affect all gas exchange parameters. This should be clarified. Figure 5: line 6 of legend: delete “in” Same figure: In both parts of the figure, it’s not clear why the enriched C (both C and delta values) is brown

and not red.

Interactive comment on Biogeosciences Discuss., 8, 2183, 2011.