

## ***Interactive comment on “Comment on “Consistent calculation of aquatic gross production from oxygen triple isotope measurements” by Kaiser (2011)” by D. P. Nicholson***

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### **General comments:**

The author criticizes a major conclusion in Kaiser (2011) that previous estimates of gross O<sub>2</sub> production severely underestimated true rates. Nicholson explains that for the most part the discrepancy between Kaiser's and previous estimates is the result of Kaiser's choice of a photosynthetic end member O<sub>2</sub>, which is too deficient in <sup>17</sup>O. Nicholson states that a correct choice considerably reduces the discrepancy. We agree

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with this statement. Overall this is well written manuscript in most parts that addresses an important aspect of marine biogeochemistry.

### **Specific comments:**

Nicholson suggests that Eq. 6 in the present manuscript is “more precise definition of the original approximate equation for calculating G” (Eq. 3 in the paper that introduced the triple isotope method for estimation gross O<sub>2</sub> production, Luz and Barkan, 2000). However, both equations are identical because parameter  $^{17}\Delta^{BSS}$  used for O<sub>2</sub> at steady state between photosynthesis and respiration is identical to  $\Delta_{max}$  in Luz and Barkan (2000).

A clarification about the near steady-state conditions used for obtaining the biologic end member 249 per meg value in Barkan and Luz (2000), is given in Barkan and Luz (2011), and there is no need to guess how this value was derived. The measurements of  $\delta^{17}\text{O}$  and  $\delta^{18}\text{O}$  of dissolved O<sub>2</sub> at near steady state in both *Acropora* and *Nannochloropsis* were done when  $\delta^{18}\text{O}$  was close to zero. In this special case, regardless of which equation or  $\kappa$  or  $\lambda$  values are used for calculating <sup>17</sup>O excess,  $^{17}\Delta^{BSS}$  is equal to the measured  $\delta^{17}\text{O}$  of dissolved O<sub>2</sub> within the experimental precision. In this respect, the mathematical manipulations done by Nicholson and Kaiser in order to recalculate the value of the biologic end member, does not make sense. Obviously, there is room for improvement but this can be achieved only by more experiments with marine phytoplankton and not by mathematical manipulations of existing data. In this respect, in a manuscript under review in GRL we present new information and suggest a better way for obtaining  $\delta^{17}\text{O}$  and  $\delta^{18}\text{O}$  of the biologic end member.

Nicholson states that photosynthetic oxygen is produced from seawater with only very small fractionation. While for many years this has been a commonly accepted concept, we have recently shown (Eisenstadt et al., 2010; Luz and Barkan, 2011) that this is partly true only for cyanobacteria. Other marine phytoplankton species, as well as oceanic communities, fractionate oxygen isotopes during photosynthesis. The relevant

enrichment in the ocean is on the order of about 4 permil (Luz and Barkan, 2011). In this case the value of  $\delta^{18}\text{O}_p$  should be about -20 ‰ and not -22.853 ‰ as in both Nicholson (2011) and Kaiser (2011). We use this value and calculate  $\delta^{17}\text{O}_p$  in a similar way to Nicholson with Eqs. 4 and 5 as -10.110 ‰. By definition,  $^{17}\Delta^{BSS}$  in this case is 249 per meg and there is no point in recalculating its value.

Following Miller (2002), we recommended using Eq. 2 rather than Eq. 1 for all calculations of excess  $^{17}\text{O}$ . But if for some reason Eq. 1 is applied, then the value of  $\kappa$  should be 0.521 (and not 0.5179 as in Kaiser, 2011) because this is the regression-slope of  $\delta^{17}\text{O}$  vs.  $\delta^{18}\text{O}$  (see Luz et al., 1999). In the same database, the regression slope of  $\ln(\delta^{17}\text{O} + 1)$  vs.  $\ln(\delta^{18}\text{O} + 1)$  is 0.518, which is the preferred value of  $\gamma_\lambda$  in Eq. 2 for calculating  $^{17}\Delta$  of dissolved  $\text{O}_2$  for estimation of gross  $\text{O}_2$  production (Fig. 4 in Luz and Barkan, 2005). However, as explained in Angert et al. (2003), Luz and Barkan (2005) and now in Nicholson's paper, for comparisons of atmospheric  $\text{O}_2$  and photosynthetic  $\text{O}_2$ , the correct value of  $\lambda$  should be calculated with Eq. 17 of Angert et al. (2003) and its value must be smaller than 0.518. A graphic illustration of the difference between these slopes is given in Fig. 2 of Luz and Barkan (2005).

**Technical corrections: (see highlighted text in the attached doc file)**

Page 2 Line 26: Change to "aquarium" instead of "terrarium".

There is a typo in Eq. 5 and Eq. A4: 1 is missing in the denominator of the rightmost part. It should be:  $\dots\dots\dots\ln(1 + ^{18}\epsilon_R)$

Page 5 Lines 20-21: There is a mistake here. Juranek and Quay (2010) correctly used  $\gamma_R = 0.518$ . It is not clear how Nicholson derived 0.5205.

**References**

Angert, A., S. Rachmilevitch, E. Barkan, and B. Luz, (2003a), Effects of photorespiration, the cytochrome pathway, and the alternative pathway on the triple isotopic composition of atmospheric  $\text{O}_2$ , *Global Biogeochem. Cycles*, 17(1), 1030,

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doi:10.1029/2002GB001933.

Barkan E. and Luz B. (2011), The relationships among the three stable isotopes of oxygen in air, seawater and marine photosynthesis. *Rapid Commun. Mass Sp.* 25, 2367–2369. DOI: 10.1002/rcm.5125.

Eisenstadt, D., E. Barkan, B.Luz, and A. Kaplan, (2010), Enrichment of oxygen heavy isotopes during photosynthesis in phytoplankton, *Photosynth Res.*, DOI 10.1007/s11120-009-9518-z.

Kaiser, J. (2011), Technical note: Consistent calculation of aquatic gross production from oxygen triple isotope measurements, *Biogeosciences*, 8, 1793–1811. doi:10.5194/bg-8-1793-2011.

Luz, B. Barkan, E. Bender, M.L. Thiemens, M.H. and Boering, K.A. (1999), Triple-isotope composition of atmospheric oxygen as a tracer of biosphere productivity, *Nature* 400, 547-550.

Luz, B., and E. Barkan (2000), Assessment of oceanic productivity with the triple-isotope composition of dissolved oxygen. *Science* 288, 2028-2031.

Luz, B., and E. Barkan (2005), The isotopic ratios  $^{17}\text{O}/^{16}\text{O}$  and  $^{18}\text{O}/^{16}\text{O}$  in molecular oxygen and their significance in bio-geochemistry. *Geochem. Cosmochim. Acta*, 69, 1099-1110.

Luz, B. and Barkan, E. (2011), The isotopic composition of atmospheric oxygen, *Global Biogeochem. Cycles*, 25, GB3001, doi:10.1029/2010GB003883.

Miller M.F. (2002) Isotopic fractionation and the quantification of  $^{17}\text{O}$  anomalies in the oxygen three-isotope systems: An appraisal and geochemical significance. *Geochim. Cosmochim. Acta*, 66, 1881-1889.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/8/C2990/2011/bgd-8-C2990-2011->

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supplement.pdf

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Interactive comment on Biogeosciences Discuss., 8, 7127, 2011.

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