

## Author response to reviewer comments (B. Luz, Referee)

I would like to thank Dr. Luz for his helpful comments. My replies are written in black below each comment.

### 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 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 <sup>17</sup>Δ<sub>BSS</sub> used for O<sub>2</sub> at steady state between photosynthesis and respiration is identical to <sup>17</sup>Δ<sub>max</sub> in Luz and Barkan (2000).

I agree, I will change the wording to say "more descriptive" equation. My point is to highlight that <sup>17</sup>Δ<sub>BSS</sub> is the appropriate term here, not <sup>17</sup>Δ<sub>p</sub><sup>#</sup>.

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 δ<sub>17</sub>O and δ<sub>18</sub>O of dissolved O<sub>2</sub> at near steady state in both Acropora and Nannochloropsis were done when δ<sub>18</sub>O was close to zero. In this special case, regardless of which equation or κ or λ values are used for calculating <sup>17</sup>O excess, <sup>17</sup>Δ<sub>BSS</sub> is equal to the measured δ<sub>17</sub>O of dissolved O<sub>2</sub> within the experimental precision.

I will note that Luz and Barkan (2011) clarify that the original 249 per meg value was measured under BSS conditions at δ<sub>18</sub>O close to zero. At 0 per mil, the choice of slope (κ or γ<sub>R</sub> or λ<sub>BSS</sub>) is irrelevant.

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.

I look forward to these new results

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 ‰.

I have added a new section that puts the recent results (Barkan and Luz/ 2011; Eisenstadt et al., 2010; Luz and Barkan, 2011) in context with my discussion. As part of this section I present a 'best estimate' scenario which agrees with the numbers you calculate. Furthermore, I show, as did Barkan and Luz (2011), that using their new measurements of  $^{17}\delta_{\text{sw}}$  and  $^{17}\delta_{\text{sw}}$  along with the newly observed fractionation during photosynthesis one can calculate that is in complete agreement with the original measurement of  $^{17}\Delta^{\text{BSS}} = 249$  ppm.

By definition,  $^{17}\Delta^{\text{BSS}}$  in this case is 249 per meg and there is no point in recalculating its value.

The section recalculating  $^{17}\Delta^{\text{BSS}}$  has been removed, and replaced with the section described in the last comment (now Section 4).

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  $\text{fg}\lambda$  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).

I agree

Technical corrections: (see highlighted text in the attached doc file)  
Page 2 Line 26: Change to "aquarium" instead of "terrarium".

Changed

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

Thank you for catching this error  $\ln({}^{18}\epsilon_R)$  changed to  $\ln(1 + {}^{18}\epsilon_R)$

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.

The value of 0.5205 is correct. This is derived from the fact that J&Q related the respiratory fractionation terms using such that  ${}^{17}\alpha_R = ({}^{18}\alpha_R)^{\gamma_R}$  with  $\gamma_R = 0.518$ .

When this is converted to the correct relationship  $({}^{17}\alpha_R - 1) = \gamma_R ({}^{18}\alpha_R - 1)$ , then

the slope is recalculated as  $\frac{({}^{18}\alpha_R)^{\gamma_R} - 1}{{}^{18}\alpha_R - 1} = 0.5205$ .