

In their reply, Kaiser and Abe (2011) claim that my comment on Kaiser's initial paper (Kaiser, 2011b) "has no merit." However, in the response, the authors have misinterpreted the central point of my reply.

It appears that the critical difference between the argument put forth in my reply, versus the argument of Kaiser is in the interpretation of the biological end member value of 249 ppm reported by Luz and Barkan [2000]. In Kaiser's original base case, he uses this value by making the assignment: $^{17}\Delta_P^{\#}(\gamma_R) = 249$ ppm (i.e. line 1, Table 1). The crux of my argument (Nicholson, 2011) is that this choice by Kaiser was an incorrect assignment and I demonstrate that the appropriate choice for the base case should have been $^{17}\Delta_{S0}^{\#}(\gamma_R) = 249$ ppm instead. In my comment (Nicholson, 2011) I outline how $^{17}\Delta_{S0}^{\#}(\gamma_R) \neq ^{17}\Delta_P^{\#}(\gamma_R)$. The experimental conditions and later clarifications by original authors (Luz and Barkan, 2000, 2011) that were used to determine the value 249 ppm clearly indicate that the value 249 ppm was a measure of $^{17}\Delta_{S0}^{\#}(\gamma_R)$ and not $^{17}\Delta_P^{\#}(\gamma_R)$. The result of this mis-assignment is a systematic bias of ~30% when calculating gross production (g).

- The ~30% bias is an issue of internal consistency of the parameters used to calculate the 'base case' and approximate equations of earlier studies (Hendricks et al., 2004; Juranek and Quay, 2010; Luz and Barkan, 2000; Reuer et al., 2007). It is not 'arbitrary.'
- I recommend that the response by Kaiser and Abe should be revised to address this central issue. Kaiser and Abe say nothing to address why the original base case of Kaiser (2011a) deviates from all of the previous calculation methods by ~30% at neutral, $f = 0$, $g = 0.4$ conditions (Here I am referring to Table 3 and Figure 3 in the Corrigendum, as the original version had an error). These 'neutral' conditions are the conditions under which the approximate equations should perform the best, but Kaiser's original base case appears to contradict this.
- Figure 1 from Nicholson (2011) shows that my revised base case removes this 30% offset as well as reducing the slopes of most of the lines comparing older methods to the 'base case.' The revised base case with $^{17}\delta_P = -11.588$ does a superior job of illustrating biases/errors that arise due to the approximations of earlier equations (e.g. what degree of error is introduced by choice of equation, rather than choice of biological end member). Do Kaiser and Abe agree that this is the case? They do not address this question in their comment.
- In studies using the iterative method (Hendricks et al., 2004; Juranek and Quay, 2010; Reuer et al., 2007), $^{17}\delta_P$ is calculated from $^{18}\delta_P$ and $^{17}\Delta_P^{\#}(\lambda) = 249$ ppm definition such that $\lambda < \gamma_R$ by an amount ranging from 0.0023-0.0025 (See Table 3 in Kaiser's Corrigendum). This has the same effect as using $\lambda_{BSS} < \gamma_R$ when defining $^{17}\delta_P$ of the base case using equation 9 from Kaiser and Abe (2011). Because Kaiser

doesn't do this (instead using $\lambda = \gamma_R$) he is prescribing values that result in an inconsistent comparison.

- As for the 'true' values of $^{17}\delta_P$ and $^{18}\delta_P$, I agree with Kaiser and Abe that there is still a need for inter-lab consistency and consensus. This issue is somewhat complicated by the recent evidence for species specific differences (Eisenstadt et al., 2010; Luz and Barkan, 2011) that Kaiser and Abe review in detail. Discrepancies still are present in the characterization of $^{17}\delta_{VSMOW}$ as well (Barkan and Luz, 2005, 2011).

- The base case I presented is consistent with recent values published by Luz and Barkan (2011) which also uses data from Eisenstadt (2010). The values reported for the average composition across representative species of $^{17}\delta_P = -10.126$ and $^{18}\delta_P = -20.014$. These equate to a value of $^{17}\Delta^{BSS} (\lambda_{BSS} = 0.5154) = 242$ ppm, within error of 249 ± 15 ppm.

- In Section 2.4, Kaiser and Abe report new analytical measurements of $^{17}\delta_{VSMOW}$ and $^{17}\delta_{VSMOW}$. In my opinion, new results such as these deserve to be published on their own, not as part of a 'comment on a comment.' I defer to the judgment of the editor on this issue.

- P 10524 L.1. Barkan and Luz (2011) do mention that $^{17}\delta_{S0}$ was close to 0 w.r.t. air.

"the value of $^{17}\Delta_{bio}$ (249 per meg) in the 2000 paper was derived from O_2 samples in which the $\delta^{18}O$ values were close to that of atmospheric O_2 ." and "when it was close to that of atmospheric O_2 , samples were taken for accurate determination of $\delta^{17}O$, $\delta^{18}O$ "

- The above information contradicts how Kaiser and Abe calculate the $^{17}\delta_{S0}$ and $^{18}\delta_{S0}$ of Acrapora. If $^{18}\delta_{S0}$ was in fact 0‰ at time of measurement, instead of $(-9.16 \pm 0.71)\%$ as calculated by Kaiser, then the resulting $^{17}\delta_{S0}$ would have been 0.252‰ which yields $^{17}\Delta_{S0}^{\#}(\theta_R) = 252$ ppm. Such a scenario would require a large $^{18}\epsilon_P$ ($\sim 10\%$). Given the uncertainty on this issue, it seems equally reasonable to estimate values of $^{17}\Delta_{S0}^{\#}(\theta_R) = 252$ ppm, $^{17}\delta_P = -6.912\%$, $^{18}\delta_P = -13.8\%$ and $^{17}\Delta_P^{\#}(\gamma_R = 0.519) = 276$ ppm. This second scenario should be added to Table 1.

Minor comments:

P 10527, L.8-11. Barkan and Luz do mention that extended storage may have been an issue, so this statement is not entirely accurate.

P 10533, L.23 Change 'centred' to centered.

Table 1:

Table 1: Line 8 of Caption: $17\Delta_{SO}$ (0.521) change 0 to 0 (zero).

Results for $^{17}\delta_W$ and $^{17}\Delta_W$ are listed under the $^{17}\delta_P$ and $^{17}\Delta_P$ columns for lines 5 and 6. Although it is noted in the caption text, I think this choice of organization is quite confusing particularly since there is a fractionation between of ≈ 26 ppm between $^{17}\Delta_W$ and $^{17}\Delta_P$ (Barkan and Luz, 2011). I suggest separate columns for the composition of water.

Table 1: I think this table also would be enhanced by clarifying which quantities are measured, versus calculated, perhaps by making all measured quantities bold.

Table 1: An additional line (6f) should be added to represent the average values for phytoplankton reported by Luz and Barkan (2011). Also, why are the numbers in 6a-e slightly different from those reported in Luz and Barkan (2011)? Is it related to the 5 ppm offset between w and vsmow?

Table 1:, Line 6c: remove "7" from end of *Phaeodactylum tricornutum*

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