

General response

We thank Drs Nicholson and Miller for their detailed reviews of our Reply to Nicholson's Comment on Kaiser (2011b). Our response (in black; reviewers' comments in blue) gives us the opportunity to further clarify some misunderstandings that have arisen from the assignment of $^{17}\delta_P$ and $^{18}\delta_P$ values to the base case scenario chosen in the original paper (Kaiser, 2011b).

Firstly, we are sorry if we misinterpreted Nicholson's Comment and may have therefore wrongly considered it to have "no merit". In addition, we would like to apologise for choosing the phrase "no merit". At the time of writing our Reply, we did not realise that this phrase could be misconstrued as a personal evaluation rather than merely a rejection of the comment.

With the benefit of the present two reviews, we think to have understood our mutual misunderstandings better, as explained in the following. Inasmuch as our general comments do not already address the individual comments by the reviewers, we will follow them up with a point-by-point reply further below.

Just for clarification, we would like to point out that our Reply (Kaiser and Abe, 2011) was based on the version of Nicholson's comment published in Biogeosciences Discussions (Nicholson, 2011a). Since the Reply was submitted and published prior to the final version of Nicholson's comment (2011b), it could not consider any revisions made in the latter version, but we can of course consider them for our revision of the Reply.

We agree with the observation in Nicholson's review that the origin of our mutual misunderstandings is the value of $^{17}\Delta_P^\#(\lambda = 0.5179) = 249$ ppm adopted for the ^{17}O excess of photosynthetic O_2 in Kaiser (2011b). Whereas Kaiser considered it to be a *hypothetical* value (which nevertheless is borne out by experimental evidence), Nicholson took it to mean the *true* (or perhaps best available) value. This misunderstanding originates possibly in the following phrase used in the final paragraph on page 1801 of Kaiser (2011b): "For the purposes of the present study and for consistency with other studies using the triple isotope technique, I assume $^{17}\Delta_P^\#(\lambda = 0.518) = (249 \pm 15)$ ppm."¹

Nicholson may have interpreted the phrase "for consistency with other studies" to mean that the base-case scenario in Kaiser (2011b) had calculation parameters identical or compatible with other studies; in particular, with their corresponding $^{17}\delta_P$ and $^{18}\delta_P$ values. However, this was not the intended. Instead, this particular phrase meant to say that the base-case scenario was constructed using the same (erroneous) assumption other studies had made, namely that the value of (249 ± 15) ppm measured in culture studies by Luz and Barkan (2000) could be adopted for the isotopic composition of photosynthetic O_2 , regardless of the mathematical definition of the ^{17}O excess. Nicholson's misunderstanding was probably not helped by the use of the word "best" in the 3rd paragraph on page 1809 of Kaiser (2011b). This word referred to the lowest measurement uncertainty, not necessarily the highest accuracy.

As a consequence of these misunderstandings, the reconstruction procedure for $^{17}\delta_P$ and $^{18}\delta_P$ from the $^{18}\text{O}/^{16}\text{O}$ ratio of seawater and the ^{17}O excess of photosynthetic O_2 differed between the base

¹ The actual coefficient used was $\lambda = 0.5179$, not the rounded value of 0.518.

case in Kaiser (2011b) and the approach taken by Nicholson (2011b). The base case in Kaiser (2011b) generally leads to higher values of g than the parameters used in the studies of Hendricks et al. (2004), Reuer et al. (2007) and Juranek and Quay (2010)², see Fig. 3 in Kaiser (2011a). However, we continue to disagree with the conclusion of the Comment paper that these higher g values are "too high" because this conclusion is based on a misunderstanding of the nature of the base case scenario and because there is experimental evidence that supports $^{17}\delta_p$ and $^{18}\delta_p$ values leading to higher g values (Kaiser and Abe, 2011). Basically, the uncertainty about $^{17}\delta_p$ and $^{18}\delta_p$ does not allow singling out a certain set of parameters as a benchmark and we therefore refrained from attributing a bias to any particular study.

Furthermore, the title of section 6.2 ("Calculation methods") in Kaiser (2011b) may have been misunderstood as this section being a study of errors solely due to the numerical treatment. In fact, such errors only concern the comparison between dual delta method and approximated calculation using Eq. (1) in Kaiser (2011b) (see also Fig. 3 in the paper). In contrast, the iterative calculation methods adopted by Hendricks et al. (2004), Reuer et al. (2007) and Juranek and Quay (2010) are, for the same input parameters, mathematically equivalent to the dual delta method (disregarding a minor difference due to the use of the actual oxygen supersaturation in the dual delta method instead of the biological oxygen supersaturation advocated by the latter studies). The equivalence of dual delta method and iterative methods was already pointed out in paragraph 3 on page 1806 of Kaiser (2011b). Any differences between these calculation methods are therefore due to the underlying input parameters, and we are in agreement with the reviewers that these need further refinement.

The remaining discussion therefore has to deal with the different choice of input parameters. Our Reply (Kaiser and Abe, 2011) tried to make the hypothetical nature of the base case adopted in Kaiser (2011b) sufficiently clear, but also showed that the base case could be reconciled with $^{17}\delta_p$ and $^{18}\delta_p$ values reconstructed from the $^{17}\text{O}/^{16}\text{O}$ and $^{18}\text{O}/^{16}\text{O}$ ratios of seawater and the photosynthetic isotope fractionation. We apologise if we still did not succeed in this attempt, as many of Nicholson's present comments seem to interpret the phrase "base case" as meaning "best case". We therefore cannot agree with the notion that our base case is due to a "misassignment" and that it is "systematically biased".

As Dr Miller acknowledges in his review, resolving the differences between the various $^{17}\delta_p$ and $^{18}\delta_p$ reconstructions is not helped by the unavailability of the original $^{17}\delta$ and $^{18}\delta$ measurements of Luz and Barkan (2000). This leaves only the triple isotope excess of $^{17}\Delta^\dagger(\kappa = 0.521) = (249 \pm 15)$ ppm (as reported by Luz and Barkan, 2000) to work with. Subsequent studies by Hendricks et al. (2004), Reuer et al. (2007), Luz and Barkan (2009) and Juranek and Quay (2010) adopted the same numerical value, but used different definitions of the ^{17}O excess, either $^{17}\Delta^\#(\lambda = 0.516)$ or $^{17}\Delta^\#(\lambda = 0.518)$. However, due to the unavailability of the original $^{17}\delta$ and $^{18}\delta$ measurements, any set of $^{17}\delta_p$ and $^{18}\delta_p$ values derived from $^{17}\Delta^\dagger(\kappa = 0.521) = (249 \pm 15)$ ppm has to be considered to be hypothetical as well, including the one of Nicholson (2011b). The studies using iterative methods (Hendricks et al., 2004; Juranek and Quay, 2010; Reuer et al., 2007) adjusted the $^{17}\text{O}/^{16}\text{O}$ respiration fractionation ($^{17}\epsilon_R$) such that the steady-state fractionation $^{17}\Delta_{\text{S0}}^\#$ was numerically identical to $^{17}\Delta_p^\#$. To achieve this, $\ln(1+^{17}\epsilon_R)$ had to be equal to $\lambda \ln(1+^{18}\epsilon_R)$. This result-

² The parameters in Juranek and Quay (2010) need to be adjusted as per Kaiser (2011a).

ed in triple isotope fractionation ratios for respiration (γ_R) of 0.5183, 0.5185 and 0.5205, respectively, see Table 3, (Kaiser, 2011a, b), as opposed to $\gamma_R = 0.5179$ (Angert et al., 2003).

Nicholson's Comment correctly points out that in order to create a base case more akin to the iterative studies, Kaiser (2011b) could have used a definition of the photosynthetic isotope excess of $^{17}\Delta^\#$ ($\lambda = 0.5154$), where $\lambda = \ln(1+^{17}\epsilon_R)/\ln(1+^{18}\epsilon_R)$ and $^{18}\epsilon_R = -20$ ‰. As Fig. 1 in the Comment shows, this leads indeed generally to a closer agreement with the other studies, including those using approximated calculations of g .³

Our Reply does not dispute this observation. However, we disagree with the notion that this means that the g values derived from our base case are necessarily too high. They would only be too high if it was certain that the $^{17}\delta_P$ and $^{18}\delta_P$ values assumed by Nicholson are correct, and there is not sufficient evidence to prove this, as we discuss in detail in Sections 2.1 and 2.2 of our Reply. Even though new measurements of the isotopic composition of seawater (Barkan and Luz, 2011) published since the original paper (Kaiser, 2011b) and updated calculations of the resulting $^{17}\delta_P$ and $^{18}\delta_P$ values (Luz and Barkan, 2011) support the reconstruction of Nicholson (2011b), the original measurements of Barkan and Luz (2005) and our own new measurements do not.

Both reviewers commented on the inclusion of new measurements of the isotopic composition of VSMOW with respect to Air-O₂. The original paper (Kaiser, 2011a), the Comment by Nicholson (2011) and the reviews of our Reply (Kaiser and Abe, 2011) agree that additional measurements are required to better constrain the parameters needed for the calculation of g . Our Reply provides such additional measurements and, in our opinion, is an appropriate forum to present them. We also note that Nicholson's final version of his comment (Nicholson, 2011b) and his present review have drawn in new measurements and papers from Barkan and Luz (2011) and Luz and Barkan (2011), which were published after Kaiser (2011a), to help support the claims made in the original comment (Nicholson, 2011a). Ignoring new results that have been generated and/or published since the original paper (Kaiser, 2011a) does not seem to make sense to us.

This is in line with Biogeosciences' editorial policy, according to which peer-reviewed commentaries and replies stand on their own footing. In particular, the description of Biogeosciences manuscript types states, "Peer-reviewed commentaries and replies continue the discussion of preceding papers beyond the limits of immediate interactive discussion. They may be longer and submitted later than the comments exchanged in the Interactive Public Discussion of papers in BGD. They undergo the same process of peer-review, publication and interactive discussion as articles and technical notes and are equivalent to the peer-reviewed commentaries and replies in traditional scientific journals."

Based on this, we have included new measurements of the oxygen triple isotope composition of water, which give a ^{17}O excess for VSMOW in agreement with Barkan and Luz (2005), but not with their more recent paper (Barkan and Luz, 2011). We note is commonplace to find such new results to be included in comments and replies to traditional scientific journals.

³ The labels JQ05 and R07 are swapped in the figure legend.

In conclusion, the reviews of our Reply to Nicholson's comment confirm the usefulness of the dual delta method developed by Kaiser (2011b) and the related method by Prokopenko et al. (2011)⁴ because a large part of the current controversy could have been avoided if $^{17}\delta$ and $^{18}\delta$ values had been reported in addition to (or replacing) $^{17}\Delta$. The reviews once again highlight the need for dedicated measurements of the input parameters $^{17}\delta_p$ and $^{18}\delta_p$ because of the ambiguity and discrepancies associated with different methods to reconstruct them from currently available measurements.

Response to Dr Nicholson's comments

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 is that this choice by Kaiser was an incorrect assignment and I demonstrate that the appropriate choice for the base case should have been $^{17}\Delta_{so}(\gamma_R) = 249$ ppm instead. In my comment (Nicholson, 2011) I outline how $^{17}\Delta_{so}(\gamma_R) \neq ^{17}\Delta_p(\gamma_R)$.

As explained above, Nicholson (2011) has misunderstood the rationale behind the construction of the base case in Kaiser (2011b).

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 $^{17}\Delta_{so}(\gamma_R)$ and not $^{17}\Delta_p(\gamma_R)$. The result of this misassignment is a systematic bias of ~30% when calculating gross production (g).

Our Reply does not exclude the possibility that the input parameters in Nicholson's comment are correct. However, we do not think that the experimental and anecdotal evidence published so far is sufficiently strong to single out a particular set of parameters as a benchmark for others.

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.'

There is no bias here because the input parameters for the base case differ from those used in previous studies. The experimental evidence is currently too ambiguous to single out a particular set of input parameters that would serve as a benchmark for other studies.

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).

⁴ The Prokopenko method is identical, but neglects kinetic isotope fractionation during gas exchange.

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.

The reason that our response did not address this issue is that Nicholson's Comment has already explained the difference. There is no contradiction here; the reason for the difference is that the base case uses different input parameters than the previous calculation methods.

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.

We disagree that Nicholson's figure does a superior job to illustrate biases/errors that arise due to the approximations of earlier equations because the differences are essentially due to the choice of input parameters. All input parameters being the same, iterative and dual delta methods give the same result. We agree that there is bias due the use of approximated calculations based on Eq. 1 in Kaiser (2011b) and have discussed this in detail in Section 3.2 of our Reply, illustrated by Figures 2 to 4.

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.

There is a misunderstanding here. The comparison not inconsistent, it is just a comparison with a different base case. Nicholson may have misunderstood the phrase “for consistency with other studies”, as explained above.

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).

We agree. We would also argue that there may be methodological problems due to the way the photosynthetic isotope fractionation was measured by Eisenstadt et al. (2010). Eisenstadt et al. (2010) themselves point out that their measured photosynthetic isotope fractionation of (4.43 ± 0.01) ‰ for *Phaeodactylum tricornutum* disagreed with an earlier measurement of (0.6 ± 0.2) ‰ (Guy et al., 1993), even though the same helium-sparging technique was used. The attribution of this difference to improved methods for the measurement of the isotopic composition of the substrate water relative to the evolved oxygen is not in line with the small difference between the Dole effect of (23.88 ± 0.02) ‰ measured by Barkan and Luz (2005) and independent estimates of (23.8 ± 0.1) ‰ (Coplen et al., 2002; Kroopnick and Craig, 1972) and (24.36 ± 0.06) ‰ (Kaiser, 2008). Also, theoretical considerations argue for little isotopic fractionation during photosynthetic water splitting (Tcherkez and Farquhar, 2007).

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 $^{17}\Delta_{\text{BSS}}(\lambda_{\text{BSS}}=0.5154) = 242$ ppm, within error of 249 ± 15 ppm.

Again, we do not dispute this and have actually used the same data from Eisenstadt et al. (2010) in our Reply, with similar results. However, the values given above use the new measurements of the $^{17}\text{O}/^{16}\text{O}$ isotope ratio of VSMOW published in Barkan and Luz (2011). Our own measurements do not support these results and a satisfactory explanation for the revision of the older measurements by Luz' group (Barkan and Luz, 2005) has not been given. Note that the value of (249 ± 15) ppm was calculated with $\kappa = 0.521$. In the present case, this is inconsequential because $^{18}\epsilon_R$ was assumed to be -20 ‰ and therefore $^{18}\delta_{\text{S0}} = -0.014$ ‰.

P 10524 L.1. Barkan and Luz (2011) do mention that $^{17}\delta_{\text{S0}}$ was close to 0 w.r.t. air. “the value of $^{17}\Delta_{\text{bio}}$ (249 per meg) in the 2000 paper was derived from O_2 samples in which the $\delta^{18}\text{O}$ 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 $\delta^{17}\text{O}$, $\delta^{18}\text{O}$ ”

As mentioned before, we asked Dr Luz on separate occasions for the original $^{17}\delta$ and $^{18}\delta$ data published in the 2000 paper, but were unsuccessful in obtaining them. We do not think that it is sufficient to rely on anecdotal, semi-quantitative evidence to establish a benchmark set of input parameters. Nevertheless, our discussion in Sect. 2.2 of the Reply includes examples that include the possibility that $^{17}\delta_{\text{S0}}$ was close to 0 (see rows 4a and 4b in Table 1).

The above information contradicts how Kaiser and Abe calculate the $^{17}\delta_{\text{S0}}$ and $^{18}\delta_{\text{S0}}$ of *Acropora*. If $^{18}\delta_{\text{S0}}$ was in fact 0‰ at time of measurement, instead of (-9.16 ± 0.71) ‰ as calculated by Kaiser, then the resulting $^{17}\delta_{\text{S0}}$ would have been 0.252‰ which yields $^{17}\Delta_{\text{S0}}^{\#}(\theta_R) = 252$ ppm. Such a scenario would require a large $18\epsilon_P$ (~ 10 ‰). Given the uncertainty on this issue, it seems equally reasonable to estimate values of $^{17}\Delta_{\text{S0}}^{\#}(\theta_R) = 252$ 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.

We agree that *Acropora* might show photosynthetic isotope fractionation.

Response to other comments by Dr Nicholson

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.

Barkan and Luz (2011) stated "We also note that throughout the period over which we conducted the new measurements, we included in the analyses VSMOW samples taken only from newly opened ampoules. Hence, we could exclude possible fractionation effects due to water storage." This does not give an explanation of the change between the 2005 and the 2007-2009 measurements as we (Kaiser and Abe, 2011) stated: "[Barkan and Luz] do not give an explanation for this change, other than that the experimental system and measurement procedures were somewhat improved". Perhaps, Barkan and Luz (2011) wanted to avoid the impression that the water was isotopically fractionated during storage from 2005 to 2007-2009.

P 10533, L.23 Change ‘centred’ to centered.

'Centred' is correct because the paper is written in British English.

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

This typo was introduced during typesetting and not spotted in the galley proofs. Hopefully, it did not lead to any confusion.

Results for $^{17}\delta_{\text{W}}$ and $^{17}\Delta_{\text{W}}$ are listed under the $^{17}\delta_{\text{P}}$ and $^{17}\Delta_{\text{P}}$ 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 $^{17}\Delta_{\text{W}}$ and $^{17}\Delta_{\text{P}}$ (Barkan and Luz, 2011). I suggest separate columns for the composition of water.

As the reviewer noted himself, the fact that rows 5 and 6 correspond to $^{18}\delta_{\text{W}}$, $^{17}\delta_{\text{W}}$ and $^{17}\Delta_{\text{W}}$ is actually explained in the caption text. In addition, the corresponding rows are identified by the lack of ϵ_{P} values. The difference between $^{17}\Delta_{\text{W}}$ and $^{17}\Delta_{\text{P}}$ varies depending on the $^{17}\Delta$ definition and the species of interest (see rows 5, 5a-5e and 6, 6a-6e). The difference between $^{17}\Delta_{\text{W}}$ and $^{17}\Delta_{\text{P}}$ is not a fixed value of 26 ppm. A separate table in the revised version of this Reply will allow a direct comparison between the three different measurements of the triple oxygen isotope composition of VSMOW and the derived isotopic composition of ocean 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.

Most of the quantities are not directly measured, but calculated from other measured values, except for θ_{P} , $^{18}\epsilon_{\text{P}}$, $^{18}\delta_{\text{W}}$ and the values listed in the table caption. We will follow the suggestion to highlight measured quantities, which was also made by Dr Miller.

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?

The differences in $^{18}\delta_{\text{P}}$ are due to the fact that we used published values rounded to two decimals (Eisenstadt et al., 2010), whereas Luz and Barkan had access to the unrounded data. In addition, the difference in $^{17}\delta_{\text{P}}$ is due to the 5 ppm offset between VSMOW and oceanic waters, which was taken into account by us, but not by Luz and Barkan (2011).

Table 1: Line 6c: remove “7” from end of *Phaeodactylum tricornutum*7

This typo was introduced during typesetting and not spotted in the galley proofs. Hopefully, it did not lead to any confusion.

Response to Dr Miller's comments

Dr Miller has kindly summarised his comments in a section at the end of his review. Our general reply above has addressed items 5.1 to 5.3, so in the following we just respond to the remaining items 5.4 and 5.5 and any minor comments throughout the review.

5.4 Although I think it is not necessary – and is possibly confusing to some readers – that Kaiser and Abe frame some of their calculations in terms of the approximated relationship between $^{17}\delta$ and $^{18}\delta$, using the parameters κ and $^{17}\Delta^{\dagger}$ as defined in Kaiser (2011a), that is consistent with the Kaiser (2011a) paper. However, to set $\lambda = \kappa$ ($= 0.5179$) for some of the calculation results pre-

sented in Table 1 is erroneous, if the calculations are referring to the same $^{17}\delta$ and $^{18}\delta$ data set. Admittedly, the same approach was also adopted in section 3.4 of Kaiser (2011a), for calculation results presented in Figure 1 of that paper. However, it is recommended that the relevant entries in Table 1 of the Response comment by Kaiser and Abe be amended to be in accord with $\kappa = 0.521$ for $\lambda = 0.5179$, and suitable explanation provided.

We agree that it would indeed be confusing if we had used an approximate relationship between $^{17}\delta$ and $^{18}\delta$. Actually, we are not assuming any particular relationship between $^{17}\delta$ and $^{18}\delta$ and treat them as independent observables, in line with the dual delta method presented in Kaiser (2011a). Based on $^{17}\delta$ and $^{18}\delta$, different $^{17}\Delta$ excess values ($^{17}\Delta$) can be defined, depending on which the interpretation of $^{17}\Delta$ varies (see section 2.1 in Kaiser and Abe, 2011 and section 2.2 in Kaiser, 2011a). For the purpose of the present work, we do not attach any particular interpretation to the $^{17}\Delta$ excess value and use them mainly for illustrational purposes and for historic reasons. However, as Figures 2a and 3a in Kaiser and Abe (2011) illustrate, a definition of $^{17}\Delta = ^{17}\delta - 0.5179 ^{18}\delta$ can be entirely appropriate for an approximate calculation of g based on $^{17}\Delta$ values and may well have smaller errors than approximate calculations using $^{17}\Delta$ values based on other definitions (Figures 2b to 2d, 3b to 3d). We therefore do not consider a replacement or additional entry in Table 1 of $^{17}\Delta^\dagger$ calculated with $\kappa = 0.521$ to be necessary. If desired, the reader can easily calculate $^{17}\Delta$ excess values based on other definitions using the $^{17}\delta_p$, $^{18}\delta_p$, $^{17}\delta_{S0}$ and $^{18}\delta_{S0}$ values provided, but for space reasons we refrained from doing so. Additional entries in Table 1 would probably also make it less readable, which seems to go against the recommendation in comment 5.5 below.

We note that a linear definition of the $^{17}\Delta$ commonly used for other compounds, e.g. nitrate (Morin et al., 2012), presumably because it makes it easier to work with $^{17}\Delta$ as a tracer because mixing relationships. Clearly, in this case, $^{17}\Delta$ cannot be interpreted in a strict sense as a quantitative measure for the deviation from a reference mass-dependent fractionation line, so the interpretation changes, but not the convenience of using $^{17}\Delta$ rather than $^{17}\delta$ - $^{18}\delta$ pairs.

5.5 Table 1 should be enlarged to make it easier to read. Also, it would be helpful to differentiate between empirical (measured) quantities and those which have been calculated.

The table will be enlarged for the revised manuscript and we will follow the suggestion to highlight measured quantities, which was also made by Dr Nicholson.

Response to other comments by Dr Miller

"It is noted here that the definition of $^{17}\Delta^*$ given in equation (6) of Kaiser (2011a) contained a typographic error (a missing '-1' term at the end); it should read as equation (3) in this Comment. Furthermore, the citation should have been to Miller (2002), not to the Miller et al. (2002) paper."

We are sorry about these typographic errors.

We would also like to use this opportunity to point out three typographic errors in the Corrigendum (Kaiser, 2011b): Line 8 of the 3rd paragraph should start with " $^{17}\epsilon_E$ and $^{17}\epsilon_I$ ", and the "%" sign in line 11 should be replaced by a "‰" sign. The phrase "best case" in the caption of Table 3 should be replaced by "base case". This error was actually already present in the original paper.

The first line of equation (5), section 2.1, has a 'λ' missing.

Thank you for spotting this typographic error.

Also, in the footnote to Table 1, '0.519' should presumably be '0.5179'.

0.519 is correct. The corresponding values for the coefficient 0.5179 are given in the footnote. The values in Table 1 itself are needed for the approximate calculations in Figure 4.

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