

1. Introduction

As noted by Prokopenko et al (2011), the use of oxygen triple isotope measurements of dissolved O₂ to evaluate gross photosynthetic production (GPP) rates in aquatic systems, as pioneered by Luz and Barkan (2000), is an “ill-posed” problem, mathematically. This is because the resulting GPP values are highly sensitive to small errors introduced into the calculations. Such errors may derive from approximations in the calculation procedure, or from inaccurate estimation or assignment of parameters used in the equations – especially if not all the requisite empirical data (at the necessary levels of accuracy and precision) are available. This is true even if an “exact” equation is used to describe the relationship between the relevant parameters, as derived independently by Prokopenko et al. (2011) and by Kaiser (2011a). GPP values determined by this latter approach – referred to as the “dual delta” method by Kaiser (2011a) – are nevertheless sensitive to small errors in assigned or measured values of the parameters $^{17}\delta_P$, $^{18}\delta_P$ and γ_R .

Although designated as a “Technical Note”, presumably to comply with *Biogeosciences* editorial policy, such classification of the nineteen-page paper by Kaiser (2011a) underplays its comprehensive and very informative assessment of the use of oxygen triple isotope measurements of dissolved O₂ to evaluate the calculation of GPP in a variety of model situations. The paper is more akin to a textbook chapter than it is to a technical note. Furthermore, it is certainly no disgrace, for a paper containing such a level of detail and complexity, that it was necessary subsequently to issue a short Corrigendum (Kaiser 2011b), describing minor corrections to a small number of the calculations.

Nicholson (2011) acknowledged the improvements to GPP calculation procedure introduced by Kaiser (2011a), but argued that the author’s “base case” relationship between the $\delta^{17}\text{O}$ and $\delta^{18}\text{O}$ values of dissolved O₂ produced by photosynthesis (designated as $^{17}\delta_P$ and $^{18}\delta_P$ respectively) was derived in a way that was not consistent with earlier publications in this field. Specifically, that Kaiser (2011a) had calculated $^{17}\delta_P$ using:

$$^{17}\Delta_P = \ln(1 + ^{17}\delta_P) - \lambda \ln(1 + ^{18}\delta_P) = 249 \times 10^{-6} \quad (1)$$

with $\lambda \equiv \lambda_R = 0.5179$, as appropriate for dark respiration only (Luz and Barkan, 2005), rather than using a λ value appropriate to a system in “biologically steady state with seawater” (λ_{S0}), which will have a value significantly less than λ_R (Angert et al., 2003). Nicholson (2011) calculated that $\lambda_{S0} = 0.5154$, for average ocean $^{18}\epsilon_R = -20 \text{ ‰}$ and for $\gamma_R \equiv \lambda_R = 0.5179$. In turn, it was suggested (and using here the terminology adopted by Kaiser, 2011a), that the resulting assignment of $^{17}\Delta_P^\# = 249 \times 10^{-6}$, rather than $^{17}\Delta_{S0}^\# = 249 \times 10^{-6}$, when calculating $^{17}\delta_P$ for the “base case” determination of GPP, was responsible for producing values of g (the derived gross production relative to O₂ influx ratio) that are ~30 % larger than those reported previously in the literature. Since Kaiser (2011a,b) was published, a paper by Luz and Barkan (2011a) reported experimental measurements of the isotopic composition of marine photosynthetic O₂ and concluded that, although the “new rigorous equation of Prokopenko et al. (2011) is a very welcome addition”, previous estimates of GPP yielded results close to those calculated with the new approach.

2. “Base case” determination of GPP

Firstly, as acknowledged by Kaiser and Abe (2011), Nicholson (2011) is not questioning the validity of the “dual-delta” method, nor questioning that it represents an improvement over previous calculation procedures. The heart of the disagreement is the validity of the point made by Nicholson (2011) that Kaiser (2011a) should have assigned $^{17}\Delta_{S0}^{\#} = 249 \times 10^{-6}$, rather than $^{17}\Delta_P^{\#} = 249 \times 10^{-6}$, when calculating $^{17}\delta_P$ for the “base case” determination of GPP, for meaningful comparison with calculations from previous published studies.

I do not think it is correct, nor is it appropriate, to dismiss the comment by Nicholson (2011) as having “no merit”, as stated in the Abstract of the Reply by Kaiser and Abe (2011). Nicholson’s case is well articulated and, regardless of whether Kaiser and Abe strongly disagree with it, is worthy of more respectful consideration. Furthermore, to dismiss it as having “no merit” is also to question the judgement of those involved in the evaluation of the Comment paper, and the decision to publish it. I recommend that the rather confrontational language be modified accordingly. Similarly, in section 2, line 9: “Firstly, the isotopic composition of photosynthetic O₂ cannot be ‘defined’; it can only be measured, modelled or calculated based on other data.” Well, an isotopic composition can be “assigned” for calculation purposes, which is surely acceptable. It is unfortunate that, in the Abstract of the Comment by Nicholson (2011), the author stated that Kaiser “inconsistently *defines* the biological end-member.”

In the final paragraph of the Conclusions section, Kaiser and Abe (2011) claim that the Comment by Nicholson (2011) is “centred on the appropriate choice of $^{17}\delta_P$ and $^{18}\delta_P$.” However, they then proceed to state that: “The demand for the ‘correct’ choice – of $^{17}\delta_P$ and $^{18}\delta_P$ – is premature and besides the main topic of the original paper.” This does indicate that Kaiser and Abe (2011) are responding to a point that Nicholson (2011) didn’t actually make. The central point of the Comment by Nicholson (2011) was to suggest that much of the discrepancy between model GPP values calculated by Kaiser (2011a), on the one hand, and actual GPP values published in previous studies, on the other hand, could be attributed to the procedure that Kaiser (2011a) used to calculate $^{17}\delta_P$ for his “base case.” There is no implication about “correct” values, nor “best case” versus “base case.” I suggest that if Kaiser and Abe (2011) were to accept the validity of the central point by Nicholson (2011), it would not detract in any way from the value and stature of the Kaiser (2011a) paper.

Instead of addressing that central point, the Reply by Kaiser and Abe (2011) discusses in detail how the isotopic composition of photosynthetic O₂ was calculated in Kaiser (2011a), including “data that were previously omitted or not yet published.” The authors then proceed to discuss the dependence of g on the assumed isotopic composition of photosynthetic O₂. It is questionable whether it is appropriate to introduce “data that were previously omitted or not yet published” into a Reply to a published Comment – as the latter could only refer to data which was already in the public domain.

Much of the Reply consists of further development of the detailed exposition presented by Kaiser (2011a) and the subsequent Corrigendum (Kaiser 2011b). Aside from the question of whether it is legitimate to introduce new and unpublished measurements of $^{17}\delta$ and $^{18}\delta$ of VSMOW relative to air O₂ (section 2.4), before submitting those for publication in a separate manuscript, much of the additional information provided by Kaiser and Abe (2011) is a useful adjunct to the original paper (Kaiser, 2011a) and

Corrigendum (Kaiser, 2011b). Kaiser and Abe (2011) conclude by noting that “considerable systematic uncertainty remains in the calculation of g due to the uncertainty in the isotopic composition of photosynthetic O_2 , $^{17}\delta_P$ and $^{18}\delta_P$ ”, complicated also by recently published experimental data (Eisenstadt et al., 2010) which demonstrate significant inter-species variability in the photosynthetic isotope fractionation and consequently in the inferred value of g . This is undoubtedly true and highlights the need for further experimental investigations, as recommended by Kaiser and Abe (2011).

3. Definitions and use of oxygen triple-isotope ‘excess’ values

Kaiser and Abe (2011) state that “the definition and use of triple isotope excess values can be very confusing, even for experts in the field. The use of different $^{17}\Delta$ definitions with different coefficients causes significant delays and misunderstandings during scientific communication, which can be avoided if the dual delta method is adopted.” This reiterates a point made by Kaiser (2011a) and undoubtedly contains an element of truth. However, it is arguable that some confusion will be caused – especially to those new to this field – by Kaiser (2011a) having reverting to the use an approximate relationship between the mass dependent fractionation of $\delta^{17}O$ relative to $\delta^{18}O$, when presenting some of his oxygen isotope budget calculations. The same procedure is used in section 2 of the Comment by Kaiser and Abe (2011), in calculating ^{17}O excess values. As far as I’m aware, no research group in this field has used such an approach since 2003, because it was generally recognised that using a non-approximated form of the fractionation relationship provides a much more robust and consistent framework for the calculations.

I believe it is worth discussing this matter in some detail, especially as section 2.2 (“Quantification of deviations from mass-dependent isotope ratio relationships”) of Kaiser (2011a) does contain some assertions which – although formally correct and well understood by Kaiser and Abe – may be misleading to some readers.

When the first use of oxygen triple isotope measurements for assessing gross biological oxygen production in aquatic systems was published, by Luz and Barkan (2000) it was usual, with few exceptions, to report deviations from a specific mass-dependent fractionation relationship by use of the relationship:

$$\Delta^{17}O = \delta^{17}O - \kappa \delta^{18}O \quad (2)$$

where the value of κ was assigned to be 0.52, or an alternative value based on empirical evidence or theoretical considerations. Here, $\Delta^{17}O$ corresponds to the parameter $^{17}\Delta^\dagger$ as defined by Kaiser (2011a). Equation (2), with $\kappa = 0.52$, had for many years been used to quantify oxygen isotope ‘anomalies’ in meteorites. It was known to be an approximation of the ‘true’ relationship between $\delta^{17}O$ and $\delta^{18}O$, which is not linear. However, equation (2) was fit for the intended purpose. For oxygen isotope anomalies measured to a precision of a few ppm (‘per meg’), and for data sets in which the range of δ values is much larger than encountered for meteorites, the limitations of $\Delta^{17}O$ as defined by equation (2) become apparent. They were discussed in detail by Miller (2002). During the year before the Luz and Barkan (2000) paper was published, those authors had been made aware that a more robust definition of $\Delta^{17}O$ was possible, which may be written as (and analogous to the δ function):

$$\Delta^{17}O = \frac{(1 + \delta^{17}O)}{(1 + \delta^{18}O)^\lambda} - 1 \quad (3)$$

or

$$\ln(1 + \Delta^{17}\text{O}) = \ln(1 + \delta^{17}\text{O}) - \lambda \ln(1 + \delta^{18}\text{O}) \quad (4)$$

This offers the advantage that $\ln(1 + \delta^{17}\text{O})$ is truly a linear function of $\ln(1 + \delta^{18}\text{O})$, with $\Delta^{17}\text{O}$ being independent of the range and distribution of the δ values and also independent of the chosen reference material for the δ measurements (such as VSMOW or air O_2). Luz and Barkan generously refrained from adopting this definition of $\Delta^{17}\text{O}$ for their 2000 paper, prior to publication of Miller (2002). Thereafter, Angert et al. (2003), on which Luz and Barkan were co-authors, adopted an approximated version of equation (4), also discussed by Miller (2002), and based on the fact that $\Delta^{17}\text{O} \approx \ln(1 + \Delta^{17}\text{O})$ for $\Delta^{17}\text{O} \ll 1$. Angert et al. (2003) used the designation $^{17}\Delta$ to distinguish this modification from the “conventional” $\Delta^{17}\text{O}$ definition given by equation (2). Thus:

$$^{17}\Delta = \ln(1 + \delta^{17}\text{O}) - \lambda \ln(1 + \delta^{18}\text{O}) \quad (5)$$

In practice, the difference between $\Delta^{17}\text{O}$ as defined by equation (4) and $^{17}\Delta$ as defined by equation (5) is only detectable, for a measurement precision of 5 ppm (per meg), when $\Delta^{17}\text{O}$ exceeds $\sim 3,200$ ppm. Since the Luz and Barkan (2000) paper was published, it seems that only a single publication (Sarma et al., 2003) has utilised equation (2) rather than equation (5), in assessing gross photosynthetic productivity by oxygen triple isotope measurements (see Table 1 of Kaiser, 2011a). Perhaps the authors of Sarma et al. (2003) were unaware of the paper by Angert et al. (2003), as indeed they didn't cite it. Sarma et al. (2005), however, did utilise the $^{17}\Delta$ definition of equation (5). Of course, if the $\delta^{17}\text{O}$ and $\delta^{18}\text{O}$ data for a particular investigation are all not far removed from zero, then the choice of $\Delta^{17}\text{O}$ definition and value of λ or κ is not so critical. Barkan and Luz (2011) used this point to argue that, because “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 ... the calculated $^{17}\Delta_{\text{bio}}$ was not sensitive to the value of λ (in equation 5), or to whether $\delta^{17}\text{O}$ and $\delta^{18}\text{O}$ were converted into $\ln(1 + \delta^{17}\text{O})$ and $\ln(1 + \delta^{18}\text{O})$.” This seems reasonable, although not possible to validate because the $\delta^{17}\text{O}$ and $\delta^{18}\text{O}$ values from that study were not published.

Perhaps if Luz and Barkan (2000) had used the $\ln(1 + \delta^{17}\text{O})$ versus $\ln(1 + \delta^{18}\text{O})$ relationship to define $^{17}\Delta$, instead of using $\delta^{17}\text{O}$ versus $\delta^{18}\text{O}$, there would not have been the need for Kaiser (2011a) to refer at all to a $^{17}\Delta$ definition based on $\delta^{17}\text{O}$ versus $\delta^{18}\text{O}$. That would have simplified his paper and perhaps made the subject of GPP calculations from $^{17}\Delta$ measurements appear a little less formidable to the non-expert. The discussion could then have been framed in the context of just one definition of $^{17}\Delta$, designated by Kaiser (2011a) as $^{17}\Delta^{\#}$, shifting the focus of debate to the question of what value of λ should be used for individual specific circumstances, besides what the “correct” values are for other parameters needed for GPP calculations, such as $^{17}\delta_{\text{p}}$, $^{18}\delta_{\text{p}}$ and γ_{R} .

Of the four $^{17}\Delta$ definitions mentioned by Kaiser (2011a), $^{17}\Delta^{\ddagger}$ and $^{17}\Delta^*$ have never been used in published GPP calculations, as far as I am aware. $^{17}\Delta^{\ddagger}$ values are very close to those calculated using $^{17}\Delta^{\#}$, for a common value of λ . Furthermore, $^{17}\Delta^*$ would give exactly the same value as $^{17}\Delta^{\#}$ in GPP calculations, for a common value of λ . 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.

Although this is not a review of Kaiser (2011a), I believe it is relevant to provide a rebuttal to the following statements made in that paper, as they underpin arguments developed in the Comment by Kaiser and Abe (2011):

- “coefficients κ and λ ... strictly speaking, their choice is entirely arbitrary, as these are merely definitions.”
- “Per se, none of the definitions (of ^{17}O excess) or coefficients is better or worse than others – all of them are merely mathematical constructs.”
- “... there is no definition of the ^{17}O excess that is inherently “better” than others... it is essentially possible to adopt any definition.”

Well, the δ value is also a “mathematical construct”, devised to allow isotope measurements to be performed on the basis of comparison with a reference material, thereby avoiding the difficulties associated with the reporting of absolute ratios. The δ scale is non-linear ($-1 < \delta < \infty$), which accounts for the fact that the definition of $^{17}\Delta$ as given by equation (2), defining $^{17}\Delta^\dagger$ as designated by Kaiser (2011a), cannot accurately describe the fractionation behaviour of $^{17}\text{O}/^{16}\text{O}$ relative to $^{18}\text{O}/^{16}\text{O}$. A definition of $^{17}\Delta$ which avoids unnecessary approximations is surely preferable to one which does contain approximations, especially when applied to small (ppm scale) deviations from the reference fractionation relationship. As to whether the assignment of the respective coefficients κ and λ is “entirely arbitrary”, it is technically correct in that, in principle, any value of κ or λ could indeed be imposed for the reference fractionation line. However, it is clearly most useful for analysis of the isotope data if the values of κ or λ correspond to the triple-isotope fractionation behaviour of the system being considered.

It is also worth noting that a ^{17}O anomaly (^{17}O excess) denoted by $^{17}\Delta^*$ or $^{17}\Delta^\#$ in Kaiser’s notation quantifies the magnitude of an ordinate shift (of $\ln(1 + ^{17}\Delta^*)$ or $^{17}\Delta^\#$, respectively) from a reference fractionation line on a $10^3 \ln(1 + \delta^{17}\text{O})$ versus $10^3 \ln(1 + \delta^{18}\text{O})$ plot. (The factor of 10^3 is optional, but without it the respective axis scales will not relate to the δ values as reported in ‘per mil’; this point is often overlooked.) Therefore, comparison or summation of ^{17}O excess values is only valid if related to the same reference fractionation line. Sometimes, even experienced practitioners fail to recognise this. For example, in Barkan and Luz (2011), the authors claim that their “recent precise measurements show that the 249 per meg ^{17}O excess of marine photosynthetic O_2 with respect to atmospheric O_2 ($^{17}\Delta_{\text{bio}}$) consists of two parts. A major portion, 223 per meg, is derived from the isotopic composition of seawater, and the remainder from a small excess of ^{17}O (26 per meg) in photosynthetic O_2 with respect to the substrate water.” However, their 223 per meg value is calculated with reference to $\lambda = 0.516$, whereas the 26 per meg value is obtained using $\lambda = 0.518$. Furthermore, δ values were reported relative to air O_2 for the 223 per meg value, but relative to a water sample of undisclosed isotopic composition for the 26 per meg result. Clearly, the claimed validation of the 249 per meg figure is not warranted. Relative to a reference line of either $\lambda = 0.516$ or $\lambda = 0.518$, the $^{17}\Delta_{\text{bio}}$ value will be somewhat greater than 249 per meg.

To illustrate how the steady-state ^{17}O excess is affected by the value of the net to gross production ratio (f), Figure 1 of Kaiser (2011a) showed the calculated variation of $^{17}\Delta^\#$ and $^{17}\Delta^\dagger$ as a function of f , for assigned values of $^{18}\epsilon_{\text{R}}$, $^{18}\delta_{\text{P}}$ and $^{17}\delta_{\text{P}}$, and with $\lambda = \kappa = ^{17}\epsilon_{\text{R}}/^{18}\epsilon_{\text{R}} = 0.5179$. However, for a given data set of $^{17}\delta$ and $^{18}\delta$ values relating to the fractionation of O_2 , $\lambda \neq \kappa$ if those parameters are based on the respective regression slopes on the $10^3 \ln(1 + ^{17}\delta)$ versus $10^3 \ln(1 + ^{18}\delta)$ or $^{17}\delta$ versus $^{18}\delta$ plot. With all $^{17}\delta$ and $^{18}\delta$

values <0 relative to the reference (air O_2 in this case), $\kappa > \lambda$. For $\lambda = 0.5179$, the corresponding κ value will be ~ 0.521 (Miller, 2002), as also noted by Luz and Barkan (2011b). The actual value of κ will depend on the range and distribution of the $^{17}\delta$ and $^{18}\delta$ values; κ is not a ‘robust’ parameter. In Table 1 of Kaiser and Abe (2011), rows 4a, 5a and 6a similarly refer to calculations in which λ has again been equated with κ . This could lead to the misleading and confusing impression that regression of $10^3 \ln(1 + ^{17}\delta)$ versus $10^3 \ln(1 + ^{18}\delta)$ could give the same slope as regression of $^{17}\delta$ versus $^{18}\delta$, for a given $^{17}\delta$, $^{18}\delta$ data set.

An additional complication with applying ^{17}O excess measurements to calculations of aquatic gross production is that the parameters θ and γ are used to characterise the discrimination against ^{17}O and ^{18}O (relative to ^{16}O), where:

$$\theta \equiv \frac{\ln(^{17}\alpha)}{\ln(^{18}\alpha)} = \frac{\ln(1 + ^{17}\epsilon)}{\ln(1 + ^{18}\epsilon)} = \frac{\ln(1 + \gamma^{18}\epsilon)}{\ln(1 + ^{18}\epsilon)} \quad (6)$$

$$\gamma \equiv \frac{^{17}\alpha - 1}{^{18}\alpha - 1} = \frac{^{17}\epsilon}{^{18}\epsilon} \quad (7)$$

and yet (as noted by Angert et al., 2003), unlike λ , neither θ nor γ can be measured directly – except in the special cases of (i) a system at biological steady state (O_2 production equals uptake via respiration), in which case θ equates to λ ; (ii) a system in which only uptake (respiration) is occurring, in which case γ equates to λ . As also recognised by Angert et al. (2003), in other cases λ is likely to be different from both θ and γ , and although the difference between θ and γ is only ~ 0.003 , this is of some significance in the context of ^{17}O excess calculations. Kaiser (2011a) noted that, for O_2 production only, or with both production and respiration but not at isotopic steady state, there is no simple relationship between $\ln(1 + ^{17}\delta)$ and $\ln(1 + ^{18}\delta)$. This highlights a major advantage of the “dual delta” approach to GPP determination, and it is encouraging to see Kaiser’s recommended procedure now being adopted by others (Hamme et al., 2012). The use of $\Delta^{17}O$ values (‘ ^{17}O excess’ is appropriate terminology for applications in which $\Delta^{17}O$ values will invariably be greater than zero) is generally much better suited to non-biological systems, such as the ^{17}O excess of meteoric waters or the identification and quantification of non-mass dependent isotopic fractionations.

4. Minor corrections (typographic errors)

The first line of equation (5), section 2.1, has a ‘ λ ’ missing. It should read:

$$^{17}\Delta_p^\# = \ln(1 + ^{17}\delta_p) - \lambda \ln(1 + ^{18}\delta_p)$$

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

5. Conclusions and recommendations

It is unfortunate and regrettable that Jan Kaiser’s request to obtain original isotope data used in the Luz and Barkan (2000) paper, specifically the measurements of photosynthetic O_2 produced under steady state conditions, was not met with a positive response, thus leading Kaiser to resort to indirect reconstruction of what the isotope composition of that component might have been. In turn, that has led to the controversy of whether a particular aspect of the reconstruction – specifically, assigning $^{17}\Delta_p^\# (\gamma_R)$, rather than $^{17}\Delta_{S0}^\# (\gamma_R)$, to be 249×10^{-6} (249 ppm), caused the “base case” scenario presented by Kaiser (2011a)

to contain a systematic enhancement of calculated g values (by $\sim 30\%$), compared to previous results based on either ^{17}O excess calculations (Luz and Barkan, 2000) or an iterative approach (Hendricks et al., 2004; Reuer et al., 2007; Juranek and Quay, 2010).

Separate from this issue is that, even when using the “dual delta” procedure, assessment of g in the marine mixed layer is further complicated by evidence (Eisenstadt, 2011; Luz and Barkan, 2011a) for species-specific differences in $^{17}\delta_{\text{p}}$ and $^{18}\delta_{\text{p}}$ values, as reviewed by Kaiser and Abe (2011); also by discrepancies between the oxygen triple isotopic composition of VSMOW relative to air O_2 , both within the same laboratory (Barkan and Luz, 2005, 2011) and between different laboratories.

Specific recommendations for revising the Response by Kaiser and Abe (2011) are as follows:

- 5.1 Sections which include rather confrontational statements should be modified to provide a more professional discourse. It is not appropriate to dismiss the Comment by Nicholson (2011) as having “no merit”, as stated in the Abstract of the Reply by Kaiser and Abe (2011). Even if Kaiser and Abe strongly disagree with points made by Nicholson, these should be refuted in a respectful manner.
- 5.2 The central point made by Nicholson (2011) is that only by assigning $^{17}\Delta_{\text{S}_0}^{\#} = 249 \times 10^{-6}$ rather than $^{17}\Delta_{\text{p}}^{\#} = 249 \times 10^{-6}$ when calculating $^{17}\delta_{\text{p}}$, can meaningful comparison be made with GPP evaluations from previous published studies. There is no implied “best case.” This matter should be addressed in the Response by Kaiser and Abe. I can find no flaw in Nicholson’s argument, and believe that if Kaiser and Abe were to recognise its validity, that would not undermine the usefulness and stature of the major contribution provided by Kaiser (2011a,b). Alternatively, if Kaiser and Abe disagree with Nicholson’s central point, their case should be clearly and succinctly presented in the Response.
- 5.3 Much of the extensive additional material and discussion provided by Kaiser and Abe is a useful and informative extension to Kaiser (2011a,b) and is certainly worthy of publishing. However, it is questionable whether it is appropriate to introduce new and unpublished measurements of VSMOW relative to air O_2 here, rather than submitting those for publication in a separate manuscript.
- 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 presented 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.
- 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.

Finally, I would like to strongly endorse the very sensible plea by Kaiser (2011a) that “any $^{17}\Delta$ value should not be cited in isolation, i.e. not without its definition and, crucially, not without the corresponding $\delta^{17}\text{O}$ and $\delta^{18}\text{O}$ values.”

6. References

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