

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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Nicholson's comment and Luz' review are both suffering from a misinterpretation of my initial paper. Both imply that I had concluded that previous estimates of gross O₂ production are too low. Luz even considers this to be "a major conclusion".

I would like to emphasise that, in my initial paper, I have not drawn any conclusions with respect to the magnitude of production estimates based on different calculation parameters and calculation methods. In particular, what I considered as the "base case" should not be misconstrued as a "best case".

Based on the available data, I have not been able to construct a "best case". I re-
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quested from Boaz Luz the original data of the 2000 measurements of the isotopic composition of photosynthetically oxygen produced at steady-state, but was unsuccessful in obtaining them. I therefore had to rely on a more indirect reconstruction of what the isotopic composition might likely have been. This was only possible in the case of *Acropora*. I also presented an alternative derivation based on the isotopic composition of water. This is discussed in Section 5 of the initial paper, as well as the choice of "base case" values. A recent correction of the earlier measurements of the isotopic composition of water by the Luz group (as referenced in Luz' comment) allows the derivation based on these values to be revisited. It appears to partially resolve some of the discrepancies I highlighted in my initial paper. Thus, it actually responds to one of the *actual* conclusions in my initial paper, i.e. the need to remeasure several of the input parameters for the oxygen triple isotope method.

The main point of my initial paper was to present a consistent and accurate way of calculating gross production from oxygen triple isotope measurements, which I have dubbed the "dual delta method". This shows, among other things, that the way the triple isotope excess ($^{17}\Delta$) is defined is irrelevant for the work with oxygen triple isotopes. In contrast to what is implied by Nicholson and Luz, there is no such thing as a "correct" coefficient for the definition of $^{17}\Delta$, or indeed the functional relationship between $^{17}\delta$ and $^{18}\delta$ itself. Other major aspects of the paper include a study of the systematic uncertainties in the derived gross production / O₂ influx ratio (termed *g*) due to the choice of calculation parameters as well as studies of how the choice of parameters and calculation methods may lead to systematically different results in practice.

I will submit a formal reply to Nicholson's comment soon. Independently, a Corrigendum will be published to correct some minor errors in the initial paper, including the implicit choice of $\gamma_R = 0.5205$ by Juranek and Quay (2010) rather than 0.518 as I had assumed. This implicit choice was confirmed by personal communication with Ellie Juranek.