

We would like to thank the two reviewers for their careful evaluations.

A major point brought up by Jan Kaiser is that we based our analysis of the ice core $\delta^{17}\text{O}$ data on the 2005 value of 173 per meg between present ocean water and present atmosphere by Barkan and Luz. We did not expect that the value would move back in the direction of the initial value of 250 per meg and therefore did not investigate our results far enough in that direction.

In the meantime we did a few calculations for the average value of 223 per meg given in Barkan and Luz, 2011 (see figures R1 and R2 below) and found that the sensitivity corresponding to the amount of C4 plants is much reduced with the revised value and also the dependence of the precipitation water anomaly becomes a minor issue. The proposed fractionation of photosynthesized O_2 from the ocean biosphere *Luz and Barkan (2011)* has a surprisingly low influence on our calculations. In essence the solutions for ocean productivity change primarily for the adaptation of ocean water vs. atmospheric O_2 from 170 per meg to 223 per meg. The initial calculations showed increased ocean productivity during glacials (red line in figure R1) which is now not obvious any longer. In a revised manuscript we would like to include those calculations and revise our conclusions accordingly.

Jan Kaiser suggests a few sensitivity tests which are shown in figure R2.

Our point by point response starts below the graphs with reviewer's comments in red and our answers in black.

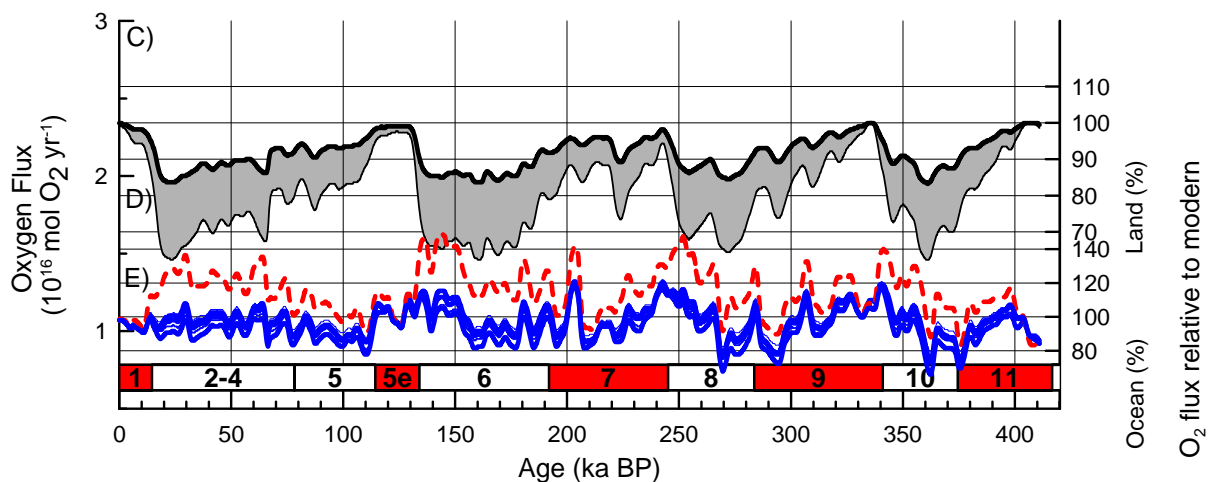


Figure R1: Similar to figure 4 of our original manuscript. The red dotted line is our preferred solution from the original manuscript. The blue lines show similar calculations but with a present ocean water vs. atmosphere value of 223 per meg instead of the 170 per meg we used for our initial calculations. The blue lines cover precipitation water anomalies 0 to 10 per meg lower than today; with and without photosynthetic fractionation of the ocean and C4 partitions of 40-70%.

Figure R2

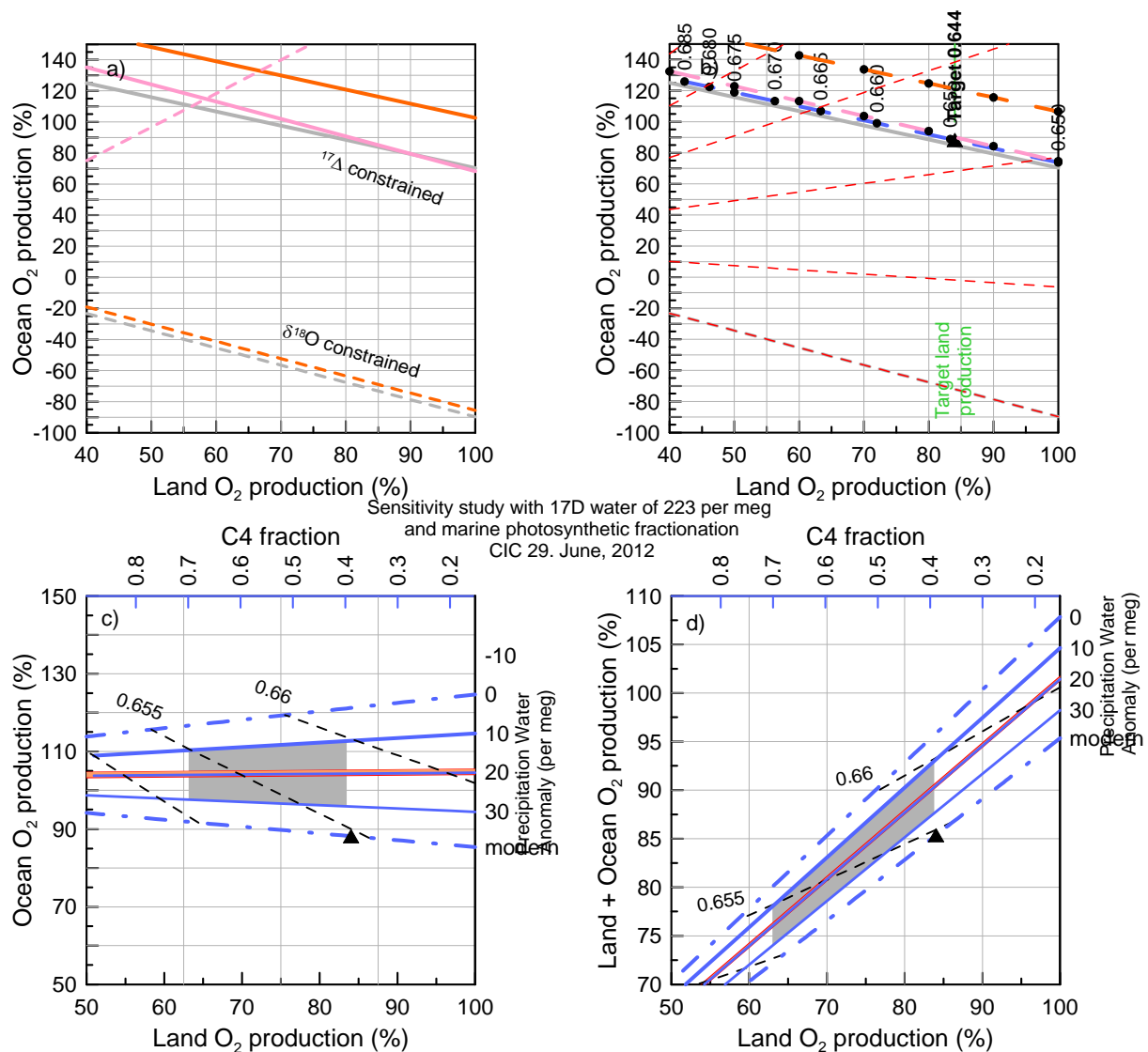


Figure R2: Similar to figure 3 in the main text.

Plots of ocean O₂ production vs. land O₂ production (a-c) and total O₂ production vs. land O₂ production (d) for the Last Glacial Maximum default scenario (see Table 4). The lines and areas indicate allowable values as constrained by the $\delta^{18}O$ and $^{17}\Delta$ of paleoatmospheric O₂ relative to the modern (100%) production.

a) Gray dashed and solid line: Solutions that satisfy $\delta^{18}O_{atm}$ and $^{17}\Delta$, respectively, assuming modern humidity and a hydrological anomaly similar to modern. For a $^{17}\Delta$ ocean water vs. atmosphere of 223 per meg and a 4.0‰ $\delta^{18}O$ marine photosynthetic enrichment ($\lambda=0.524$) with respect to seawater in the surface ocean. The orange lines are calculated for identical conditions but with a 20% increased stratospheric flux. Pink lines are for a $^{17}\Delta$ ocean water vs. atmosphere of 223 per meg and no photosynthetic enrichment.

b) Solutions of a) in gray. Increasing the relative humidity (black dots and associated labels; blue line for the gray scenario in a) drives the intercept of $^{17}\Delta$ and $\delta^{18}O$ towards the target value for land productivity (see text for details). The light dashed red lines give solutions for $\delta^{18}O$ with increasing relative humidity (for the gray scenario in a). The $^{17}\Delta$ line is hardly affected by variable relative humidity.

c) and d) Again for a $^{17}\Delta$ ocean water vs. atmosphere of 223 per meg and a 4.0‰ $\delta^{18}O$ marine photosynthetic enrichment ($\lambda=0.524$) with respect to seawater in the surface ocean, the value for land carbon productivity is locked. Therefore the land O₂ production is inversely proportional to the C4 contribution (top axis). We show solutions for precipitation anomalies from 0 to 40 per meg and soil temperature 5°C lower than modern. The area in red represents solutions with soil temperatures 4-7°C lower than modern and a precipitation $^{17}\Delta$ anomaly of 20 per meg lower than modern. Black dashed lines show isolines of global GPP weighted humidity. The gray area represents solutions we favor. Black triangles show the target value of 3b.

Clim. Past Discuss., 8, C131–C135, 2012
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**Climate
of the Past
Discussions**

**Interactive comment on “Planetary fertility during
the past 400 ka based on the triple isotope
composition of O₂ in trapped gases from the
Vostok ice core” by T. Blunier et al.**

Anonymous Referee #1

Received and published: 15 March 2012

General comments:

The manuscript describes how changes in the biological activity – either in the ocean or on land – can be retrieved by the triple oxygen approach. However significant uncertainties are present for these estimates that mainly originate from the time-dependent C4-plant percentage and C4/C3 plant distribution as well as the D17O anomaly in precipitation. Until these two parameters remain rather unconstrained the triple oxygen isotope approach is moderately helpful for quantifying biological activity changes.

Despite the fact that the uncertainty is still rather high for the estimates they are of importance in order to learn more about the origin of those uncertainties by – for in- stance – sensitivity tests with sophisticated vegetation models. Moreover, once these uncertainties can be lowered, the presented record will be extremely valuable because of its high time resolution and its long-term aspect covering several transitions. The paper is well structured and well written, however due to its complexity it is partly difficult to follow. Nevertheless the paper should be accepted for publication in ACP after some revisions. The revision must include the following general comments:

1) Figure 2 is valuable but should be extended to today’s situation, i.e. C4 contribution of 20-25%. 100% on the axis should be explicitly mentioned what it means? I guess, present-day conditions. If this is true then today’s situation is not correctly given, but would correspond to a 50% C4 contribution to GPP.

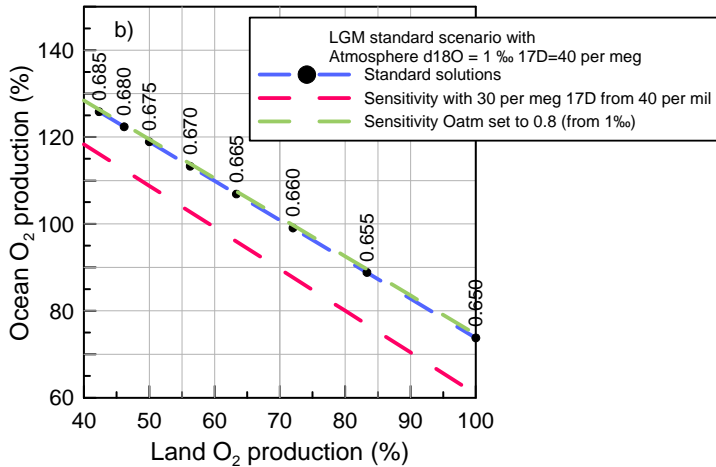
The figure is strictly for glacial conditions e.g. $\delta^{18}\text{O}$, 17D and CO_2 . It is not possible to extract results for the present from this graph. It is also not possible to extend this graph to a present day conditions. We added a note to the figure caption to clarify this fact. The meaning of 100% is now explained. It could be extended to C4=20% but this would increase clutter, and the results of the extrapolation are implied anyway.

2) Figure 3 is difficult to follow. Again the reviewer is not convinced that today’s conditions are correctly retrievable from the diagrams.

Again the figure describes a LGM situation and does not represent today’s condition. We added a reference to table 4 that gives the conditions for this graph. The caption can be improved.

3) Were sensitivity tests performed for the ocean to land oxygen flux ratio regarding changes of D17O and d17O and d18O?

Find below a calculation for our standard last glacial maximum scenario with $^{17}\Delta$ of 40 per meg and $\delta^{18}\text{O}_{\text{atm}}$ of 1‰. A change of $\delta^{18}\text{O}_{\text{atm}}$ to 0.8‰ has almost no effect on the solutions by tuning the humidity. A 10 per meg lighter $^{17}\Delta$ results in an about 10% lower ocean productivity for a given land productivity. This graph can be included in the appendix.



4) Why were changes in fractionation factors excluded? Due to the significant temperature changes during transitions changes are expected.

The reviewer is not correct that temperature effects on fractionation factors were generally excluded. In figure 3 the temperature effect on the land productivity is considered. Further temperature dependence of the ocean solubility is taken into account. As far as they are known temperature effects are taken into account.

5) The author's should check the title's expression "Planetary fertility" for its adequateness.

Our use of "fertility" is intended to conjure a broader image in the minds of readers, paralleling the fundamental importance of the observations and analyses that we have conducted in this manuscript. Oxygen productivity is ultimately a function of the greenness of the biosphere, and therefore its capacity to support life. Fertility is a term beyond agriculture and includes the potential to generate offspring. Our work addresses some of the most fundamental processes for life on earth, and therefore we find "fertility" to be a suitable term here. However, we could also use production instead (similar to our response to Jan Kaiser).

Specific comments:

Page 437, line 17: there are earlier studies that reported O2 measurements in ice cores mentioning the potential to derive biological activities on land and in the oceans (Sowers et al., 1989, Leuenberger 1997).

There are more earlier studies than the two mentioned by the reviewer. We give the references to the most recent once who include both 180 and 170 data.

Page 440, line 6: references for coefficients
 Added

Page 441, line 15-17: Is this scaling necessary?

Yes it is. E.g. The d18O constraint becomes offset if the relative change glacial-interglacial between atmosphere and ocean are not correct.

Page 441, line 18-20: Where applicable . . . the reader cannot follow what has been done to the original data. This must be described in detail.

If the editor sees it the same way we can add the mathematical formula for the averaging to the appendix.

$$x(t) = \frac{\sum_{i=1}^n w_i * x_i}{\sum_{i=1}^n w_i} \text{ where}$$

$$w_i = \frac{1}{\sigma_i} * e^{-(t_i-t)^2/2*\sigma^2}$$

Page 442, line 1-4: Is the isotope exchange rate dependent on the concentration? I guess only when the rate is concentration dependent. I am asking since the residence times of CO2 and O2 are very different. Furthermore, for a dead ocean the CO2 concentration would rise and not fall, which would increase the exchange rate and not decrease it.

Yes the exchange rate is concentration dependent which is described on page 442 and several times

<p>throughout the manuscript. Yes a dead ocean would have potentially this effect. However, the CO₂ concentration is measured and we do not need to guess how much CO₂ was in the atmosphere.</p>
<p><i>Page 442, line 8: Why 0.55 per meg? Isn't it just the division of -170/280 = -0.61 per meg/ppm?</i></p> <p>Correct, thank you</p>
<p><i>Page 443, line 1-2: This interference is now . . .this statement has to be taken with care since otherwise no change in carbon stock can occur! The significant change of more than 500 GtC within roughly 5000yrs between LGM and the Holocene corresponds to an imbalance of 0.1GtC/yr, not even taking into account the atmospheric CO₂ concentration change. This is indeed small compared to the annual fluxes of roughly 100GtC/yr for photosynthesis or respiration.</i></p> <p>We agree with that comment and we make a similar statement in the previous sentence saying that "...production by photosynthesis very nearly balances consumption..".</p>
<p><i>Page 443, line 8: A reference to Leuenberger (1997) might be adequate.</i></p> <p>Leuenberger (1997) derived similar equations, however, not including the stratospheric processes and only for d18O. As we explicitly use the formulation introduced in Blunier et al., 2002 we refrain from citing previous work here.</p>
<p><i>Page 446, eq. 8: This is not true – at least in the stringent way – because otherwise neither carbon stock changes nor CO₂ concentration variations in the atmosphere would be possible.</i></p> <p>We made the stated assumption that the system is in steady state justified by the long lifetime of O₂ in the atmosphere. The equation is true under that assumption.</p>
<p><i>Page 449, line 3-12: This is a matter of discussion, how have you calculated it? Based on with GPP and humidity datasets? More information would be helpful. Also stating an uncertainty would be worthwhile in order to judge the changes in humidity discussed later on.</i></p> <p>The details of the model and model assumptions are given in the cited paper. The humidity is calculated among others based on SSTs from 1950-1975. More details are found in: Langen, P. L., and B. M. Vinther (2009), Response in atmospheric circulation and sources of Greenland precipitation to glacial boundary conditions, <i>Climate Dynamics</i>, 32(7-8), 1035-1054.</p> <p>The uncertainty of the humidity is a few per cent. The point we try to make here is that our model is in the right range to the observations. We do not expect to find an exact match. However, it is worthwhile noting that the change in the ocean vs. atmosphere signature based on the latest publication by Barkan and Luz leads to a relative humidity of 62% much closer to the GCM simulation.</p>
<p><i>Page 450, line 8: When assuming no change in several fractionation factors then you do not allow for temperature dependent fractionations. Is this justified?</i></p> <p>We do not expect significant changes of those physical fractionation factors over the small temperature range relevant for this study. We are not aware of any study showing such dependence for those highly studies fractionation factors.</p>
<p><i>Page 450, line 9-12: Why should the net oxygen fluxes scale to the CO₂ concentration? Is this only based on models or also on data?</i></p> <p>The stratospheric exchange of oxygen depends on the presence of CO₂. This is explained in detail on page 437. Data (which to our knowledge does not exist for O₂) can only give the present day situation and will not answer the scaling factor.</p>
<p><i>Page 450, line 13-15: Why do you consider a CO₂ concentration dependent fractionation of photorespiration but do not allow temperature dependencies? Is this more important?</i></p> <p>This is a misunderstanding. The amount of photorespiration is CO₂ concentration dependent. The contribution of photorespirative fractionation to the total therefore depends on the CO₂ concentration. As CO₂ and temperature (globally) correlate to a high degree over glacial interglacial times you may also argue that we actually scale the C₄/C₃ partition to temperature.</p> <p>The reviewer may have a point: Photorespiration scales with both T and CO₂, and we are accounting only for CO₂. T scaling however is I believe smaller than CO₂ scaling, and we might say this in the text.</p>
<p><i>Page 450, line 23-24: Continental productivities. . .give reference.</i></p> <p>We are discussing "continental productivities in ice free areas about 45°N during an ice age". These areas are in front of glaciers and (by modern analogy) those areas are generally dry and therefore low productive. A reference can be found if desired.</p>
<p><i>Page 451, line17: we adopt values 4-7.C . . . , for which regions do you adapt this, globally averaged? If yes, how was this averaged?</i></p> <p>All our modeling average globally not distinguishing individual regions.</p>
<p><i>Page 453, line 1-2: Is the GPP scaling to atmospheric CO₂ concentration changes adequate having in mind the influence of the ocean's chemistry on the atmospheric CO₂ concentration?</i></p> <p>Joos et al., 2004 show that there is a good correlation between land productivity and CO₂ over the last transition which covers the mentioned ocean chemistry change. Therefore we argue that yes it is adequate.</p>
<p><i>Page 453, line 19: What is the uncertainty for Joos GPP estimates? What kind of uncertainty does this imply for the humidity adjustments?</i></p>

R ² for the correlation is 0.997. However, we project that correlation over 400 kyr and one might wonder how big the uncertainty is. There is probably no way in knowing. However, we indirectly incorporate a large uncertainty for the land productivity into our calculations which likely covers that uncertainty.
<i>Page 453, line 26ff Has the isotopic composition of the precipitation now an influence or not? And if yes, wouldn't it be worthwhile to do a similar approach for the isotopic composition of the precipitation as you have done for the humidity approach?</i> On the approach finally taken what happens is the following. By changing the d18O of precipitation we will obtain a different solution for the free parameter which is global GPP weighted humidity. The resulting change on the productivity is however minimal. For that reason we neglect further on potential changes in the signature of precipitation water.
<i>Page 454, line 24-27: Wouldn't it be worthwhile to use values from an model such as the Bicycle model (Köhler et al., 2010) that has been run for the entire 400kyrs?</i> Not really. This is a simple carbon cycle model. What is really needed here is a biome model coupled to a GCM that includes the hydrological cycle.
<i>Page 455, eq.9: This is very vague since applied to the industrial change this equation would lead to smaller fluxes for CO2 concentrations above 380 ppm. Do you allow for a time-delay in equation 9?</i> The industrial change is not part of that correlation nor to the original study from Joos et al., 2004. No time delay is allowed for.
<i>Page 455, line 8-9: The C4 variations that are assumed for full glacial conditions are very large.</i> This number represents the range of numbers we found in the literature (see appendix)
<i>Page 456, line 19: The ocean productivity is not only marginally higher with 29 ± 20%!</i> This wording will be adapted in a revised version
<i>Figure 2: 100% is not defined, but the reviewers guesses that it corresponds to today's conditions. If this is true, the C4 contribution today would be more than 50%. This would be far too high for today's conditions [Still et al., 2003].</i> See comment to point 1 and 2

Technical comments:

<i>Page 446, line 9: ...kinetics and equilibrium processes ...</i> Changed
<i>Page 452, line 18: ...are larger for land than for ocean productivities ...</i> Changed

References:

Köhler, P., H. Fischer, and J. Schmitt (2010), Atmospheric $\delta^{13}C_{CO_2}$ and its relation to pCO_2 and deep ocean $\delta^{13}C$ during the late Pleistocene, *Paleoceanography*, 25, PA1213, doi:10.1029/2008PA001703.

Leuenberger, M. C., "Modeling the signal of seawater $\delta^{18}O$ to the $\delta^{18}O$ of atmospheric oxygen using a diagnostic box model for the terrestrial and marine biosphere", *Journal of Geophysical Research*, 102/C12, 26841-26850, 1997

Still, C. J., J. A. Berry, G. J. Collatz, and R. S. DeFries (2003), Global distribution of C3 and C4 vegetation: Carbon cycle implications, *Global Biogeochem. Cycles*, 17(1), 1006, doi:10.1029/2001GB001807.

Clim. Past Discuss., 8, C421–C429, 2012
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**Climate
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**Interactive comment on “Planetary fertility during
 the past 400 ka based on the triple isotope
 composition of O₂ in trapped gases from the
 Vostok ice core” by T. Blunier et al.**

J. Kaiser (Referee)

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Received and published: 22 May 2012

Blunier et al. use O₂ isotopologue measurements of air extracted from the Vostok ice core to constrain paleo-productivity on land and in the ocean. They use a box model for their calculations, which requires a large number of parameters (Tables 1 and 2). A formal uncertainty analysis is not presented, but according to the paper, the main factors contributing to the uncertainty, next to the analytical uncertainty itself, are relative humidity and the fraction of C₄ plants. Relative humidity is constrained by the 170 excess of water, but measurements of its value are inconsistent across different ice cores. Since the mass balance calculation is very sensitive to relative humidity, the results have significant uncertainties and additional estimates of paleo-production of the land biosphere from the literature are invoked to reduce these uncertainties. The authors are to be highly commended for this major effort, which despite the uncertainties, is a valuable contribution to answer the question how productivity changed between glacial and interglacial times.

My main concern with the paper in its present form is the lack of an uncertainty analysis or sensitivity study into how the measurement uncertainty in $\delta(180)$ and $\delta(170)$ [or 170 excess] as well as parameters other than relative humidity and C₄ fraction propagate into the calculated productivities.

We do intend to interpret an individual datum. Therefore the analytical uncertainty is of minor importance. However, after averaging, analytical errors are small.

In 2011, a number of new measurements of some of these parameters have been published and it is currently unclear to what extent the proposed changes might affect the results in the present paper. A corresponding analysis should be added to the discussion, drawing in the new measurements, as detailed below.

*1) Barkan and Luz revised their measurement of $\delta(170)$ in VSMOW rel. to Air- O₂ from -11.93 to -11.88 ‰ (Barkan, E., and Luz, B.: The relationships among the three stable isotopes of oxygen in air, seawater and marine photosynthesis, *Rapid Commun. Mass Spectrom.*, 25, 2367-2369, 10.1002/rcm.5125, 2011.) How does this affect the results? Note that these revised measurements contradict with results from Kaiser and Abe (Kaiser, J., and Abe, O.: Reply to Nicholson's comment on "Consistent calculation of aquatic gross production from oxygen triple isotope measurements" by Kaiser (2011), *Biogeosciences Discuss.*, 8, 10517-10541, 10.5194/bgd-8-10517-2011, 2011.)*

Barkan and Luz revised the value for 17D of H₂O vs. atmosphere several times. We based on analyses on their 2005 value of 173 per meg. Using the larger value they found before and after 2005 in our calculations has a significant effect. In the attached figure R1/R2 larger $^{17}\Delta$ results in making the ocean productivity largely independent from the land productivity (panel b and c).

2) Luz and Barkan (2011) suggest that there is isotopic fractionation during photosynthesis (of about 4 ‰ for 18epsilon) and that the marine (surface) Dole effect is in fact (23.7±1.8) ‰ How does this affect the results?

With our approach of changing relative humidity the effect of the proposed fractionation of photosynthesized O₂ from the ocean is surprisingly small. See figure R1 b pink and blue lines

*3) The bulk air stratosphere-troposphere exchange flux may have changed when temperature and CO₂ mole fraction of the atmosphere changed (Rind, D., Lerner, J., McLinden, C., and Perlwitz, J.: Stratospheric ozone during the Last Glacial Maximum, *Geophys. Res. Lett.*, 36, 2009.) How much does an increase or decrease of perhaps 20 % affect the results?*

For an increase in the exchange flux the O₂ production has to increase to compensate for the increased flux of $^{17}\Delta$ light O₂ from the stratosphere. The increase is of the same order of magnitude (See figure R2 orange vs. blue line). However, according to Rind the global ozone change is only a few percent.

I'd also like to suggest that the used terminology regarding 17O is potentially confusing and should be revised. E.g., it would be better to refer to 17Delta simply as 17Delta, or perhaps 17O excess. Use of the word "anomaly" opens up large number of questions, such as what isotope composition is "normal" (which may vary in modern and glacial times), effects of different mass-dependent processes on 17Delta, mixing etc. Really, 17Delta is a convenient definition to facilitate easier discussion of small quantities and I would suggest it to avoid terms such as "anomaly" and "mass-independent fractionation" as far as possible. In particular, the term "fractionation" refers to a process (or isotope effect associated with it), not a delta value (which is measured relative to an arbitrary standard).

We agree that the term "anomaly" is unfortunate. It was common when the stratospheric effect was found and made sense as it is anomalous to what one was used to. We are not suggesting that any of the processes is not normal therefore we use the term strictly with quotes and in general descriptions of effects. We are also using the term referring to previous publications where they have been used in exactly that way.

"Excess" has exactly the same problem as "anomaly". We're talking about something that we didn't originally think was going to be there, but now we know why it's there.

Have all co-authors agreed to the publication of this paper? The affiliation of Bruce Barnett has changed - I think he is at Duke University now. von Fischer is spelled with "sch". Is his affiliation correct?

Yes and corrected.

Quantity symbols should be used, e.g. instead of 18O/16O, use n(18O)/n(16O) or R(18O/16O). Also, x(CO2) or y(CO2) instead of CO2 to indicate mixing ratios.

Strictly speaking Jan is right. However, it makes the nomenclature heavier without, in our view, adding clarity. For clarity we might include a short definition of what we mean by these terms when they are first used.

Specific comments:

436/5 + 436/10: This is contradictory/unclear. It is correct that the slope of ln(1+delta17O) vs. ln(1+delta18O) in stratospheric CO2 is 1.7. This is transferred to O2 via isotope exchange. It is less obvious (and unlikely) that there is non-mass dependent fractionation during the exchange. Both sentences should be rephrased. Perhaps the word "mass-independent" could simply be deleted in both cases.

Changed as proposed.

436/12: Would 17O excess be a more appropriate term than anomaly?

removed

436/12: Replace "analysis" by "use" or something similar to avoid confusion with the actual measurement process.

We suggest replacing "analysis" with "interpretations".

436/14 and 437/23: A single isotope cannot be fractionated.

fixed

437/23: I don't think the exchange is "anomalous". More likely, the 17O excess is transferred from O3 to O(1D) and then to CO2 (e.g. Shaheen, R., Janssen, C., and Röckmann, T.: Investigations of the photochemical isotope equilibrium between O2, CO2 and O3, Atmos. Chem. Phys., 7, 495-509, 2007; and references therein). The section needs to be rewritten to ensure accurate description of the atmospheric chemistry involved.

Nothing that nature does is anomalous. The term is used to distinguish the bulk of biochemical reactions from the stratospheric effect. We are not describing the atmospheric chemistry involved but only the effect; and that is all that matters in the scope of this manuscript. We changed the wording to be more clear.

*437/25: Mass-dependent fractionation is described in ln(1+delta) space. What is the reference for these delta values? Actually, delta17O (12.08 ‰ vs. VSMOW) is more than 0.5 delta18O (0.5*23.88 ‰ = 11.94 ‰ Barkan and Luz, 2005).*

We try to recapitulate a complicated process into a simple statement, obviously with little success. We will reformulate.

438/1: Mass-independent anomaly is a tautology. Perhaps use "17O excess"?

Fixed

438/17: Luz and Barkan (2011; Luz, B., and Barkan, E.: The isotopic composition of atmospheric oxygen, Global Biogeochem. Cycles, 25, GB3001, 10.1029/2010gb003883, 2011) should be cited here.

We prefer citing this work in section 4.1 where the isotopic fractionation of photosynthesis (the topic of the manuscript suggested) is discussed.

439/21: To show clearly that this is a definition a triple equal (identity) sign (≐) should be preferred.

Agreed

<p><i>439/23: It should be clarified that the data are reported to oxygen in modern air.</i></p> <p>Added "modern"</p>
<p><i>439/23: The sentence "It is an approximation for the exact ratio of 17O/18O fractionation during processes influencing the isotopic composition of O2 in air (Luz and Barkan, 2005)." should be deleted because the choice of lambda is explained better in the next paragraph.</i></p> <p>This statement is needed here since we need to tell which lambda we used in our calculations.</p>
<p><i>439/24: As it stands, Delta17O should be replaced by 17Delta and the equal (=) sign be replaced by an approximately equal (≈) sign to show that is an approximation. Alternatively, if this sentence is meant to say that another definition was used then it should be rephrased and the equal sign be replaced by an identity sign (≡). Any definition would be acceptable, since 17Delta is essentially used as a (mathematically convenient) tracer rather than reflecting a physically observable quantity. In any case, a better choice of symbols would be helpful, e.g. using indices, to differentiate better between 17Delta and Delta17O</i></p> <p>We disagree. We simply point out that we used a different definition in our previous publication. We think the phrase says exactly that.</p>
<p><i>440, 1st paragraph: This section confounds definitions of 17Delta with expectations for mass-dependent relationships under certain conditions. I am not sure whether the chosen definition "works best" or that the coefficient of 0.516 is "optimal". It would be sufficient to say that a coefficient of 0. reflects the expected relationship between 17delta and 18delta for mitochondrial respiration fractionation with $17\alpha - 1 = 0.518 * (18\alpha - 1)$ in steady-state with production, perhaps referencing Angert, A., Rachmilevitch, S., Barkan, E., and Luz, B.: Effects of photorespiration, the cytochrome pathway, and the alternative pathway on the triple isotopic composition of O2, Global Biogeochem. Cycles, 17, 1030, doi:10.1029/2002GB001933, 2003. As the authors observe, there is no single mass-dependent fractionation line and other MDF processes can create non-zero 17Delta values. 17Delta is therefore more akin to a mathematically convenient tracer, rather than an actual observable, which is also reflected by the separate treatment of 17O and 18O in the model. Consequently, the linear definition of 17Delta may actually be preferable because it behaves conservatively with respect to mixing. I've discussed this in Kaiser, J.: Technical note: Consistent calculation of aquatic gross production from oxygen triple isotope measurements, Biogeosciences, 8, 1793-1811, 10.5194/bg-8-1793-2011, 2011 and Kaiser, J., and Abe, O.: Reply to Nicholson's comment on "Consistent calculation of aquatic gross production from oxygen triple isotope measurements" by Kaiser (2011), Biogeosciences Discuss., 8, 10517-10541, 10.5194/bgd-8-10517-2011, 2011.</i></p> <p>While Jan is correct we do not think it makes sense to add another paragraph to justify the lambda we pick. As Jan pointed out earlier it is a definition and for what matters here a choice.</p>
<p><i>440/22: The meaning of "per meg" should be explained. In Kaiser (2011), I pointed out that the symbol "ppm" for 10⁻⁶ is more widely accepted by international bodies and more widely known in general.</i></p> <p>The choice of ppm is very confusing as it normally applies to concentrations which some prefer to call mixing ratios (another area of constant discussion about wording). Isotope ratios are clearly not mixing ratios therefore one should not use ppm as a unit for them.</p> <p>We added (1 per meg = 0.001%) which was indeed missing.</p>
<p><i>441/23: The following sentences are potentially confusing because of the terms "anomalous" and "mass independent fractionation": "However, this standard is itself anomalous: the 17_ of air O2 is lowered by 170 per meg vs. SMOW using values found by Barkan 25 and Luz (2005). As mass-independent fractionation decreases, 17_ of O2 with respect to modern air increases, reaching +170 per meg when there is no mass independent fractionation." Since modern Air-O2 is defined as reference in Eq. (1), it is unclear where the reference to SMOW [which should actually be VSMOW] comes from. Obviously, this is because the isotopic composition of seawater is at the basis of that of photosynthetic O2, however this has not been explained at this stage. It would be sufficient to say that as CO2 decreases during glacial times, there is less preferential transfer of 17O from O2 to CO2 in the stratosphere and correspondingly, O2 is left more 17O (and 18O-rich). Note that the value of 170 ppm is given as 173 ppm in Barkan and Luz (2011); Barkan, E., and Luz, B.: The relationships among the three stable isotopes of oxygen in air, seawater and marine photosynthesis, Rapid Commun. Mass Spectrom., 25, 2367- 2369, 10.1002/rcm.5125, 2011. In any case, the same paper has revised the value to 223 ppm, as mentioned above.</i></p> <p>The difference between 170 and 173 per meg arises from the precision that is given in Luz et al., 2005. We rechecked our calculations and found that (opposed to our expectations) changing the atm to ocean difference in 17D has a larger influence on the outcome of our calculations.</p> <p>Responding to points 1 and 2 we made some additional calculations (see also attached figure R1) that can easily be incorporated into a revise version of the manuscript.</p>
<p><i>442/4: Do you mean present-day air?</i></p> <p>No here we refer to figure 1 (reference added)</p>

<p><i>442/9: What are the uncertainties of these values?</i></p> <p>R2 is given in Figure A1. However, as Jan is certainly aware of, and discussed right below, this slope will not be constant. The reason to discuss this simplified approach is to demonstrate that there is more than the CO₂ dependence in 17D of O₂.</p>
<p><i>442/19: Do you mean present-day air?</i></p> <p>Yes</p>
<p><i>443/5: What do you mean by "ultimate"? Ts the terms biogeochemical and hydrological cycle imply that there is no beginning or end.</i></p> <p>Due to the size of the ocean reservoir versus the rest one can well use that term. Also it is true that O₂ comes out of the ocean since at the beginning of time there was no atm. O₂ in the system.</p>
<p><i>443/16: Luz and Barkan (2011; Luz, B., and Barkan, E.: The isotopic composition of atmospheric oxygen, Global Biogeochem. Cycles, 25, GB3001, 10.1029/2010gb003883, 2011) suggest there is isotopic fractionation during photosynthesis and Equation (2) should be updated to reflect this possibility.</i></p> <p>See comments to Jan's points 1 and 2. Will be added.</p>
<p><i>445/3: O2 should be deleted after "mol" because it is not a unit. The unit "mol" does not have an "e" at the end.</i></p> <p>Corrected</p>
<p><i>445/3: O2 should be deleted after "mol" because it is not a unit. The unit "mol" does not have an "e" at the end.</i></p> <p>It is not part of the unit but it needs to be indicated that this is mol O₂ not O which otherwise would not be clear.</p>
<p><i>446/18: This is not a fact, but an assumption.</i></p> <p>True. Added "closely" to the sentence</p>
<p><i>447/21: Is the Bender et al. (2000) reference correct? What ratio did you use?</i></p> <p>Should be: Bender, M., J. Orchardo, M. L. Dickson, R. Barber, and S. Lindley (1999), In vitro O₂ fluxes compared with ¹⁴C production and other rate terms during the JGOFS Equatorial Pacific experiment, Deep-Sea Res Pt I, 46(4), 637-654.</p>
<p><i>447/23: Seawater is depleted in 17O by 5 ppm relative to VSMOW (Luz, B., and Barkan, E.: Variations of 17O/16O and 18O/16O in meteoric waters, Geochim. Cosmochim. Acta, 74, 6276-6286, DOI: 10.1016/j.gca.2010.08.016, 2010.).</i></p> <p>This corresponds to a change in standard in our calculations. As this affects all our calculations we do not expect any effect on the relative changes.</p>
<p><i>449: What are the uncertainties of the global mean humidity and the leaf water isotope composition?</i></p> <p>Those numbers should not be taken face value. E.g. changing the value for ocean water vs. atmosphere from 170 to 220 per meg changed the outcome moderately. However, it has a large effect on humidity and leafwater.</p>
<p><i>450/25: Again, the term "anomaly" might create improper expectations; "17O excess" would be more neutral.</i></p> <p>We definitely agree that there is nothing anomalous. We use the term to distinguish O₂ that was fractionated heavily in 17O vs 18O coming from the stratosphere. We think we made this clear in the introduction. The 17O excess as Jan likes to call it refers to the resulting signal in the troposphere relative to a well-defined standard. In that respect it is not identical to our term and cannot be replaced by it.</p>
<p><i>452/21: I think a new section heading might be appropriate here or a division of section 4.2 into two parts.</i></p> <p>Done</p>
<p><i>453/5: There is a contradicton here to 444/15, where the fraction of C4 is stated as 27.5 %.</i></p> <p>The number here is for glacial. On page 444/15 it is for present.</p>

Table 1: I could not quite reproduce some of the figures in Table 1, based on the given references. The coefficient lambda should be renamed because it is already used in the definition of 17Delta (Eq. 1). Alternatively, lambda in Eq. 1 should perhaps be renamed lambda_ref (or omitted).

Adopted

Mehler reaction: Helman et al. (2005) give gamma = 0.526. This results in 17eR = -5.681 ‰ not -5.685 ‰

Mehler reaction fractionation comes with diversity. 0.526 is for (*Pisum sativum*) but Helman states a lower value for *Synechocystis* sp. strain PCC 6803 (0.497). The weighted average is 0.525.

Helmann give another number for Photorespiration: Based on the assumption that for every two O2 molecules converted by Rubisco, one is converted by glycolate oxidase (Tolberg, 1997 as cited in Helman et al. 2005), the weighted average should be -21.367 ‰ for 18eR and -10.932 ‰ for 17eR.

This is the case when weighted in delta space, which is not absolutely correct. When averaged in ratio space (also not absolutely correct) the number we give is correct.

Alternative oxidase: Based on gammaR = 0.5179 and 18e = -30 ‰ 17e should be -15.537 ‰

Gamma for alternative oxidase is 0.514 according to Angert et al., 2003.

For the weighted averages, I obtain 18eR = -18.020 ‰ for modern (rather than -17.998 ‰ and -18.320 ‰ for LGM (rather than -19.161 ‰. Similarly, for 17eR.

The glacial average cannot be reproduced directly from the table as some fractionation factors are temperature dependent and this is taken into account. We can explain better in the caption..

Fig. 2: This figure is difficult to read due to the 3D projection. It would be better to show a series of 2D projections or if quantitative interpretation was not desired, then these 2D projections should be included as an Appendix.

This figure illustrates the range of uncertainty associated with the uncertainty of the input parameters. We feel that putting all results into this figure passes our message that there are “too many unknowns to derive both land and ocean fluxes”. We also feel that we may improve the caption.

Fig. A1: I think doing a regression with interpolated data might give different results than using the sparser original data. What regression coefficients and R² values result if the original data are used instead? If there is any bias, what effect does this have on the productivity calculations?

It will be different but 1) the data spacing is pretty consistent throughout the record therefore the effect is small and 2) this is only an illustration that has absolutely no effect on the following calculations.

Fig. A2: What are the regression coefficients and R² value?

The regression coefficient is given in the accompanying text.

R Square=0.999575045

Fig. A3: What are the regression coefficients and R² value? How do the results compare to the Benson and Krause (1979) measurement of the equilibrium isotope fractionation of O2, see Benson, B. B., Krause, D., and Peterson, M. A.: The solubility and isotopic fractionation of gases in dilute aqueous solution. I. Oxygen, J. Solution Chem., 8, 655-690, 1979.

The regression coefficient is given in the accompanying text.

R Square=0.58778192

Benson and Krause 1984 give $\delta^{18}\text{O}$ (‰)=0.833243639 -0.005156667*T(°C) resulting in values within our calculated uncertainty between 5 and 25°C.

Technical corrections:

Throughout the manuscript incl. the title, the term "fertility" should be replaced by "productivity" because "fertility" has a connotation of potential production and is mainly used in agriculture.

Our use of “fertility” is intended to conjure a broader image in the minds of readers, paralleling the fundamental importance of the observations and analyses that we have conducted in this manuscript. Oxygen productivity is ultimately a function of the greenness of the biosphere, and therefore its capacity to support life. Fertility is a term beyond agriculture and includes the potential to generate offspring. Our work addresses some of the most fundamental processes for life on earth, and therefore we find “fertility” to be a suitable term here. However, we could also use production instead (similar to our response to reviewer 1).

Also, "mass-independent" should be replaced with "non-mass dependent" (it does not follow a mass-dependent law and the fractionations for 17O/16O and 18O/16O are not equal, i.e. they are not massindependent).

Mass-independent was the term introduced in previous publications. As lined out before in our response it makes sometimes sense to use that term to refer to earlier findings, although we agree that in a physical sense it is incorrect.

436/15, 438/18+19+20, 441/3, 452/24, 454/23+24, 458/9: Replace "kyr" with "ka" for consistency and as per international conventions. Done
442/23, 443/18, 444/8, 444/13, 445/3, 447/23, 449/24: Replace "yr" with "a" for consistency and as per international conventions. Done
437/9: "Ocean and land ..." Done
437/18: Fractionation is a process, not a state; "mass-independent fractionation" should be replaced by 17O excess or a similar term. Replaced by signal
437/18: Replace "is encoded" with "influences". We think encoded is the right term here. In other words the rate of photosynthesis needs to be decoded from the atmospheric signal.
439/4: Replace "cc" by cm ³ or cubic centimetres. Done
439/5: "Louwers-Hapert" Done
439/13: "separated chromatographically to remove other gases, e.g. N ₂ " We are separating O ₂ , Ar from all other gases. The formulation above is not saying this but the original text does.
443/12+13: "isotopomer" should be replaced by "isotopologue" corrected
443/18, 447/22: The unit should be "mol a ⁻¹ ". done
444/13: Is "burned" the right word? "Burned" is a correct formulation but it could be changed.
448/21: The equation should be numbered. done
449/14+28: Flux should have units of amount per time (mol/a). done
449/18: "decreases" done
449/23: Please define the symbol ppb or use SI units for the mole fraction (nmol/mol). done
451/9: BP should be defined or spelled out. done
453/7: "than modern" done
455/23: "is" should be replaced by "would be" done
456/21: Replace "fertile" by "productive and contain more chlorophyll" done
459/10: The left hand side should be 10 ⁻³ epsilon or epsilon/‰ done