Clim. Past Discuss., 8, C131–C135, 2012
www.clim-past-discuss.net/8/C131/2012/

Climate of the Past Discussions

Interactive comment on "Planetary fertility during the past 400 ka based on the triple isotope composition of O_2 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-

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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.

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

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

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

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

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).

Page 440, line 6: references for coefficients

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

CPD
8, C131–C135, 2012

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.

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.

Page 442, line 8: Why 0.55 per meg? Isn't it just the division of -170/280 = -0.61 per meg/ppm?

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.

Page 443, line 8: A reference to Leuenberger (1997) might be adequate.

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.

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.

Page 450, line 8: When assuming no change in several fractionation factors then you do not allow for temperature dependent fractionations. Is this justified?

Page 450, line 9-12: Why should the net oxygen fluxes scale to the CO2 concentration?

CPD

8, C131–C135, 2012

Is this only based on models or also on data?

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? Page 450, line 23-24: Continental productivities. . ..give reference.

Page 451, line17: we adopt values 4-7°C . . ., for which regions do you adapt this, globally averaged? If yes, how was this averaged?

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?

Page 453, line 19: What is the uncertainty for Joos GPP estimates? What kind of uncertainty does this imply for the humidity adjustments?

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?

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?

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?

Page 455, line 8-9: The C4 variations that are assumed for full glacial conditions are very large.

Page 456, line 19: The ocean productivity is not only marginally higher with 29 $\pm$ 20%!

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].

CPD

8, C131–C135, 2012

Technical comments: Page 446, line 9: . . .kinetics and equilibrium processes . . ..

Page 452, line 18: . . .are larger for land than for ocean productivities. . .

References: Köhler, P., H. Fischer, and J. Schmitt (2010), Atmospheric δ13CO2 and its relation to pCO2 and deep ocean δ13C during the late Pleistocene, Paleoceanography, 25, PA1213, doi:10.1029/2008PA001703.

Leuenberger, M. C., "Modeling the signal of seawater δ18O to the δ18O 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.

Interactive comment on Clim. Past Discuss., 8, 435, 2012.