

## **Review for *Terrestrial biosphere changes over the last 120 kyr and their impact on ocean $\delta^{13}\text{C}$* by Hoogakker et al.**

In their study Hoogakker et al. nicely compile BIOME maps using pollen records and model simulations. From the simulated biosphere changes they infer plant productivity and terrestrial carbon storage, and by using budget equations finally the ocean  $\delta^{13}\text{C}$ . While I very much appreciate their effort in compiling BIOME maps and vegetation distributions over the past 120 kyr, I am not convinced by their conclusions on  $\delta^{13}\text{C}$  changes. The manuscript itself is well organised and written. I thus encourage publication in CP after a major revision.

### General:

The compiled BIOME maps from pollen data sets are very informative and useful for the paleo data and model community. In addition there are very few simulations over the past glacial-interglacial cycles with a fully coupled model. So, it is good to see that these simulations are evaluated against paleo data.

My main critics concern the interpretation of the model results. In my opinion the approach to reconstruct ocean  $\delta^{13}\text{C}$  is too simplistic and without recognition of available evidence. Therefore, I am not convinced that terrestrial carbon stock changes have the dominant role in ocean  $\delta^{13}\text{C}$  changes over the past 120 kyr. It could well be true, but there are a lot of assumptions involved and other mechanisms, e.g. ocean water mass changes (Bereiter et al., 2012) that possibly could explain the observations. As a neutral reader I would expect a more thorough calculation and uncertainty consideration of carbon stock and  $\delta^{13}\text{C}$  changes. Here my suggestions:

1. The Vostok CO<sub>2</sub> record is outdated and often lower by 10-20 ppm than newer data. Use a composite record of newer data sets (see Figure below). Also use the common timescale AICC2012 (Veres et al. 2012). 20 ppm can greatly affect NPP and thus carbon storage.
2. Show the uncertainty and impact of atmospheric  $\delta^{13}\text{C}$  on ocean  $\delta^{13}\text{C}$  using ice core records for the last 20 kyr (Schmitt et al., 2012) and MIS 5 (Schneider et al., 2013). Even though the majority of the  $\delta^{13}\text{C}$  signal is transferred to the ocean it could give you an indication on the direction of change.
3. One shortcoming of the  $\delta^{13}\text{C}$  analysis is the missing of peatlands and permafrost carbon stocks in the model, as mentioned in the beginning of the paper. They act exactly on these long time scales that matter for the terrestrial carbon change over the observed period, and also have an opposite effect on carbon storage compared to e.g. forest ecosystems. According to Ciais et al., 2012, as you write, the inert carbon stock was larger by ~ 700 PgC during the LGM compared to PI. Assuming a linear increase in permafrost carbon between the previous interglacial and the LGM: How would the increasing carbon storage in permafrost areas affect the  $\delta^{13}\text{C}$  budget? Please discuss and see below for more specific comments.
4. The equations used to estimate carbon storage from NPP has underlying assumptions that are questionable. It is assumed that soil carbon is in steady state at each time step in the past, which is wrong for ecosystems with a turnover time

larger than the model time step (1000 years for FAMOUS I guess), i.e. for wetland ecosystems or again permafrost areas. Further, turnover times are estimated from present day soil carbon storage. Again it is assumed that for current conditions soils are in steady state, in a time of rising temperature, CO<sub>2</sub>, and nutrient input. A discussion of the implications is needed here.

Given the richness of BIOME data and the complexity of the climate models used in this study I think the analysis of  $\delta^{13}\text{C}$  falls short. I don't think new climate simulations are needed, but additional simulations with BIOME4 for sensitivity tests and a thorough uncertainty estimate (1 sigma band for land and ocean  $\delta^{13}\text{C}$ ) would considerably improve the statement of the paper.

### Specific:

p. 1039, l. 2: The Vostok CO<sub>2</sub> record is at least 16 years old and outdated by records with higher temporal resolution and measured by more accurate techniques (e.g. direct measurements by ice sublimation). Please replace it with data from newer ice cores (see Figure and References below).

p. 1943, l.2: should this be Fig. 1 or Table 1? Could not find reference to Fig. 1 in text.

p. 1049, l.11: Are HadCM3 surface temperatures absolutely 1°C colder at present or is the LGM-present anomaly 1°C colder? In the first case this should not matter, when you use anomalies for BIOME4. Please clarify.

p.1052, l. 19: The interpretation of the "sahara greening" in the model is at its limit, when only a hand full of grid cells swap color at this coarse resolution. In general the description for comparing model grid cell changes could be shortened and less speculative.

p. 1060, l. 23: Is the CO<sub>2</sub> fertilization effect or the CO<sub>2</sub> climate effect more dominant for NPP? Could this be tested with a BIOME4 simulation with constant CO<sub>2</sub>?

p. 1061, l. 7: These numbers directly depend on prescribed CO<sub>2</sub>. Using an updated CO<sub>2</sub> record (see Figure below) should result in e.g. smaller differences between the Eemian and the Holocene.

p. 1092, l. 24: Which soil carbon data has been used for the calibration of the turnover times?

p. 1063, l22: If I would argue that NPP is dominated by CO<sub>2</sub> fertilization, as the curve in Fig. 5 visually correlates with the CO<sub>2</sub> record, would you still get a precessional cycle in terrestrial carbon storage with constant CO<sub>2</sub> in BIOME4? Using the updated CO<sub>2</sub> record may change the periodicity. Please reassess.

p. 1064, l. 19: Replace 'decrease' by 'difference' as the former has a time direction associated. Time runs from LGM to PI.

p. 1065, l. 6: Please use updated CO<sub>2</sub> (see Figure below) and  $\delta^{13}\text{C}$  (Schmitt et al., 2012; Schneider et al., 2013) records for the atmospheric part of the budget.

p. 1066, l. 2: You could mention that biomes do not include permafrost (normally C3

plants) and peatlands (C3 plants and sphagnum moss with  $\delta^{13}\text{C} = \sim -30$  per mill). Having said that, please also clarify that the variability of terrestrial  $\delta^{13}\text{C}$  in Fig. 6a is of secondary importance for ocean  $\delta^{13}\text{C}$ . What matters are terrestrial carbon storage changes.

p. 1066, l. 14: This is correct, but only because both models lack inert carbon pools. If you include them like in Ciais et al., 2012, then the FAMOUS model would agree better (see paragraph 4.3. in your own words).

p. 1066, l. 24: Please also cite Bereiter et al., 2012.

p. 1067, l. 5ff: This statement is too strong, I'm not convinced. I believe that the trend in modelled ocean  $\delta^{13}\text{C}$  from MIS 5 to MIS 2 may be robust, but not the variability in between, e.g. the variability from HadCM3 climate is rather small.

p. 1068, l. 25: Again, I'm not convinced by the presented material that the role of land  $\delta^{13}\text{C}$  is "dominant" for ocean  $\delta^{13}\text{C}$ . See General comments.

p. 1068, l. 13: This is very valuable and a good reason this paper deserves publication after a revision.

References: Ciais et al., 2011 should be Ciais et al., 2012 in the entire text.

Figure 2: What does (a) and (b) signify? Is there any difference between plots on top right and left? Please enlarge this figure panel in two figures for better visibility.

#### References:

Ahn J., Brook E. J. (2008) Atmospheric CO<sub>2</sub> and climate on millennial time scales during the last glacial period. *Science* 322:83–85.

Ahn, J., et al. (2012), Abrupt change in atmospheric CO<sub>2</sub> during the last ice age, *Geophys. Res. Lett.*, 39, L18711, doi:[10.1029/2012GL053018](https://doi.org/10.1029/2012GL053018).

Bereiter, B., et al., (2012) Mode change of millennial CO<sub>2</sub> variability during the last glacial cycle associated with a bipolar marine carbon seesaw, *Proceedings of the National Academy of Sciences of The United States of America*, 109/25, 9755-9760

Ciais, P., et al. (2012) Large inert carbon pool in the terrestrial biosphere during the Last Glacial Maximum, *Nat. Geosci.*, 5, 74–79, 2012.

MacFarling Meure, C., et al. (2006), Law Dome CO<sub>2</sub>, CH<sub>4</sub> and N<sub>2</sub>O ice core records extended to 2000 years BP, *Geophys. Res. Lett.*, 33, L14810, doi:[10.1029/2006GL026152](https://doi.org/10.1029/2006GL026152).

Monnin E., et al. (2001) Atmospheric CO<sub>2</sub> concentrations over the last glacial termination. *Science* 291:112–114.

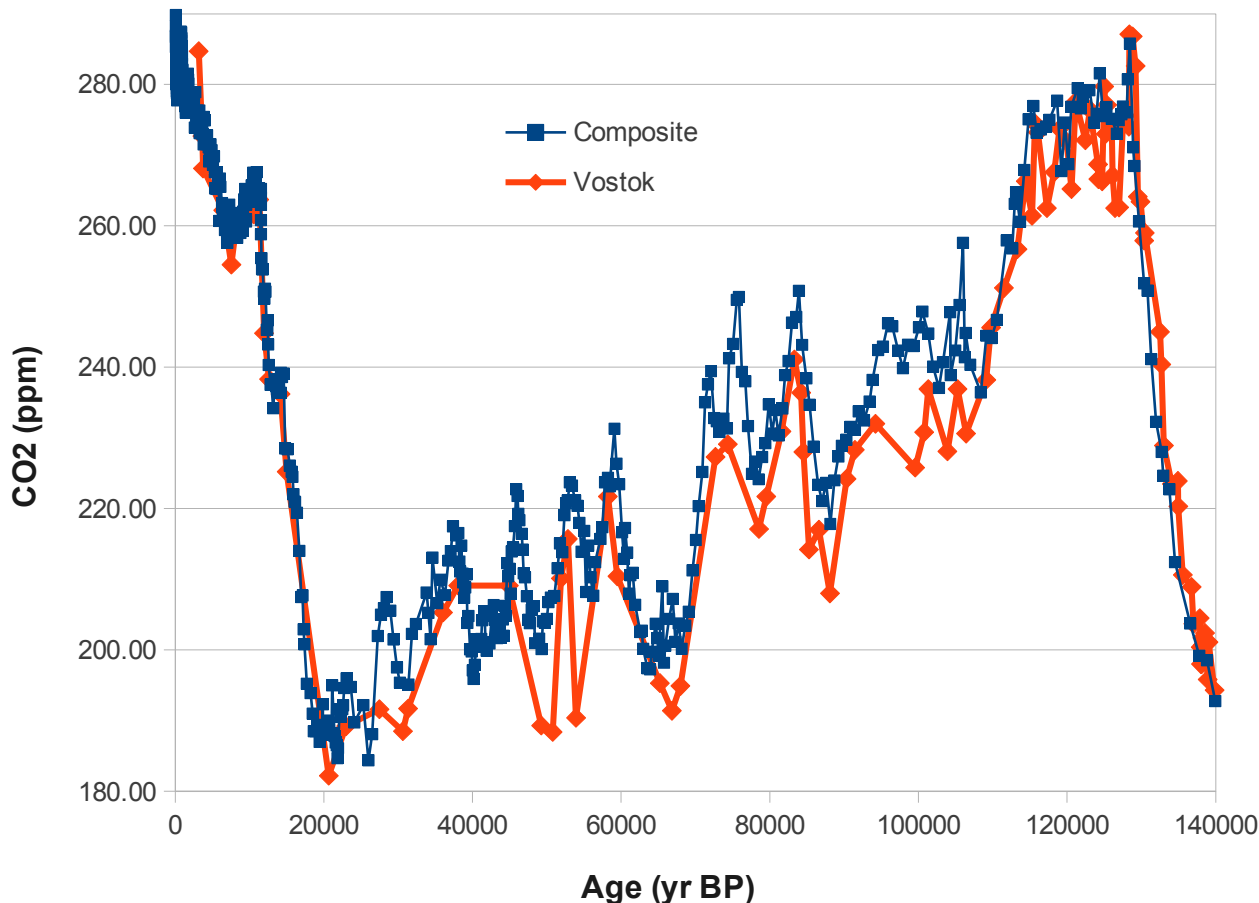
Schmitt, J., et al. (2012) Carbon isotope constraints on the deglacial CO<sub>2</sub> rise from ice cores, *Science*, 336, 711–714.

Schneider, R., et al. (2013) A reconstruction of atmospheric carbon dioxide and its stable

carbon isotopic composition from the penultimate glacial maximum to the last glacial inception, *Climate of the Past*, 9, 2507-2523.

Veres, D., et al. (2013) The Antarctic ice core chronology (AICC2012): an optimized multi-parameter and multi-site dating approach for the last 120 thousand years, *Clim. Past*, 9, 1733-1748, doi:10.5194/cp-9-1733-2013

Figure: Composit record following Bereiter et al., 2012.



Composite CO2 record on AICC2012 (Veres et al., 2012)

- 46 - 10 yr BP: Law Dome (MacFarling Meure et al., 2006)
- 0 - 1 kyr BP: WAIS (Ahn et al., 2012)
- 1 - 2 kyr BP: Law Dome (MacFarling Meure et al., 2006)
- 0 - 22 kyr BP: Dome C (Monnin et al. 2001)
- 22 - 24 kyr BP: Dome C Sublimation (Schmitt et al., 2011)
- 24 - 38 kyr BP: Byrd (Ahn et al., 2008)
- 38 - 60 kyr BP: TALDICE (Bereiter et al., 2012)
- 60 - 115 kyr BP: EDML (Bereiter et al., 2012)
- 105 - 155 kyr BP: Dome C Sublimation (Schneider et al., 2013)