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Climate of the Past
Discussions

Interactive comment on "A model-data assessment of the role of Southern Ocean processes in the last glacial termination" *by* Roland Eichinger et al.

Anonymous Referee #1

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Eichinger et al. present results of simulations performed with DCESS model of intermediate complexity. The main goal of the study is to understand the changes in atmospheric D14C between 17.5 and 14.5 ka B.P. The impact of changes in permafrost, deep ocean ventilation, dust and a higher PO4 content are briefly looked into over the whole deglaciation. My main criticism is that in some ways the authors try to do too much without really looking at each of the processes. So, at the end little new information is coming out of the manuscript or some important information is missing to really understand the implications. Please find some specific comments below.

The model used here is a simple Earth System Model of intermediate complexity, which comprises one high latitude zone and one low latitude zone, without "proper" water

masses. This is thus a very idealized (simple) set up, which has significant implications when discussing the deglaciation, changes in permafrost, upwelling...

1. Southern Ocean upwelling:

The authors always refer to Southern Ocean upwelling, whereas there is no "proper" (NADW, AABW...) water masses. In addition, in the pre-industrial set up water downwells in the high latitude box and upwells in the low latitude box. There is therefore no Southern Ocean upwelling (as far as I can tell, because there is little information on the topic). In the LGM state, vertical diffusion is reduced in the high latitude box below 1km. Therefore in the LGM state sinking at high latitude is restricted to $\sim$2000m, correct?

What are the water transports at the PI and LGM?

The change in ventilation is simulated by a change in vertical diffusion, whereby diffusion linearly increases between 17.5 and 14.5 ka B.P. So is the upwelling during MI occurring in the low latitude box in the model? Is the whole deep ocean ventilated between 17 and 15 ka B.P.? What are the associated changes in water transport during MI?

Shouldn't the authors show the vertical profiles of oceanic D14C, d13C and DIC, ALK and O2 at $\sim$ 15 ka B.P? The alkalinity profiles are not shown whereas it plays a strong control on atm. pCO2. All this information is crucial to understand the physical and biogeochemical response of the model to the forcing. The implications associated with the experimental design and model geometry ishould be clearly discussed and should be compared thoroughly with previous studies (see also references issues).

2. Heinrich 1-B/A:

Throughout the manuscript the authors refer to the "Mystery Interval", while totaling ignoring what we could call the "climatic intervals" of the last deglaciation,(Heinrich 1, the Bolling-Allerod) and the possible changes in oceanic circulation which occurred during that time period. While these changes in oceanic circulation cannot be simulated with the model used here, the implications should be discussed. H1 is not a simple case of “enhanced Southern Ocean upwelling” as NADW was probably very weak.

3. Given these simplifications, the calculated C14 production rate is also associated with high uncertainties, even though it is quite informative to use several C14 production rate forcings. This is one of the most interesting part of the paper. As a summary, the experiments presented here serve as an estimate of the impact of high latitude diffusion change on atmospheric D14C. In general simple models give an upper estimate of possible changes, and if really the whole ocean below 2000m gets ventilated during MI, you would expect the value presented here to be an upper estimate. . .

4.Permafrost: The authors briefly study the impact of changes in permafrost across the deglaciation on pCO2, D14C and d13C, but some important information is missing. How much permafrost is stored at the LGM compared to PI? How does that compare with other studies? What is the time evolution of the permafrost changes? Changes in permafrost are associated with high uncertainties and given the very simple approach used here, a more accurate change in terrestrial carbon content associated with permafrost cannot be obtained here. The experiment can however give a bit of information on the associated change in pCO2 and D14C for a given terrestrial carbon release, but it is imperative to know the change in terrestrial carbon (in GtC) separated into vegetation and permafrost. There is no discussion on the reasons behind the possible changes in permafrost, their timing . . ... As such I don’t really see the added value of the permafrost section. Also please see Kohler et al. 2014 (Nat. Comm.).

5. References: The introduction is imprecise and lacks a lot of important references for the work described. A lot of work on glacial/interglacial changes in pCO2 is not mentioned (see work by F. Joos team for example). A lot of references on C14Âăwork is missing (Kohler et al. 2014, Huiskamp and Meissner 2012).

Some references are also used in the wrong context:

For example p 18, the reference to Burke and Robinson (2012) is completely misused

as they present D14C data from the Southern Ocean and not d13C data. As such the whole paragraph p18, L3-14 is wrong and please note that there is no d13CO2 dip during the B/A.

P3, L9-10 in the intro is wrong. P6, L9 is wrong

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