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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "A reconstruction of radiocarbon production and total solar irradiance from the Holocene ^{14}C and CO_2 records: implications of data and model uncertainties" *by* R. Roth and F. Joos

L. Skinner (Editor)

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Dear authors,

Your manuscript has now been seen by two reviewers, and in addition a short comment has been posted online. The reviewers have both recommended that your paper be published subject only to minor revisions. I also found your paper to be an innovation on previous studies on this topic, as well as a very informative read. I am therefore recommending that you submit a revised version of your manuscript, along with a brief

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Interactive Discussion

Discussion Paper

description of how you have addressed the reviewers' main comments (very minor corrections to the text need not be commented on), prior to eventual publication in Climate of the Past. The reviewers have made some helpful recommendations on the text that I think deserve consideration. Indeed, although the length of your manuscript is not a major issue, it would be helpful if you thought it was possible to simplify and/or condense the text somewhat. This may be a challenge given that the reviewers have also asked for extra details on two aspects of your study; 1) its implications for deglacial atmospheric/surface ocean/deep ocean radiocarbon dynamics; and 2) the treatment of radiocarbon in the vegetation model. The former raises a question regarding how the focus of the manuscript is spelled out to the readers (i.e. radiocarbon dynamics since 21kyr versus radiocarbon dynamics during the Holocene). The latter is a matter of completeness.

I would like to add one comment of my own, which I think might be relevant if the manuscript does indeed venture to discuss deglacial radiocarbon dynamics (which I understand that it is NOT intended to; though I agree with reviewer 2 that this is not extremely clear). The issue that I perceive is that, although the BIO and CIRC experiments are seen as crude 'bounding' cases that might represent a range of processes for making CO2 increase across the last deglaciation, it is not clear (to me at least) how consistent these 'bounding' cases need to be with the atmospheric D14C forcing. The latter is a forcing in the model, such that the production/total inventory of radiocarbon is diagnosed so as to maintain consistency between the atmospheric D14C forcing and the physical changes that are implemented in CIRC and BIO. In other words, the accuracy of the inferred radiocarbon production changes across the last deglaciation hinge entirely on whether or not CIRC and/or BIO are in any way meaningful representations of how the marine carbon cycle changed across the last deglaciation. Presumably this issue is complex enough, and the uncertainties in the deglacial marine carbon cycle are large enough, that a discussion of simulated radiocarbon production prior to the Holocene must indeed be reserved for another paper. The main point in your paper is that whatever happened across the last deglaciation, it did not matter for Holocene

CPD

9, C628–C630, 2013

radiocarbon dynamics; am I correct? If not, please could you clarify this in your response?

One other query I would like to add, as something of a curiosity perhaps, is with regard to the so-called '8.2 kyr event', when the North Atlantic overturning circulation is proposed to have been significantly perturbed (e.g. Ellison et al., 2006). Is it possible that changes in the ocean circulation such as this, that were forced by processes that are not included in your Holocene forcings, might be misconstrued as changes in radiocarbon production (as for the deglaciation)? Or conversely might it be possible to provide an argument that such changes can be ruled out on the basis of the inferred radiocarbon production changes and their statistical properties for example? Presumably this is relevant to the proposed '1.5 kyr periodicity' in North Atlantic regional climate/overturning (e.g. Bond et al., 1997); would one be correct in inferring that this periodicity does not exist on the basis of your simulations and analyses (as it would appear to me)?

Finally, there are a number of very minor grammatical errors and typos in the manuscript (plurals, pronouns etc...), some of which have been identified by the reviewers, which should also be ironed out in your revised version.

I look forward to receiving your response and revised manuscript soon.

Yours sincerely,

Luke Skinner

Interactive comment on Clim. Past Discuss., 9, 1165, 2013.

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