

Interactive comment on “Marine productivity response to Heinrich events: a model-data comparison” by V. Mariotti et al.

A. Schmittner (Referee)

aschmitt@coas.oregonstate.edu

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The paper compares a compilation of productivity reconstructions with model results. It is interesting and appropriate for readers of CP. I recommend major revisions because some of the conclusions are not substantiated with evidence. A major point is the discussion of the reason for the global decrease in productivity in relation to reduced upwelling as found in Schmittner (2005). This should be discussed.

Below a list of specific comments:

Page 559 Line 4: It is not true that modeling studies generally show a global decrease in PP. Schmittner et al. 2008 GBC show an increase.

Page 560 Line 21-23: Specify where which model doesn't reproduce which observations
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tions

Page 560 line 29: what is the debate ? Schmittner et al. 2008 was not about AMOC collapse.

Page 561 line 24: remove “slightly”.

Page 562 line 7: replace “are” with “is”

P 563-564 Exp. Setup: H-events did not occur during the LGM but rather during MIS3 and the deglaciation. This discrepancy with the experimental setup should be discussed.

P 564 line 3: I looked up Kageyama et al. (2009) but I couldn't find the FWF experiment in that paper. They refer to different LGM runs LGMa, LGMb, and LGMc but none of those is the one described here. I'm confused. Please refer to the acronyms used in Kageyama to identify the experiment.

Table 1: Comparison of 500 years after HE with 3000 year average before HE may include stadial DO-events in the pre HE average. Consider shorter average (500 yr). What is the source for the dates of the HE, and what is the uncertainty? I'd recommend to include a column indicating the type of proxy used.

Schmittner 2005 did already a comparison between model and productivity reconstructions. I wonder if the authors have checked if they have included the reconstructions used in Schmittner 2005.

An issue discussed in Schmittner (2005) was that the model response evolves with time such that the full reduction in PP in the Indian and Pacific oceans was only expressed more than 1000 years after the AMOC shutdown, whereas PP in the North Atlantic recovered after the initial decrease. Although in Schmittner and Galbraith (2008) global NP increased recovered after 500 years (their Fig. S3). The latter study may use a better model but regardless of these differences both studies highlight the time dependence of the response. This may affect the model data comparison, in particular if

different averaging periods are used for the model and the observations as here.

The model and data averaging periods are inconsistent. Model data are averaged for only 50 years, whereas observations are averaged for 500 years and 3000 years.

Page 565 line 10-12: this sentence doesn't make sense. If you consider only cores where the model matches the data you should get 100% match, not 80%. I suggest to delete this sentence.

Page 565 line 26: "decreasing winter mixed layer depth" this was already shown in Schmittner (2005), who also get a similar % decrease in EP

Fig. 1: is the unit in g carbon ?

Page 566 line 18: I don't agree with the statement "Our simulations match these 4 records". All four observations are located in a region of no (e.g. S. Atlantic) or between two regions of positive and negative (NZL) change in the model.

Page 566 line 27: Fig. 3 doesn't show Si input. It shows Si concentrations.

Fig. 3. I suggest to show a zonal average over the box rather than a section at one longitude, which may be less representative.

P 566, l 28-29: show wind changes (or refer to figure in Kageyama paper if this is already published)

P 566, l 9: replace "data" with "reconstructions"

P 566, l 10-13. This discussion of the Mashiotta et al results doesn't make sense to me. The first sentence states that SSTs did not decrease (but does not specify the time periods referred to) and the second sentence states SST increased from the LGM to the Holocene (but it is not clear if this refers to the Mashiotta data or what else)

I wonder if it would make more sense to compare Si fluxes to the Anderson data.

I think it would be important to show some of the physical fields that are referred to

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(e.g. sea ice, winds).

P 567, l 24-25: I thought Table 2 shows model results, but this sentence suggests it is reconstruction results. Please specify in the table caption what is shown.

P 568: Comment on the EEP discussion. The model predicted changes in the EEP are very small (2%) . I wonder if such small changes would be recorded in the sediment and if they could be detected given the noise.

P 569: line 1: include comma before "which"

P 569: MAU interestingly Schmittner 2005 simulate an increased EXP there.

P 570: It was first shown by Schmittner 2005 that upper ocean nutrient concentrations decrease globally due to reduced upwelling of deep water. It is likely that this is also the reason for the global response here. This should be discussed and properly cited.

It is not appropriate to relate global EXP to northern hemisphere mixed layer depth. I suggest to remove this plot and the discussion. It has been shown by Schmittner et al. (2007 <http://magg.coas.oregonstate.edu/~andreas/pdf/S/schmittner07agu.pdf>) that mixed layers in the southern hemisphere increase as a response to an AMOC shut-down and a vertical redistribution of salt. So global MLD probably changes very little.

Page 572, l 19: remove " he claims that"

Page 572, l 24: include comma before "which"

Page 573, l 11-12: It is inappropriate for the authors to claim that the increased productivity in Schmittner 2005 cannot be explained by increased upwelling. It is important to realize that upwelling is not only due to wind forcing but also thermohaline forcing, the changes of which led to increased upwelling in Schmittner (2005).

P 575, l 2, include comma before "which"

It may be appropriate to mention that the reconstructions span time periods that were

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very different in solar irradiance due to the changes in the Earth's orbit. This may have affected the response.

Interactive comment on Clim. Past Discuss., 8, 557, 2012.