

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

V. Mariotti et al.

veronique.mariotti@lsce.ipsl.fr

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Response to Referee #2 (A. Schmittner)

We thank Referee #2 for the interesting comments and address the questions raised below.

Referee:

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.

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Response: Acknowledged and addressed (see part 3.3).

The text now reads: *“When globally integrated, EXP decreases by 16%, or 1.5 Pg (Tab. 2 and Fig. 7b) by the end of the simulations, in response to a HE. EXP strongly depends on nutrients concentrations in the illuminated waters, and the nutrient availability itself is highly dependent on nutrient supply through ocean ventilation and mixing. The constant freshwater flux that we use to approximate a HE induces a decrease of the thermohaline circulation (shown by a reduction in the strength of the AMOC by 87% (or 13 Sv), see Fig. 7a)) associated to a strong ocean stratification in the North Atlantic. This leads to a reduced upwelling of deep water (as already shown in Schmittner (2005)) and a decrease in the ventilation of the subsurface ocean, which induce a decrease of nutrient supply (cf. the decrease of almost 13% of global nitrate concentration at the surface in Fig. 7c). This explains the global decrease in EXP. We note that whereas the AMOC stabilises at around 2 Sv by the end of our simulation, EXP continues to decrease linearly over the entire 400 year period.”*

Referee:

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.

Response:

Acknowledged and addressed.

The text now reads: *“Coupled climate – marine biogeochemical models are used to simulate the evolution of marine PP over the historical period and under future scenarios. Four models (Steinacher et al., 2010) show a global decrease in PP and in the export production of organic carbon at the base of the euphotic layer (EXP) to deeper waters of between 2 and 20% by 2100, relative to preindustrial conditions, notwithstanding regional variability in the response. A reduced input of nutrients to the euphotic zone from subsurface waters due to increased stratification decreases PP in the North Atlantic and tropical regions, whereas lower light limitation increases Southern Ocean PP (Bopp et al., 2001; Steinacher et al., 2010). Another model*

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(Schmittner et al., 2008) shows an increase in PP and a decrease in EXP due to enhanced remineralisation in response to increasing temperature.”

Referee:

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

Response:

Both models (Uvic and LOVECLIM) do not represent the increase of EXP (export production of organic carbon) in the Eastern Equatorial Pacific shown in data from our compilation (Table 1). Instead they show a decrease. For the Southern Ocean, there is not a significant increase in both models as seen in our data compilation (Table 1). The text now reads: *“They all simulate a global decrease of marine productivity and a common response in certain regions (North Atlantic Ocean, Benguela coast, Mauritanian coast) matching the data, whereas in other regions they do not agree with the marine response seen in data (Eastern Equatorial Pacific, Southern Ocean, see Tab. 1).”*

Referee:

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

Response:

Aknowledged and addressed.

Referee:

Page 561 line 24: remove “slightly”.

Response:

Aknowledged and addressed.

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Referee:

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

Response:

Aknowledged and addressed.

Referee:

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.

Response:

We have pointed this limitation in the experimental design section and briefly recalled it in our conclusions.

The text now reads: *“We chose an LGM reference state because it was closer to MIS3 conditions than a pre-industrial run and because we obtained an abrupt collapse of the AMOC for a rather small amount of freshwater hosing, which is relevant for the study of mechanisms occurring during Heinrich events. This is partly due to the AMOC of the reference state being rather strong and hence closer to an interstadial than to the full LGM. Besides, Heinrich events 1 and 2 did happen actually relatively close to the LGM so these conditions are also relevant for this reason.”*

And:

“Despite the fact that we used full glacial boundary conditions rather than more realistic MIS3 conditions, the model results regarding the response of marine biology to Heinrich events are most of the time qualitatively consistent with paleo-data, which is encouraging for its ability to simulate future climate impacts on primary productivity and especially abrupt centennial-scale changes.”

Referee:

P 564 line 3: I looked up Kageyama et al. (2009) but I couldn't find the FWF experiment

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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.

Response:

Acknowledged and addressed. Our simulation GLA corresponds to the simulation LGMb in Kageyama et al. (2009) and our simulation FWF corresponds to the simulation LGMc.

Referee:

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.

Response:

Considering a shorter average like 500 yr is difficult in most of the records because their resolution is already 500 yr. So it would mean to base our pre HE average on one point. We preferred to consider several data points through time and make the mean and variance of these data points. We agree that in that case we can include stadial DO-events in the pre HE average, and even interstadial DO-events. In this last case, the variance induced by the DO-events is often more important than the change in HE compared to pre-HE and this leads us to conclude that the data does not show a significant change in response to HEs (see the large amount of "x" signs in Table 1). The source for the dates of the HEs is Sanchez Goñi and Harrison (2010) (see Table 1 and References). We added a column indicating the type of proxy used (see Table 1).

Referee:

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

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used in Schmittner 2005.

Response :

We included all reconstructions used in Schmittner 2005 except for two of them. We did not use Hendy and Kennett 2003, because we could not find any direct indication for paleoproductivity in this study and we did not take Lebreiro et al 1997 because the data had not enough temporal resolution (>500 years between two data points).

Referee:

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.

Response:

We agree this is an important issue. Because of CPU time constraint, we could only run 2 simulations of 500 years. On the other hand, data resolution is hardly lower than 400 years so it was difficult to have a 50 year data resolution. It would be interesting indeed to consider the time component of changes in longer simulations than what was possible in this study.

We have added a comment to the manuscript to make this clear and the text reads: *"The comparison between 50-year averaged simulations and 500-year resolution data is not ideal, but because of calculation time constraint, we could only run two simulations of 400 years each. Schmittner (2005) and Schmittner and Galbraith (2008)*

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highlight the time dependence of the response in EXP to HEs. For example, full reduction in PP in the Indian and Pacific oceans is only expressed more than 1,000 years after the AMOC shutdown in their simulations. It would be interesting to consider the time component in longer simulations than what was possible in this study."

Referee:

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.

Response:

We changed the sentence. That now reads: *"In conclusion, if we exclude the cores located on the model front areas and the ones that do not agree regionnally with other nearby records, we match the sign of the response of EXP in 72% of the locations (34 over 47)."*

Referee:

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

Response:

Aknowledged and addressed.

Referee:

Fig. 1: is the unit in g carbon ?

Response:

Yes it is. Acknowledged and addressed.

Referee:

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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.

Response:

Aknowledged and addressed: we reformulated the statement.

Referee:

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

Response:

Aknowledged and addressed.

Referee:

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

Response:

Aknowledged and addressed (Fig. 5).

Referee:

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

Response:

Aknowledged and addressed (Fig. 1).

Referee:

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

Response:

Aknowledged and addressed.

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Referee:

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 (e.g. sea ice, winds).

Response:

The referee is right, we clarify this in the manuscript. Mashiotta et al results show an increase in SST from LGM to the Holocene. From our model results we would have expected a decrease of SST during HE1 in this area, to explain our simulated increased sea-ice extent. We added to the manuscript a figure of silica export (Fig. 3) to compare with Anderson data and comment on this in the NZL discussion. We added a figure of global physical fields (Fig. 1, SST, mixed-layer depth, Ekman pumping and wind stress anomalies, sea-ice extent) and two subfigures of regional physical fields (MLD, Ekman pumping and wind stress anomalies, sea-ice extent) in Fig. 5 (NZL region analysis).

Referee:

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.

Response:

Aknowledged and addressed. Table 2 shows model results. We changed the sentence and detailed the table caption.

Referee:

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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.

Response:

We acknowledge the fact that the simulated EEP changes are small in the new version of the manuscript (end of 3.2.3 section).

Referee:

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

Response:

Aknowledged and addressed.

Referee:

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

Response:

The referee is right. This is discussed in details in part 4.1 of the manuscript.

Referee:

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.

Response:

Aknowledged and addressed.

Referee:

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

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et al. (2007 http://mgg.coas.oregonstate.edu/_andreas/pdf/S/schmittner07agu.pdf) that mixed layers in the southern hemisphere increase as a response to an AMOC shutdown and a vertical redistribution of salt. So global MLD probably changes very little.

Response:

Acknowledged and addressed. We removed the plot and modified the discussion.

Referee:

Page 572, l 19: remove “ he claims that”

Response:

Acknowledged and addressed.

Referee:

Page 572, l 24: include comma before “which”

Response:

Acknowledged and addressed.

Referee:

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).

Response:

Acknowledged and addressed.

Referee:

P 575, l 2, include comma before “which”

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Response:

Acknowledged and addressed.

Referee:

It may be appropriate to mention that the reconstructions span time periods that were very different in solar irradiance due to the changes in the Earth's orbit. This may have affected the response.

Response:

Acknowledged and addressed in part 2.3.

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

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