

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

Received and published: 31 July 2012

Response to Referee #3 (A.Voelker)

We thank Referee #3 for the interesting comments and address the questions raised below.

Referee:

The biggest issue I have with the paper is the problem of the response in the Mauritanian/NW African upwelling system. The authors are stating –correctly– in their paper that the problem of the contrary response between the model and data results most likely from the freshwater parameterization in the North Atlantic. What I am missing, in particular because of the discussion in paragraph 4.2, is a sentence that

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this problem that partially already occurs in the IPSL-CM4 model (p. 571) needs to be fixed in the future for more reliable predictions. This upwelling system is very important for the global carbon cycle and future food (e.g. fish) supply. Having a reduced upwelling in the NW African system will also have an impact on the intermediate depth circulation, i.e. the northward advance of the Antarctic Intermediate Water (AAIW). From paleo-data we know that AAIW was upwelled off NW Africa during glacials and that this water mass penetrated into the North Atlantic (e.g. Pahnke et al., 2008 Nat. Geosci); so I wonder if the authors ever compared biogeochemical parameters (nutrient concentrations) in the depths where AAIW is to be expected, between their Heinrich event run and a glacial or modern control run. Unfortunately, paleo-data for productivity changes in the Cape Blanc area on millennial-scales are rare because older studies focused on glacial/ interglacial changes. Nevertheless, the authors might want to look at some of the records published in Bertrand et al. (1996) in Mar. Geology; Martinez et al. (1999) in Mar. Geology or in Sarnthein et al. (1988) in Paleoceanography; Sarnthein and Winn (1990) in Schlesinger, M. (ed.) Climate-Ocean Interaction, Kluwer Ac. Publ.; or Sarnthein et al. (1992) in Summerhayes, Prell, Emeis (eds). Upwelling systems: evolution since the early Miocene. These records might not enter into the data/ model comparison but trends could be discussed and would strengthen the discussion. Indirect indicators could also be those records discussing dust (Jullien et al., 2007 Quat. Res.; is that Heinrich event dust signal comparable to the glacial one used in the model?) or ITCZ shifts (Zarriess et al., 2011 Geophys. Res. Lett.).

Response:

The referee is right. The problem with the Mauritanian upwelling system needs to be fixed for future projections. We added this in part 4.2. In our simulations, we focus on large scale regions mostly - the models having large biases in these coastal upwelling regions, so we did not investigate further this region. A way to go further to explore the response of such a regional system to large climate changes would be to use a regional model configuration forced by or embedded in our global model. Such

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projects are indeed under way and we hope to be able to present results shortly.

Referee:

p. 560: Though not essential for the paper, I would have liked to see reference to the ice core N₂O data and linkages to the nitrogen cycle in the introduction (e.g. Flueckiger et al., 2004 Global Biogeochem. Cycles; or Stocker & Schilt 2008 comment in Nature).

Response:

Acknowledged and addressed.

Referee:

p. 561: because of the bipolar seesaw and its impact on records from the southern hemisphere I am missing text on how Heinrich events and the Younger Dryas were defined in the various records. Did the authors just follow the indications given by the authors they are citing? Did they define specific age intervals (thereby keeping potential difference in age models in mind, e.g. GRIP vs. GISP2 vs. NGRIP GICC05 chronologies)?

Response:

We follow the indications given by the authors we are citing when they date the Heinrich events on their records. When they do not, we decided not to reject the records, but instead to make exploratory hypotheses on the change of productivity. As explained in the legend of Table 1, we used the dates of the Heinrich stadials onsets defined in Sanchez Goni and Harrison, QSR, 2010 (based on NGRIP GICC05 chronology), and we differentiated those records by adding a sign “?” in front of our results in Table 1.

Referee:

p. 564 and Table 1: please correct to ODP Site 882, etc. that is include “Site”

Response:

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Aknowledged and addressed.

Referee:

p. 565 first paragraph and p. 569 MAU discussion: increased productivity is also observed just a bit north of MD04-2805 CQ at site MD99-2339 (Voelker et al., 2009; GCubed) and in the Alboran Sea (Med. Sea; Moreno et al. 2004; Paleo3).

Response:

We added the sites MD99-2339 (as well as the other records in Voelker et al., 2009) and MD95-2043 (Alboran Sea) to our data compilation. Nonetheless, we see no significant increase in respect with our standards at site MD99-2339 and we even see a decrease at site MD95-2043. That is why you will not see increases at these sites in Table 1.

Referee:

p. 567 line 12: add “cover” at end of sentence

Response:

Aknowledged and addressed.

Referee:

p. 569 there are more records available for the Indian Ocean and I will list them below in relation to Table 1

Response:

The records have been taken into account in our data compilation.

Referee:

p. 570 lines 8 and 9: this finding is in contrast to Rashid et al. 2007 Science; so the authors might want to comment on it.

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Resonse:

- Rachid et al. (2007) use d18O of foraminifera to derive mixed layer depth. Their signal may be acquired during the growth of foraminifer and thus reflects preferentially spring / summer / autumn mixed layer.
- In our simulation, we clearly show the role of a maximum (winter) mixed layer depth, in the supply of nutrients for the growing season. Thus, our model results and Rashid et al. data may not be inconsistent, but targeting different quantities.

Referee:

p. 577 and following: delete the numbers (page numbers?) listed at the end of each reference after the publication year.

Response:

It is automatically generated for the CPD format, but it won't stay in the published format. The numbers correspond to the paper pages where the references are cited.

Referee:

p. 585/ Table 1: as suggested by Schmittner either list the respective proxy used for the productivity estimation or at least indicated if the reconstruction is for PP or EXP.

Response:

We added to Table 1 a column with the proxy used in the studies included in our data compilation.

Referee:

Additional paleo-data references.

Response:

We took into account the references given. The referee though will not find the study of Villanueva et al 1997 because the results of the studied core SU90-39 did not have enough resolution compared to our standards (we only selected studies where the time

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resolution is less than 500 years). But the core SU90-39 is studied in Nave et al. 2007 and is included in our compilation.

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