Clim. Past Discuss., 8, C375–C378, 2012
www.clim-past-discuss.net/8/C375/2012/

Climate of the Past Discussions

Interactive comment on "Contribution of changes in opal productivity and nutrient distribution in the coastal upwelling systems to late Pliocene/early Pleistocene climate cooling" *by* J. Etourneau et al.

Anonymous Referee #1

Received and published: 10 May 2012

The manuscript "Contribution of changes in opal productivity and nutrient distribution in the coastal upwelling systems to late Pliocene/early Pleistocene climate cooling" by Etourneau et al. reports a new diatom Silicon Isotope (d30Si) record from the Benguela Upwelling System (BUS) covering the interval between 1.5 and 3.5 Ma. The record can provide important new insights on the evolution of the degree of Silicate utilization in the surface waters of the BUS during a time period significant changes in global climate. Thus, the manuscript is within the scope of Climate of the Past. However, there are some important aspects of the interpretation of the record, and the potential implications for the evolution of atmospheric CO2 and global climate that are unclear and, in some cases, contradictory. These issues should be carefully addresses by the

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Interactive Discussion

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authors before publication.

Major Comments:

1 - The authors argue that the increase in the d30Si observed during the MDM (i.e. 2 – 2.8 Ma) can be explained by the development of more stratified conditions, and weak upwelling in the BUS during this time (page 681, lines 1-5), and state that this hypothesis is supported by warm SST (reconstructed by Etourneau et al 2009, Geology) and the presence of T. Antarctica, a species that requires periods of stable and stratified waters to grow. However, none of these two records (SST and abundance of T. Antarctica) is shown in the Figures, making difficult the comparison of the timing of the reconstructed changes in the different proxies. Indeed, Figure 2 in Etourneau et al 2009 seems to indicate a progressive cooling from 2.7 to 2 Ma that coincides with low d15N and high opal and alkenone MAR, suggesting an intensification of upwelling during this period, instead of a change towards more stratified conditions. The authors should include the SST, alkenones MAR, T. Antarctica and Chatoceros records in Figure 2 and discuss in more detail the timing of the proposed changes. In particular, how can high productivity (opal and alkenone fluxes), surface water cooling, and low d15N be reconciled with the more stratified conditions that are proposed to explain the high d30Si during the interval 2 - 2.7 Ma.

2- In section 4, the authors discuss the potential effect of the reconstructed changes on atmospheric pCO2 concentrations. They argue that after 2.7 Ma the stratification of the polar oceans was accompanied by the redistribution of nutrients to low latitudes, stimulating productivity in the upwelling areas as proposed by Cortese et al 2004. And then suggest that “the dominantly stratified conditions in the low latitude upwelling systems probably prevented significant upwelling of dissolved CO2-enriched waters to the surface, which diminished the CO2 flux from the ocean to the atmosphere in these regions”. Again, I do not see how the supply of nutrients and productivity can increase if the upwelling regions are stratified in the interval 2-2.7 Ma as the authors seem to propose.

3 - Indeed, later the authors recognize that upwelling is needed to explain the high opal fluxes but suggest that “Given the extremely high opal production in these regions, we infer that the uptake of CO2 was probably considerably enhanced and higher in flux than release from the deep ocean to the atmosphere via upwelling”. This argument can be invalidated by considering some available data from other regions of the ocean, for example, higher increases in opal fluxes (from 0.5 up to 6 g cm-2 ky-1) than the ones recostructed at ODP Site 1082 (0.5 – 3.5 g cm-2 ky-1) have been observed in the SO during the last deglaciation associated with a net release of CO2 from the deep ocean to the atmosphere that coincides with the deglacial atmospheric CO2 rise (Anderson et al 2009, Science). This example demonstrates that high opal fluxes do not represent any constrain on the balance between CO2 uptake and release. However the combination of d15N, d30Si and opal fluxes may provide some important insights on this process (Horn et al 2011, EPSL). In this respect I recommend that the authors follow a similar approach to the one proposed by Horn et al 2011 to estimate more quantitatively the potential changes in upwelling and nutrient demand in the BUS during the Plio-Pleistocene.

4- In page 684 lines 21-25, the authors state “For comparison, such reorganization during the last glacial periods would have accounted for a reduction of the atmospheric CO2 by 60 ppm (Brzezinski et al., 2002)”. The mechanism proposed by Brzezinski et al., 2002 is based on the idea that increased silica leakage from the SO may have caused diatoms to displace coccolithophores at low latitudes during late Pleistocene glacial stages, weakening the carbonate pump and increasing the depth of organic matter remineralization. This hypothesis provides a mechanism by which changes in low latitude productivity may have lowered atmospheric CO2 also during the Plio-Pleistocene transition. It is strange that the authors cite this publication here but do not discuss if the new data presented in the manuscript can provide any constrains on the evolution of this mechanism over the Plio-Pleistocene. Does the new data provide any evidence of changes in the relative contribution of coccolithophores/diatoms to export production in the BUS during the Plio-Pleistocene?

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Minor Comments:

- In the comparison of the pCO2 evolution Vs the changes in the BUS in Fig. 4 the authors only include one of the available pCO2 records. I think that a compilation of all the available pCO2 records and associated errors (as for example in Fig. 4 of Bartoli et al 2011, Paleoceanography) would represent more accurately the evolution of atmospheric CO2 concentrations during this period. Such a compilation seems to suggest that changes in glacial atmospheric pCO2 occurred in the interval from 4 to 3.2 Ma, around 2.7 Ma and during the MPT.

- In the introduction (page 671, lines 6) the authors state that the process driving the decrease in glacial atmospheric CO2 during the Plio-Pleistocene transition is unknown. I think it would more fair to say that it is not completely understood. As the authors discuss later in the paper there are several hypothesis related to changes in stratification, deep ocean ventilation and Fe deposition in the polar oceans that can in combination explain the observed CO2 change (e.g. Sigman et al 2004; Haug et al 1999; Martinez-Garcia et al 2011; Hodell et al 2006).

- In lines 22-23 of page 673 the authors mention that the abundances of radiolarian fragments were compared to the d30Si data and no evidence of correlation was found. I think that the radiolarian fragment counts in the different samples should be included as a Supplement (vs age and d30Si) to support the argument that changes in the dSi30 are not affected by variations in the relative contribution of radilarian fragments.

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