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Interactive Comment

Interactive comment on "Modelling an alkenone-like proxy record in the NW African upwelling" *by* X. Giraud

X. Giraud

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Answer to anonymous referee 1 (comments of the referee are in italic)

The full potential of forward modelling is the ability of the forward model to differentiate between the spatial gradients of the climate field and proxies (the relationship between all points at a single time) and the temporal gradient (the relationship at a single point over all times). This approach has been sucessfully applied to ice core records for instance (Werner et al, 2000, GRL). This goal should be more strongly brought out in the introduction and conclusions.

We emphasize the interest of the modelling approach at the end of the introduction, mostly by referring to the recent global calibration study of Conte et al. (2006).

I cannot comment on the details of the NPZD modelling nor on the regional ocean model set up, but I am satisfied by the sensitivity tests done with the ecosystem model. I am slightly more concerned about the potential sensitivity of the regional

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results to the climatological boundary conditions. Possibly something could be said about whether boundary conditions derived from specific ocean sections (rather than the highly smoothed Levitus climatology) would significantly alter the results?

A climatology is necessarily smooth and is best suited to provide boundary conditions in regional modelling. The use of a transient situation as permanent boundary condition would assimilate a temporary situation to a general feature. This is not to be desired.

In comparison to the data, it might be more useful to average the model output to the grid of the observations to have a cleaner comparison (i.e. it is not clear from fig 4 whether there are really systematic differences).

The simulated MLD has been averaged to the grid of the observations for a clearer comparison.

I am a little surprised that no direct comparison to of the seasonal SST is being made (except in a selected fashion in fig 10). Since the main conclusion of the model is based on the differences of the IPT to the annual mean SST, significant biases in the SST (and mixed layer depths) may play a role in the differences between the modelled IPT and core-top alkenone-derived SST.

One major point is that I am not sure that the Levitus SST data are appropriate to be used in Table 3 and fig 10. In particular, I suspect that they are significantly smoothed and do not capture the full temperature gradient near the coast. I would suggest that satellite derived temperatures be used instead since they may have better resolution data. Shipboard analyses may also be helpful.

The seasonal simulated SSTs are now compared with satellite data. The satellite AVHRR climatology (Fig. 3) presents the same smoothed patterns as the Levitus SST data.

Long term climate changes is mentioned, but even over the 20th Century this region may have warmed by about a degree (http://data.giss.nasa.gov/gistemp/maps/). It is *therefore possible that the core top alkenones may have been produced in slightly colder conditions (making the explanation of the results even more difficult). Although if the mean alknenone ages are in the thousand year range, it may be that mid-Holocene warmer samples are also mixed in (the discussion and Appendix A address this possibility though).*

The mixing with mid-Holocene warmer samples is the topic of the Appendix A.

There are some puzzling features in the final results. Firstly, the actual alkenoned-erived temperatures are biased high compared to the Levitus annual mean SSTs. This implies that their real IPT (assuming that the Prahl et al culture experiments are valid) is warmer than the annual mean (implying a bias towards summer conditions perhaps). (This situation is of course made worse by any recent climate change influence). However, the model produced IPT is biased low compared to the model SST (due to the significant subsurface temperature weighting). I don't have any problem in understanding why the model behaves as it does, but the real world data require explanation. This could lie in the seasonality of the temperatures and in a mis-characterisation of the coccolithophore bloom. Could the Seawifs chlorophyll data help in validating the timing here?

We think that the simulations reproduce the correct seasonal cycle for SSTs (Fig. 3) and coccolithophores (section 3.2).

In the conclusions it should be acknowledged that it is conceviable that the bias in IPT found here (due to subsurface production), would be calibrated away in a coretop calibration like Muller et al's since the correlation over large temperature ranges of the IPT and annual mean temperature should still be strong. It is therefore surprising that the Muller calibration is so similar to the Prahl values - something that could be explained by an IPT that was warmer than annual mean SST due to seasonality. Thus while this paper is interesting and should be published, there is still more work to do in reconciling the alkenone and modelling results. This paper is a solid first step.

It is true that the recent results about the alkenone index calibration by Conte et al. (2006) show a positive offset between the IPT and the sedimentary calibration. That is in opposite to our findings. Although Conte et al. invoke seasonality and depth production, they also suggest a diagenesis effect. This is unfortunately out of purpose for the actual work and model set-up. Further developments of the model are planed in order to answer these interesting questions.

Minor points:

on page 95 it is stated that it is the Muller et al calibration that is used in Table 3, but in the Table 3 caption it states that Prahl et al is used. I would suggest that Prahl is the most suitable, although the differences are very small

There are indeed very small differences between Prahl et al. (1988) and Müller et al (1998) calibrations. We chose the calibration of Müller et al (1998) because it is based on coretop sediments. Text and figure caption are corrected.

Other minor points are edited accordingly

Interactive comment on Biogeosciences Discussions, 3, 71, 2006.