

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 3 (comments of the referee are in italic)

In the description of the circulation model used two pieces of information are missing: - why there are two different choices for the wind (or wind stress?) field at the model lateral boundaries (COADS) and in the interior (ECMWF)?

The wind field from COADS data set has been used in parallel with the World Ocean Atlas data in order to produce monthly climatology for the open boundaries. Both dataset are climatology and agree to produce coherent long term forcing conditions at the boundaries. The wind field from ECMWF used for the surface forcing conditions are daily re-analysis by Röske (2001). This daily forcing is appropriate for the short term variability (pulse-relaxation phases) of the coastal upwelling, producing eddies and filaments. This is commented in the final version.

- does the model contain an explicit vertical mixing scheme? Which one? This might

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be important for sediment resuspension.

The vertical mixing coefficients for momentum and tracers at the ocean interior are calculated according the Large, McWilliams and Doney (1994) mixing scheme, and used in a non-local, K-Profil Parameterization scheme (KPP). This information has been included in the Model description, section 2.1.1.

The description of the ecosystem model focuses of the different parameterizations used for the two phytoplankton groups present. The description is self-contained and understandable. I have two remarks concerning the presentation of the model: - The growth rate is described as being dependent on nitrate, depth and time. Is that just a somewhat complicated way to express that it really depends on nitrate and irradiance?

The growth rate depends indeed on nitrate and irradiance, as shown by formulas in Table 1.

In describing how the two phytoplankton groups are parameterized and why so, the references are almost exclusively to other modellers works (Chai et al., Gregg et al., Moore et al., ...), but except for the classical paper by Eppley (1969) none of the original biological studies that the parameterizations are based on is cited. In short, the argumentation is that other modellers have used similar parameterizations. Coming to the substance of the model, the two phytoplankton groups differ only in two respects: The group representing diatoms has a higher maximal growth rate, but also a higher half-saturation constant for nitrate than the group representing coccolithophores. All other dependencies are chosen equal. This gives diatoms a relative advantage under nutrient-replete conditions in the upwelling, while it gives coccolithophorids an advantage in the oligotrophic open ocean. While this is a completely admissible choice, it should be acknowledged that a higher affinity for nutrients is just one of many factors discussed for causing coccolithophore blooms. The whole discussion is neglected here completely, and so are many modelling approaches with parameterizations different from the one chosen here. I wonder whether it is justified to ascribe the same

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grazing preference and the same light dependency to diatoms and coccolithophores. The latter is explored briefly later in one of the sensitivity studies, but no mentioning of that is made here.

The parameterization of the PFTs should be certainly based on observed or measured physiological features. These are unfortunately rare, and the measured parameters (in situ or from lab experiments) are not always appropriate to fill the modellers' expectations. It also appears that the model structure does not allow a complete match with natural behaviours. As mentioned by the referee, the competitors against coccolithophores are not limited to diatoms, and the grazing is the fact of many different classes of zooplankton. The parameterization of our model has therefore to reproduce the temporal and spatial coccolithophores distribution without having this full complexity of trophic relations. The possible factors to distinguish between coccolithophores and diatoms in the model are limited. The experience of former PFT models is therefore very valuable and constitutes a solid base for our purpose. As reminded in the revised version, the aim of the construction of the PFT model is to reproduce the spatial and temporal distribution of coccolithophores versus diatoms in the context of the coastal upwelling. This phytoplanktonic sequence shows a dominance of diatoms in the central parts of the upwelling and in summer, and a dominance of coccolithophores in winter and mid- and outershell. The choice of our parameters is validated by the seasonality and distribution of coccolithophores, as discussed in section 3.2. In order to better explain this point, the section 2.2.1 about the initial biological model has been split in two parts. The section 2.2.1 describes the initial biological model, and the new section 2.2.2 comments more in details the plant functional types. It includes a discussion about the factors controlling the coccolithophores growth and distribution vs the diatoms, and finally the parameters choice.

In the section on modelling of the alkenone-like proxy, the description is complete in the sense that one could rebuild the model from the description. However, the presentation is not very clear. The concept of the concentration-weighted temperature is not

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explained, and the two limiting cases (infinitely fast turnover of the temperature-proxy in phytoplankton vs. no turnover but just accumulation of the signal) are not explained well.

The explanation of the concentration-weighted temperature concept has been improved as follow: Additional state variables are also needed to carry the information about the temperature at which the coccolithophores grew. However, considering only the advection of a temperature record would miss the information of the concentration of the coccolithophore or phytodetritus carrying this temperature record. For modelling purpose only, we thus introduce the concept of concentration-weighted temperature. The advection of the concentration-weighted temperature is a linear process both in regard of coccolithophore or phytodetritus concentrations and their temperature. At any time, the temperature record is retrieved by dividing the concentration-weighted temperature by the concentration.

Moreover, the parameterization assumes that the temperature-proxy behaves linearly under mixing; is that the case for the UK37 proxy that the model is aiming to reproduce?

The UK'37 proxy is a ratio of unsaturated alkenones. As mentioned by the referee, the mixing of a ratio is not a linear process. Still, this question is tightly linked to the turnover rate of alkenones. It is obvious that the linear mixing of the UK'37 has no meaning as long as we consider an infinitely fast turnover. The turnover case study (simulation DELAY) considers variations of the temperature proxy in its lowest possible rate (maximal possible effect for a temperature bias). The temperature proxy varies due to growth rate accumulation and decreases due to grazing and mortality. The non-linearity of the mixing process would only add some small variations in one direction or another. These variations would be small, since lateral gradients are small. It has also been shown that this DELAY simulation does not change much compared to the standard simulation (STD) with immediate turnover. Therefore we consider that the mixing of the UK'37, even if not linear, would not greatly affect the results, and in no

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way change the main result, that is the influence of the production depth.

My general impression with the results section is that the validation of the model is taken somewhat too lightly; the recurring statement 'data and model are in good agreement' is often not specified very much. It would help sometimes if a measure for the model-data distance was given. Some examples for this: In section 3.1, it is not clear to me over which time intervals the depicted 'summer' and 'winter' mixed layer depths have been averaged both for the model and for the gridded observations. Are the two comparable, given that the model is forced with interannually varying winds, while that Kara dataset is a climatology? Also I would like to have some quantitative information on the differences between model and 'data' MLD, e.g. the rms difference. Does the mixed layer depth in the model depend strongly on model choices such as the vertical mixing scheme?

The seasons used for comparing the MLD of the model output and data are identical, and have been specified in the figure caption. The simulated MLD has been averaged to the grid of the observations for a clearer comparison. The root-mean-square of the difference for the MLD is mentioned in section 3.1.

In section 3.2 the modelled distributions of diatoms and coccolithophorids are presented. I was wondering whether the modelled dominance of coccolithophorids outside the coastal upwelling and their biomass are indeed realistic. The green ocean model (on which the model parameterization is based) is known (Anderson, 2006, J. Plankt. Res.) to produce coccolithophorid blooms throughout the subtropics (Le Quere et al., 2006), a feature that is in contrast to observations. Is the modelled coccolithophorid biomass outside the upwelling maybe so high because the model lacks other competitors (e.g. other flagellates) for diatoms? It would be good to have some numbers to compare to. A possibility would perhaps be to compare the modelled vertical calcite fluxes to sediment traps in the region.

As already mentioned above, the main concern about the simulation of the coccol-

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ithophore distribution is about the seasonal cycle and the spatial distribution. Both are already confirmed by the references in sections 3.2. (e.g. Tilstone et al., 2000; Müller and Fischer, 2001; Köbrich and Baumann, 2004; Schiebel et al., 2004). Based on time-series sediment trap records, the study of Müller and Fischer (2001) indicates that alkenone fluxes showed considerable interannual variations and no consistent seasonality.

The discussion of the different possible factors that might influence the modelled offset using a few sensitivity studies are the best part of the paper. The main result of the sensitivity runs that it is the production depth of the coccolithophores that most strongly influences the bias between SST and the temperature proxy. This is shown in a sensitivity run that makes use of a different parameterization of coccolithophorid growth based on Merico et al. 2004. That there are other possible parameterizations should be acknowledged in the model description earlier on. I suspect that the dependency on the coccolithophorid light sensitivity and on grazing probably is the most important factor affecting the modelled proxy bias, and would have warranted some more sensitivity studies.

Other possible factors influencing the coccolithophore distribution are discussed in section 2.2.2.

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