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

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General comment

A central question in reconstructions of past climate using sedimentary proxies is how reliably the proxies record or map the actual climate parameter that is reconstructed, and how independent this mapping is from the influence other factors. The aim of the manuscript by X. Giraud is to contribute to an understanding of the relation between alkenone-derived and actual sea surface temperature by modelling the production, vertical flux and accumulation in the sediment of a temperature proxy produced by coccolithophores. The location chosen for this exercise is off the Mauritanian coast in NE Africa, a location where several sediment cores provide a comparison to actual alkenone records.

The model used for this purpose is rather complex: It comprises a high-resolution physical circulation model for reproducing the distributions of salinity, temperature and ad-

vection velocity, a nitrogen-based ecosystem model with two phytoplankton functional groups (diatoms and coccolithophorids), zooplankton and detritus, a representation of sediment redistribution through resuspension at the bottom, and finally a separate module for representing the production, accumulation and settling of an alkenone-like temperature proxy.

Because of its complexity, the model is able to describe mechanistically a number of processes that could affect the mapping between the modelled proxy and the variable that it is supposed to represent, the annual average sea surface temperature, such as the vertical distribution of the plankton species producing the proxy, advection of signals from other locations, or sediment redistribution. But the complexity of the model also makes the validation of the model results extremely challenging.

Specific comments

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)? - does the model contain an explicit vertical mixing scheme? Which one? This might be important for sediment resuspesion.

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? - 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.

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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 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.

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 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. 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?

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

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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 diference. Does the mixed layer depth in the model depend strongly on model choices such as the vertical mixing scheme?

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.

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 aknowledged 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.

Summary and recommendation to the authors

In summary, this paper presents a completely new approach, namely to model the production and distribution of a alkenone-like temperature proxy, that is clearly innovative

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and should be published in Biogeosciences. To cover a large range of possible effects on the mapping between actual temperature and the proxy, a very complex model is used, comprising a regional high-resolution circulation model, an ecosystem model with two functional groups, a model for the proxy itself, and a sediment sinking and redistribution model. The complexity of the model means that there are a number of parameterization choices to be made that possibly affect the model outcome. The validation of the circulation and ecosystem model results is a weakness of the manuscript presented. Some more quantitative comparisons, and perhaps an estimate of the sensitivity of the model outcomes to model choices would make the paper much better. However, the two main conclusion of the model, that there is an offset between the actual sea-surface temperature and the proxy-recorded temperature and that this offset is mainly caused by the depth distribution of the phytoplankton species producing the proxy is mainly a qualitative statement and is probably not affected by possible quantitative offsets between model and reality. I just would not take the modelled offsets too quantitatively.

I therefore would recommend to publish the paper in Biogeosciences after revision. The revision should include a somewhat more extended discussion of possible factors influencing the distribution of coccolithophores (not only based on other modelling papers), and a somwhat more quantitative validation of the models outcomes. Some sensitivity studies concerning the growth formulation for coccolithophores would be helpful, but are not absolutely necessary. Before resubmission, the paper should be read by someone with english as his mother-tongue.

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