

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 referee 2 Duane Thresher (comments of the referee are in italic)

The correction of the terms “alkenone-like”, “alkenone-derived”, “coretop” and “coccolithophore” are edited as recommended.

The “K” in “UK’37” should be capitalized. See Prahl et al. (1988), which you cite for this index. I have edited accordingly.

I used the notation $U_{37}^{K'}$ for the revised manuscript.

Abstract, Introduction, Conclusion - One of the reasons I think this work is so important is that it could address (with climate models, proxy models and proxy data!) an important problem with alkenone SSTs: they seem to give overly warm tropical SSTs at the Last Glacial Maximum compared to climate models and other proxy data. This should definitely at least be mentioned, if not thoroughly discussed, in one or more of

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these sections (e.g., what are the implications of your conclusions to this problem?). It would give some high-impact context to your paper, which otherwise might seem to be, at first glance, just another obscure modelling experiment.

The interpretation of LGM alkenone core-top SSTs has been indeed one of the first motivations for this work. However, the conclusions of the present paper are, as a first step, more oriented toward the calibration of the alkenone index and the evaluation of potential biases. We mention thus the recent global calibration work of Conte et al. (2006) in the introduction.

Section 2.1.2 - Why wasn't the more-recent Levitus 1998 used instead of Levitus et al. 1994? Could some temperature discrepancies be due to that?

The recent satellite AVHRR climatology (Fig. 3) is used for comparison of the seasonal SSTs. They present the same smoothed patterns as Levitus data 1994. We think thus that the use of Levitus 1998 would not change the results of SST simulations.

Section 2.3 - Some mention and consideration of sediment bioturbation is in order (perhaps in Appendix A too).

Edited in appendix A.

Section 2.3.3 -

You state flatly that lack of biogeochemical feedback between the parent and child grids is not a problem but current loops are quite prominent in the study area. Comment?

The current loops mentioned by the referee are probably eddy structures. Their scale is much smaller than the domain extent. The eventual feed-back would only impact the borders. This is commented in the revised manuscript.

The description of the protocol of the simulations is a little sparse. I had to edit it based too much on assumption.

It would be better to consider the "sensitivity tests" simulations in their own right, i.e.,

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give them names, formally describe their setup, etc..

The protocol description has been re-written.

What class of computer were the simulations run on and how much real time did it take? I realize there is a tendency in the literature to not mention this anymore but it is important for readers to know this. For non-modellers in particular, which may be a large part of your audience, an idea of the practicality of the work is necessary, for future use of it and to gauge its uniqueness.

The simulations were performed on a 3.6 GHz PC under a Scientific Linux CERN 3 system. For each simulation, the 3-years spin-up phase takes 5 days computing and each additional simulated year takes another 5 days. This is edited at the end of the protocol.

Some discussion of equilibrium is absolutely necessary. Even realistic results can be due to transients ("right for the wrong reasons"). While the study area is relatively shallow, these are ocean simulations and they are short. Plus, you have parent and child grids and piecewise simulations. All might cause transients. Perhaps all this is discussed in the original model references but at least that should be mentioned.

Some discussion of equilibrium is added in the description of the protocol and at the beginning of section 3.1.

Section 3 - In each subsection/paragraph it should be made clear which simulation you are talking about. I have not edited to do this.

All paragraphs refer now to the simulation name.

I assume it but it might be worth mentioning (again, particularly for non-modellers) that the ocean results are the same for all simulations.

Mentioned at the beginning of section 3.1.

You try to show good agreement between model and observed using different resolu-

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tion plots (Figure 4) and then explain away differences as due to the different resolutions. It would be better just to regrid the higher-resolution (model) plots. This is not reflected in my line-by-line editing though.

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

Section 3.2, fourth paragraph (and Fig. 6) - what simulations are the coccolithophore and phytodetritus concentration depth profiles from? I did my editing leaving this information blank.

Edited

What does "with an increase scale of 200 m" mean? I left it out in my editing so you would note and rewrite it.

This is the scale-height. The equation of the sinking rate has been added to the revised manuscript.

Section 4.4, 4.5 - In the first paragraph of 4.4, after dismissing other factors as small, you state flatly that the cause of the temperature difference "has to be found in the production depth". My reaction was "or some factor you've missed or the sum of all the minor factors". Then in the third paragraph of 4.5 you actually discuss "missing processes in the modelling". It might be better to do this in 4.4 (also). My editing there only softened your statement. And what about the possibility of the sum of all the minor factors being the cause?

We did not completely ignore the impact of the cumulative effects, since we already stated in the conclusion that "the temperature discrepancy we found seems to be mostly due to the production depth of the coccolithophores, with minor contributions from seasonality and lateral advection." Nevertheless, as mentioned by the referee, we make clearer in the beginning of section 4.4 that the sum of these effects is not sufficient to explain the temperature difference and that missing processes are going

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to be discussed in the following section.

Section 4.4 -

Define PI. Edited

Does "According to this last sensitivity case" really mean simulation DELAY+GROWTH? Simulation GROWTH or both seems more likely. To cover the possibilities I've made it less-specific in my editing.

Appendix A -

The general layout of the argument is awkward (Assumptions 1/2 but then you mention others as well) but I've left that in my editing.

I don't understand (and thus could not edit) the sentence "This assumption could be discussed in regard to the variation of primary productivity or sedimentary flux over the past period, as well as modifications in the mixing rate in the sediments." Are you saying the exponential decrease could be due to one of these factors? But then you assume constant production and mixing rates.

Immediately following the preceding, I don't understand what "The present function" is. Equation (A1), (A2), the whole Appendix?

The Appendix A has been edited accordingly.

Figures and Tables -

Table 1 - What are the units of time? Indicate this in the sentence describing the variables. Also in that sentence, should be "radians", plural.

Edited

Fig. 1 - Are the SSTs simulated or climatological?

Simulated - Edited

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Fig. 6 - What simulations are the coccolithophore and phytodetritus concentrations from? Fill this in in my editing (xxxx).

Edited

Fig. 10 - It may be my PDF viewer but I see no linear regression lines. Nor do I understand what they would be of. I've left mention of them in my editing though.

This was a mistake in the figure caption. There are no linear regression lines. The core results are simply ordered by increasing alkenone index.

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