

Interactive comment on “A model-data comparison of the Holocene global sea surface temperature evolution” by G. Lohmann et al.

Anonymous Referee #1

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The authors compare SST changes simulated at various locations in the world oceans by climate models subject to insolation forcing for the past 6 kyr with SST changes estimated from chemical and elemental measurements on deep-sea sediment cores (alkenone index and foraminiferal Mg/Ca). Important differences are found between the simulated and estimated SST trends over this period (e.g., the simulated SST trends are generally smaller than the estimated ones). The authors show that the differences in trends can be partly reduced by invoking changes in the season (depth) at which (when) the alkenones are synthesized and the foraminiferal tests are secreted. A possible role of model deficiencies in the differences is also discussed. The authors conclude, inter alia, that the disagreement between simulated and estimated SST trends could raise doubt on the ability of climate models to properly represent climate sensitivity to insolation changes on long time scales.

C447

I think that this work significantly extends previous studies that similarly relied on sediment records for the Holocene for testing aspects of climate models applied for predictions. The most significant merit of the present manuscript (ms. hereafter) is perhaps the level of attention paid to the role played, in the comparison with model results, by some of the factors influencing temperature estimates based on alkenone indices or foraminiferal Mg/Ca. Particularly valuable in the present ms. are (1) the provision of quantitative estimates of the changes in growth season and depth habitat of coccolithophorids and foraminifera which would be required for the simulated SST trends to better agree with the estimated ones and (2) the discussion of whether such changes are plausible given current understanding of the eco-physiology of these organisms in the modern ocean. Furthermore, the finding that the models being considered poorly simulate the SST trends estimated from the sediment records is my opinion equally important. Indeed, it points to fundamental deficiencies in our reading of the sediment records and/or in aspects of the climate models. Overall, I think that the detailed analysis that is described in the ms. would be a very valuable contribution to the paleoceanographic literature.

Whilst I am generally enthusiastic about this work, I also think that some comments (listed below) would need to be addressed before this ms. be considered acceptable for publication. Major comments are listed below, followed by minor ones.

MAJOR COMMENTS

I think that more consideration should be given in the ms. to (1) the significance of the SST trends which are estimated from the sediment records and (2) some of the potential deficiencies of climate models from the viewpoint of their ability to simulate upper ocean temperatures. Specifically, the following comments appear to be in order.

1) A first comment bears on the adequacy of the statistical model that is used in the ms. to describe the variability in the sediment SST records. Linear regression is used to determine all the SST trends that are analyzed in the ms. (Figs. 1-2). Linear regres-

C448

sion, however, does not necessarily provide an adequate description of the variability in Holocene SST records. In a previous analysis of Holocene SST records from the North East Atlantic and the Mediterranean Sea, Marchal et al. (QSR, 2002) found that a significant fraction of the records are not adequately described by a linear regression, i.e., by a constant trend. The question emerges, therefore, as to whether linear regression provides a good description of the variability in the records that are considered in the present ms. A related question is whether the time-dependent SST responses to orbital forcing from 6 to 0 kyr BP which are simulated by the ECHO-G model are also linear. If departures from a linear trend are apparent in the data and/or in the ECHO-G simulation, then the linear trends in the data and in the simulation may not provide an appropriate measure for use in the comparison. In short, I think that the ms. would be strengthened if it includes arguments (based on residual analysis) that linear regression provides an adequate description of the variability in the data and in the ECHO-G simulation. If residual analysis fails to demonstrate that a linear regression is adequate, another measure of the SST changes from 0 to 6 kyr BP in the data and in the simulation should be considered as a basis for comparison.

2) The second comment relates to the uncertainties in the SST trends that are estimated from the sediment records. These uncertainties (standard errors of the linear regression slopes) have been estimated and illustrated by the red dashed lines in Figs. 1 and 2 of the ms. However, they do not appear to have been considered in the comparison with the simulated trends. Whether this approach is justified is unclear, since the neglect of errors in the SST trends estimated from sediment records may potentially lead to over-interpretation of model-data misfits. I would like to encourage the authors to give more consideration in their ms. to the uncertainties in the SST trends. One approach might be to retain, in the comparison with simulated trends, only those trends from sediment records which are significant at some specified confidence level (e.g., by retaining those SST trends that differ from zero by more than one or two standard errors). Another approach is to consider, as a measure of (dis)agreement between the data and the model(s), the difference in SST trends divided by the standard error in the

C449

SST trends from the sediment records (this second approach would have the benefit of keeping all the records in the analysis). Of course, errors should also be considered if a measure other than the one provided by linear regression is used in the analysis (see comment 1). In the absence of consideration of the errors in the observational trends, the robustness of the misfits between the observational and simulated trends, which is the focus of the ms., could not really be assessed.

3) The third comment concerns the role of model deficiencies in the misfits between the estimated and simulated SST trends from 6 to 0 kyr BP. A series of model deficiencies are discussed, rightfully so, in the ms. However, I think that a further potential deficiency would need to be discussed or at least mentioned, which is the likely inability of the ocean component of climate models to properly simulate the seasonal dynamics of the upper layers of the ocean (the mixed layer and the seasonal pycnocline). The seasonal dynamics of these layers, which involves a primary role by turbulent phenomena, is complex. It seems unlikely that the ocean component of climate models, with their coarse vertical resolution and crude representation of subgrid-scale processes, can produce accurate simulations of the seasonal changes in SST that are observed at open-ocean locations and some coastal locations. Inaccurate simulation of the seasonal cycle would be particularly critical for the present study if the temperature signal recorded by organisms used for paleoceanographic reconstruction is biased towards a specific season of the year. Accordingly, the question emerges as to how well the ocean component of the ECHO-G model simulates the seasonal changes (and annual mean) SSTs in the modern ocean. It may be useful to provide an answer to this question and elaborate on that answer in the ms., as it may provide insight into what we might (and might not) expect from climate models, from ECHO-G in particular, in their ability to simulate surface ocean temperatures in the past.

MINOR COMMENTS

1) Abstract: the last sentence is a little unclear and could perhaps be rephrased.

C450

- 2) Page 1010, line 23, “fulfill minimal statistical requirements”: Please clarify. Which requirements are being referred to?
- 3) Page 1011, line 13: write “in latitudinal direction”
- 4) Page 1012, lines 3-5: write “Three models including a vegetation representation (. . .) have also been used”. Lines 14 and 18: write “temperature trend” (reserve the noun “gradient” to denote a variation in space). Line 23, “first two levels”: Please provide the depth values.
- 5) Page 1013, line 18: write “is negative and not significant”.
- 6) Page 1014, line 11: write “is obtained for local summer”. Lines 24-26: please rephrase, e.g., “For Mg/Ca, there is a positive but insignificant correlation for the winter mean . . ., a negative and significant correlation for the summer mean . . ., and a negative but insignificant correlation for the annual mean . . .”
- 7) Page 1015, line 5, “the upper layers”: please specify the depths. Lines 7-8: drop “in the literature”. Line 20: add “. . . according to a modern nutrient climatology (Conkright and Boyer 2002).” Line 21, “they capture”: what does “they” refer to? Line 27: please specify “. . . seasonal and vertical gradients of . . .”.
- 8) Page 1016: I think it would be more natural to introduce here the two concepts described on p. 1012 (model-data SST difference divided by model SST trend and model-data SST difference divided by model temperature gradient). Line 5, “could be relaxed”: do you mean “would vanish”?
- 9) Page 1017, line 14: write “Instead the median of .. is evaluated as a overall measure of . . .”. More generally, I think that the second half of that paragraph (line 14 and below) is a little unclear and could be rephrased.
- 10) Page 1018: lines 7-8: write “The deviation between . . . could be at least partly attributed to . . .”. Lines 17-20: please rephrase (see comment 6 above). Line 23: Please clarify what is meant by “seasonal skewing”.

C451

- 11) Page 1019, line 1: write “production of coccolithophorids occurs” (I think all coccolithophorids can properly be labelled as phytoplanktonic). Section 4.2, 1st sentence: please substantiate with reference(s). Line 16: write “Alkenone SST in the southern”. Two sentences on lines 25-27: I do not see why weak seasonality at low latitudes would help explain the best agreement with the mean temperatures (compared to winter temperatures). Please clarify the logic.
- 12) Page 1020: last sentence before section 4.3: the authors seem to criticize the approach of Schneider et al. (2010). If kept in a future version of the ms., the criticism should be elaborated with arguments.
- 13) Page 1021, line 27, “a respectively corrected proxy-based SST trend”: Please rephrase.
- 14) Page 1022, lines 5-6, “the organisms record an underestimated temperature signal”. Please clarify (e.g., underestimated to what other temperature?).
- 15) Page 1024, line 23: write “a higher sensitivity to . . .”
- 16) Page 1025: line 8: write “the signal is clear”. Lines 12-13, “Spatial heterogeneous patterns provide an additional uncertainty for our data-model comparison”: please clarify. Last 2 lines: write “we assume them as having”.
- 17) Page 1026, line 20, “may be materialized”: do you mean “explain”?
- 18) Page 1028, line 4, “Vertically, this integration”: which integration is being referred to? Please clarify or rephrase. Line 9: write “of its symbiont-bearing”. Lines 26-27: write “One particular example of . . . trends in . . . The opposite long-term SST trends . . .”
- 19) Page 1029, line 12: write “solar activity” in lieu of “insolation” (G. Bond et al.’s paper was specifically about solar activity).
- 20) Page 1030, line 2: clarify why obliquity would be particularly relevant in this con-

C452

text. Line 3: do you mean “should identify?”. Lines 4-5, “. . . indicate potential positive feedback amplifying external forcing . . .”: please rephrase. Section 5, 1st line: I do not think that the model “largely agree” with the trends estimated from the data. Indeed, the highest [linear] correlation coefficient between estimated and simulated trends that is reported in the ms. amounts to 0.49, implying that about 75% of the trend variance in the sediment records remains unexplained even in this case. Please qualify.

21) Page 1031, line 1: “climate models have deficiencies” is in a way stating the obvious. Write perhaps “fundamental deficiencies”. Line 13: write “of model deficiencies and/or data particularities”. Line 14: write “two contrasting assumptions”.

22) Page 1032, line 7, “the model under and/or the data overestimation of . . .”: please correct. Line 28: write “to the lack of representativeness of”

23) Table 2: replace “Filters” by a more appropriate word.

24) Figs. 4 and 9: It is very useful to see these scatter plots. Please add to these plots (e.g., with dashed lines) the least square (linear) fits from which the correlation coefficients are calculated.

25) Figure 10: the caption should read “Global alkenone- and Mg/Ca-based SST trends . . .”

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