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Climate of the Past Discussions

Interactive comment on "A synthesis of marine sediment core δ^{13}C data over the last 150 000 years" *by* K. I. C. Oliver et al.

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Data synthesis is a really useful work. New features might appear from such data compilation and it is the prerequisite for good model-data comparisons. Many problems arise for such a compilation and a common time scale to all the data records is essential. Many other problems like species vital effects or interlaboratory calibration are also important points. At last, evaluating the errors is essential for the synthesis to be usefull. The paper by K. Oliver et al. is clearly presented and the authors give a detailed description of the different steps they achieved to build their regional stacks. They did present the errors associated with a large part of the initial data and the work done for the synthesis. Unfortunately the choices done at different steps of the data synthesis (common age scale, regional area choice, phytodetritus correction..) introduce large noise and as a result the data synthesis that is obtained is not valid. That is why this paper is not suitable for publication. Detailed comments are following.

The common age scale for the data synthesis is very important and represents a difficult step. I could not found the companion paper (Hoogakker et al, 2009, to submit to Clim. Past.) where the method is fully described. Thus I'll comment on what is presented in this paper. To produce a common time scale, the choice has been made of 5 "pivot" dates (18, 62, 87, 108 and 137Âăka) and a uniform sedimentation rate between the corresponding pivot points chose on the different records. Climatic transitions occur in between the pivot dates and there will be quite often large sedimentation rate changes at these climatic transitions: productivity as well as deep and surface currents change with climate transitions and produce large sedimentation rate changes. Thus the choice of only 5 pivot dates with constant sedimentation rates between these points will produce rather strange age scale for a lot of the records the authors listed. I suppose that's one reason why they rejected a lot of cores afterwards. I was not able to found which cores where kept for the final data synthesis and which cores were rejected, the tableÂă1and fig 2 presenting all the cores and not only those kept for the synthesis. The authors are aware that there is inter-basin differences in the timing of δ18O changes (Skinner and Shackleton, 2005), they might not be aware that it is also the case intra basin, within different water masses (Labeyrie et al., 2005), but anyway it seems they decided not to take into account uncertainties associated with the initial selection of pivot dates. For example for their first pivot date 18ka, it will correspond to the beginning of δ18O decrease in the upper deep waters (1000-2000Âăm depth) in the Atlantic ocean and will be close to the middle of isotopic stage 2 for Pacific deeper cores. 1) It is clearly important to include the error associated with the choice of the pivot dates 2) Over the last climatic cycle, the isotopic stratigraphy could still be a good choice for a global common time scale, even keeping in mind that there is water masses differences in the timing of δ18O changes, but it then requires to use the work done by Martinson et al., 85, and not only 5 pivot dates with constant sedimentation rate in between. Consequentely the error on the time scale is larger than the proposed 6Âăkyr and I do not understand the meaning of giving such 2 kyr synthesis regional records, even filtered to 6kyr.

In section 3, the authors presents their methods for smoothing the records and list the biases in reconstructing sea water δ13C changes from foraminifera isotopic analyses. They give a rather good overview of the different biases and indicate the correction and/or error associated. They rightly emphasize that the errors associated with the difference ΔLGMδ13C will be lower than for absolute values. However the choice they made for the phytodetritus correction might largely increase the error associated with the ΔLGMδ13C records. Mackensen et al., 1993 proposed that a phytodetritus effect was the reason of the Cibicidoides δ13C departure from water δ13C in the high productivity area of the southern ocean. However they did not observe this effect for top sediment cores in any other high productivity area (Africa costal upwellings, Pacific equatorial upwelling). It is then rather difficult to decide when and for which areas should there be a correction for such a phytodetritus effect (Mackensen et al. 2001). While for other biases (potential temperature and carbonate ion effect) Oliver et al. decided not to do any correction and keep the raw data, they decided to correct, in some cases, for the phytodetritus effect. Previous compilations that did so (Mackensen et al., 2001 and Bickert and Mackensen 2003) where using only Cibicidoides data. Here different benthic species data are used, thus using infaunal species for which productivity changes could produce larger biases. The choice has been made in this paper to correct only for the Cibicidoides taxon and in some high productivity areas. Moreover their correction is proportional to the LGM Late Holocene difference. It means that part of the phytodetritus correction they will apply could be linked, at some periods, to mean ocean δ13C changes. When both epifaunal and endo faunal records are available for a core they choose to correct the epifaunal record and not the infaunal one, not checking before, core by core, for a constant or not difference in the two records. The authors might thus introduce a large noise in the records. The option of giving the raw data with a summary of all biases (phytodetritus effect included) would have been much better. Another step is the choice of the regional averaging. First the authors chose to mean open ocean records and coastal upwelling ones in the principal region they determined. Second, they choose geographical regions with no attention to water

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masses. The carbon isotopic composition is a tracer of water masses and the different water masses will have different evolutions over the last climatic cycle. The area occupied by the different water masses will also change along the climatic cycle and the choice for regional means should be done carefully. In their synthesis, the authors do not distinguish tropical from subpolar zone. In the surface ocean changes along successive front might be very different.

For all the reasons presented above, the records presented in figure 5 to 8 show mainly a very large noise and seems of poor meaning. (Moreover they are not presented against a oxygen isotopic stratigraphy of one of the cores, that would be coherent with all the cores considered if the common time scale was don using Martinson et al., but with ice core records on an other time scale). For the last glacial maximum, many benthic δ13C reconstructions have been made, either on a latitudinal/depth section, either on geographical maps for different depths (Curry et al., 1988, Duplessy et al., 1988 Michel et al., 1995, Mackensen et al., 2001, Bickert and Mackensen, 2003, Curry and Oppo, 2005) and they show coherent clear features. The glacial reconstruction presented here on fig. 9 is very different probably because of the noise introduce by the age scale construction, the benthic species considered, the corrections added. . .. . . Moreover when looking at the benthic-planktonic difference, the author could have calculate the difference for each core first, avoiding at that step the problem of the time scale between different cores and looking at regional trends in a second step. It seems they use there mean regional records for benthic and planktonic foraminifera to calculate the difference at the last step. Once again they increase a lot the noise in the obtained record. Thus the data synthesis presented here is of poor meaning and is not suitable for publication.

Interactive comment on Clim. Past Discuss., 5, 2497, 2009.