Clim. Past Discuss., 5, C1239–C1247, 2010
www.clim-past-discuss.net/5/C1239/2010/

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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Received and published: 24 February 2010

We thank both reviewers for their thoughtful discussions which will substantially improve the manuscript. As requested by the editor, this is a first response to the Reviewers' comments prior to revision of the manuscript.

An update on the status of the Hoogakker et al. paper is required

We intend to submit this paper in time to appear in *Climate of the Past Discussions* prior to the submission of the revised manuscript. Should this not be possible, we will include the necessary materials, described below, in the Supplementary Materials to the revised manuscript, also citing the Hoogakker et al. paper. For the remainder of

this reply, we will refer to such material simply as "to be included in Hoogakker et al."; note that it may be included in the revised manuscript instead, but will be available at *Climate of the Past Discussions* in either case.

Reviewer 2 argues that noise is introduced to the data synthesis by a variety of processes

(1) Using δ^{18}O pivot dates and a uniform accumulation rate to constrain the age model

Reviewer 2 argues that δ^{18}O-based age model requires the use of "the work done by Martinson et al., 85" (we believe that the paper referred to is Martinson et al., 1987, Age dating and the orbital theory of the ice ages: Development of a high-resolution 0 to 300,000-year chronostratigraphy, Quaternary Research 27, 1-29). This paper addresses a significantly different question from the one we are tackling. It attempts to identify the uncertainty field associated with the choice of particular parameters and their relationship to orbital forcing. We are not here attempting an orbital tuning of any of these datasets or making any assumptions about the leads or lags of any particular proxy, whether δ^{18}O, δ^{13}C, or $CaCO_3$, to the orbital forcing. Instead we are using a template chronology for the last glacial that draws on some of the best available chronologies for ice cores, including the N/O dated Dome Fuji and the marine record (eg MD95 2042, the chronology for which is linked to that for the Greenland ice cores and the methane-correlated Vostok core) to create a first order template to which to compare datasets that include variations down to the millennial scale. We do realise that there will be diachronies in the the temporal occurrence of millennial-scale isotopic excursions between and within ocean basins; therefore our approach cannot be used to develop a best possible chronology for any of the particular cores involved. It does, however, provide a heuristic for placing all of the cores on a common time frame for which the errors can be estimated, including cores for which no more sophisticated method is suitable (necessary to improve data coverage in poorly sampled regions). Without such a framework, the various uncertainties associated with dating for the suite of cores included including uncertainties over the quality of radiocarbon dates, reservoir effects within and between basins, use of combinations of dating techniques etc. would be significantly greater and more difficult to quantify. Here the uncertainty field is restricted to the absolute chronology of events, the ability to recognise them in particular cores, and their diachroneity between and within basins. These sources of error do lead to wide uncertainties, as noted, but the use of heterogenous dating techniques would only add to the relative error between age estimates in cores.

Accumulation rates do indeed vary considerably. Because of the interdependence between implied accumulation rates and the chronology for the cores, it would in general not be possible to obtain better accumulation rate estimates than the age models imply, unless independent estimates of accumulation rate are available (eg from ^{230}Th normalization). In general therefore the accumulation rate variations are inextricably linked to the age models and the uncertainties about chronology between the tie points become greater the more distant in depth a particular point is from a chronological tie.

Any approach that highlights and attempts to identify the chronological uncertainties that exist between global marine records is likely to attract the criticism that large errors are introduced. The key point is that we are not attempting to provide the highest possible quality age model for each record, but rather a consistent age model suitable for the investigation of orbital timescales (20-100 kyr), so that higher resolution variability may be filtered out. The question becomes: given the dating errors that result from the necessarily basic method applied, is the dataset suitable for examining orbital timescale variability? An estimate of the error introduced by our method can be obtained by comparing our age models with previously published age models (using an independent method), for the subset of records where this is possible. The age modelling approach that we use agrees with published age models to within 6 kyr for 88% of all observations and to within 10 kyr for 95% of all observations, including low

resolution records which we have stated are likely to have errors greater that 6 kyr. The published age models used for comparison are based on a variety of approaches that have been used over the last 30 years, errors in the published age models are likely to be a factor in the few records with large disagreement. A detailed analysis will be included in Hoogakker et al. and summarised in the revised manuscript.

In summary, our method inevitably leads to age models that are poorer than the best that be achieved for some records, but it is necessary if we are to synthesise a wide range of data. The timescales that we investigate are much longer than the age error in most records, so that the existing age models are suitable for the purpose for which we have used them. We caution against use of the data synthesis on the shorter timescales associated with inter-basin or intra-basin error (Skinner and Shackleton, 2005; Labeyrie et al. 2005) raised by Reviewer 2. A data synthesis suitable for such an investigation could only be achieved by rejecting a substantial proportion of the records in our synthesis, especially in regions where there is already poor data coverage.

Nevertheless, we recognise that the age model discussion can be improved in several ways described by Reviewer 2. The revised manuscript will include: (1) error estimates for the pivot dates; (2) a comment noting the large error introduced by the use of uniform accumulation rates; (3) a quantitative description of age errors; (4) the δ^{13}C error estimate implied with the age error estimate, included in the final δ^{13}C uncertainty; (5) a justification of the 2 kyr temporal spacing of the final data set, together with a comment that this does not imply a precision of 2 kyr. (To answer Reviewer 2's question regarding the presentation of a 2 kyr synthesis based on a 6 ky filtered signal, signal theory states that a uniformly sampled time series should have a frequency greater than double (the Nyquist frequency) the highest frequency present in the sampled time series. On the basis of the age error estimates, we are considering filtering the data to 10 kyr rather than 6 kyr, with a spacing of 4 kyr, in the revised manuscript. This would not imply a precision of 4 kyr and we will state this in the revised manuscript.) Finally, we appreciate that the unavailability of the companion paper was a hindrance to the reviewers; this will not be an issue for the revised manuscript, as detailed above.

(2) Applying a phytodetritus correction

We agree that it is difficult to estimate the phytodetritus effect, especially for taxa other than *Cibicidoides* or for regions which have not been the target of previous studies, and that this adds error to such data. Including additional data in a synthesis reduces the total error in estimates drawn from the synthesis even if the additional data has a larger random error, provided this error is included and propagated, and the additional data is downweighted as we have set out. In the existing synthesis, a large random error of 0.4‰ was added to records with a reasonable probability of being affected by the phytodetritus effect.

However, including data with an uncorrected systematic error can increase the total error of the synthesis, motivating a desire to account for biases due to the phytodetritus effect. Due to the difficulty in estimating this systematic error, we intend to follow the recommendation of Reviewer 2 not to correct data for the phytodetritus effect in the main data output, but to accompany the data with an estimated bias due to the phytodetritus effect. Our intention is to instead compare records unlikely to be subject to a phytodetritus effect to the entire dataset in the revised manuscript (using the phytodetritus flags already provided), and base a recommendation to users whether or not to exclude flagged data based on our findings.

(3) Choice of regional averaging

This criticism does not relate to the data synthesis itself, which is not regionally averaged, but rather its presentation in Figures 6 and 8. Our view expressed in the manuscript is that the greatest error in regional averaging is due to significant sampling biases within each region, and that data coverage is not sufficient to estimate true regional averages without the aid of an Earth System Model. Therefore these figures are a representation the range of data observed in each region, not regional averages.

In principle, averaging over specific water masses could mitigate against sampling biases, but it would not be practical here. First, outside the Atlantic there are insufficient data to meaningfully subdivide the regions into water masses (Reviewer 2 cites reconstructions of the data-rich LGM Atlantic, whereas the present manuscript addresses the global ocean over 150 kyr). Second, the positions/volumes of water masses may evolve in time (as noted by Reviewer 2), and with few exceptions except for the LGM-Holocene Atlantic, this evolution is poorly constrained other than by δ^{13}C itself; to use δ^{13}C to reconstruct the movement of water mass boundaries and then to average δ^{13}C within the moving regions would be circular. Finally, a key goal of this manuscript was to assess the large-scale coherence of δ^{13}C in the global ocean over a glacial cycle, and we have found global coherence in the deep ocean, and to a lesser extent the intermediate ocean. Determining the detailed causes of intra-basin inhomogeneity is outwith the scope of this study, and better left to studies focusing on individual basins.

Nevertheless, the data can be sorted using criteria that are physically meaningful on the scales of interest here and likely to vary little over a glacial cycle. This has motivated the choices made, although there is scope for improvement. The equator is an appropriate boundary as it acts as a barrier to potential vorticity and the propagation of anomalies (e.g. Johnson and Marshall, J. Phys. Oceanogr., 2001). The Southern Ocean is divided from the remainder of the ocean by fronts of the Antarctic Circumpolar Current; while the positions of these likely vary in time, the position of Drake Passage and the Kerguelen Plateau, provide constraints; the revised manuscript will reflect this. Most ocean properties, including δ^{13}C, vary more strongly with depth than longitude. There is little theoretical reason to choose boundaries at 2500 m and 1500 m (or any other depth); these were chosen because data coherence is greatest with these boundaries, suggesting that they delineate water masses for much of the record. There are insufficient data to sort in more detail in any region other the Atlantic Ocean; here, we will improve the manuscript by using the mid-ocean ridge, rather than a meridion, as the boundary. Finally, in addressing (2) above, we will produce figures that can be used to distinguish regions of upwelling from other regions.

(4) Core-by-core versus regionally averaged planktonic-benthic differences

The analysis was carried out core-by-core before averaging, as Reviewer 2 advocates. We will clarify this in the revised manuscript.

Based on the noise introduced in (1)-(4) above, Reviewer 2 states that the data synthesis has poor meaning

Errors are indeed introduced to the synthesis data set in (1) and (2) – however (3) and (4) concern only presentation of results – but this does not lead to the synthesis having poor meaning. Previous studies have typically rejected data with large errors, which provides maps with less noise (as noted by Reviewer 2) that are useful for visualisation and qualitative understanding of the processes. However, when a primary goal of a synthesis is its use in quantitative studies such as model-data comparison, as is the case here, data with large errors are more appropriately downweighted rather than excluded (but see discussion on biases in (2) above). For example, model-data comparison would not be distorted by the presence an observation with an error of 0.8‰ amongst observations with an error of 0.3‰, because a closer fit to the more tightly constrained observation is demanded of the model. If on the other hand a pair of simulations produces a δ^{13}C value at a given location that differ by 2‰, observations with an error of 0.8‰ will certainly aid evaluation of these simulations, especially if there are few nearby observations.

A difficulty lies in satisfactorily visualising four-dimensional data (or slices through it), together with the errors in those data. The figures in the original manuscript are unsuitable for plotting error bars, which may contribute to Reviewer 2's perception that they are excessively noisy. However, the globally coherent signals described in the abstract are significant at the 95% level, and the revised manuscript will include the statistical analysis. The time-slices will be replaced by vertical profiles with error-bars, sorted by region as described in (3), from which it will be possible to see that much of the apparent noise is reconciled by the large error in some records.

The signal-to-noise ratio in planktonic data is poor, however, producing few statistically significant results. We will note this, and little emphasis has been placed on planktonic data as a result. Nevertheless, planktonic data are included in the synthesis for completeness, and as an aid to future researchers wishing to investigate these records.

The raw data from the synthesis needs to be made available

In our opinion, the simplest way to achieve this will be to include the raw data as Supplmentary Materials to both the revised manuscript and the Hoogakker et al. manuscript. This will consist of a single .csv file containing the age estimate, depth and isotope data associated with cores listed in Table 1.

It had been our intention to place the data set on Pangaea once accepted for publication, rather than beforehand as recommended by Reviewer 1. The synthesis includes records for which high quality dating has been carried out by previous authors, but not used in the synthesis due to the need for a consistent approach. We feel that it is essential for this reason (as well as to ensure proper attribution) that there should be no danger of the synthesis being mistaken for a primary database of raw records. Contributors of raw data have expressed concern that such misunderstanding is likely if synthesis are placed on Pangaea alongside individual records. Therefore, though we will attempt to satisfy Reviewer 1's recommendation and submit the data to Pangaea prior to submission of the revised manuscript, we will exercise caution. Nevertheless, we will meet the stated requirement of transparency and reproducibility by the using Supplementary Materials, where we will be able to ensure that the appropriate caveats are in place.

Reviewer 1 also noted that the Delphi Project site was not accessible. For users' information prior to submission of the revised manuscript with the raw data, this has moved and the new web address is http://www.esc.cam.ac.uk/research/research-groups/delphi.

Response to additional detailed comments of Reviewer 1

Age modelling: It is suggested that we account for inter-basin and planktonic-benthic variation in the timing of δ^{18}O maxima, by using nearby radiocarbon-dated cores as a reference. Resolving the timescales on which these phase differences occur is not a goal of this study, but reduction of error would of course be beneficial. A homogeneous timescale requires that we use δ^{18}O for all records, since radiocarbon ages are unavailable for many. Nevertheless, making pivot dates a function of region using radiocarbon dates or high resolution records, on sufficiently large scales that several well-dated cores are available, is a promising option. We will investigate whether this improves the agreement of our age model with published age models for cores where published models are available.

Low resolution cores: It is suggested that these be excluded, because of the danger of under-estimating the glacial cycle. This is a good point. The revised paper will include a comparison of the glacial cycle in low/high resolution records, and we will base our decision whether to exclude low resolution records on the results of this.

Light availability: We will include light availability in our error assessment for the revised manuscript, and increase the error estimate as appropriate.

Structure of the paper: It is difficult to respond to this before rewriting the manuscript, but we will move the comparison with literature to the Discussion section and see if this is an improvement.

Minor points 6-9: These will be corrected/clarified.

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