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Interactive comment on "Integrating palaeoclimate time series with rich metadata for uncertainty modelling: strategy and documentation of the PALMOD 130k marine palaeoclimate data synthesis" *by* Lukas Jonkers et al.

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The authors present a database consisting on paleoclimate records from several climate-sensitive parameters from 143 marine sites spanning the last 130 kyr. New chronologies have been built for each site for consistency while corresponding original chronologies are also provided. One advantage over other data compilations is the careful treatment of chronological data. The database contains rich metadata needed to perform a robust assessment of the paleoclimate signals and is available in three different formats facilitating its use in free software. The manuscript is clearly written and

has a straightforward explanation of the data search and treatment strategy, database structure, data storage and future plans.

The authors provide good rationale on the need of a database of time series data for modelling purposes and the importance of data formatting and standardization and their effort to build the presented database should be welcomed by the paleoclimate and climate modeler community. There are a couple comments I raise below I think the authors should take care of:

Lines 257-265: The use of variable reservoir ages to build age-depth models is a hot topic that is currently under debate. So far, no clear consensus exists on whether the general use of this or the "static reservoir" approach is more robust, advantageous, or beneficial than the other and the choice is made by every author based on different reasons. A discussion on why the authors decide to apply this approach should be included.

Also, I find their specific approach strongly relies on Butzin et al. (2017) model and is limited by the spatial coverage of the latter. How did the authors decide on which reservoir ages to use for sediment cores located out of the original data coverage of Butzin et al. (2017)? What is the uncertainty associated with the reservoir ages taken from the extrapolated regions in Butzin et al. (2017) and do the authors account for it? More specifically, do the authors account for additional uncertainty of reservoir ages for cores retrieved from upwelling regions and semi-isolated basins (if any is included in the database) where the effect of regional processes are not considered by the coarse-resolution of most general circulation models?

SST is a vital component of a vast majority of modelling efforts. I highly encourage the authors to re-calibrate SST estimates from alkenone ratios with the latest global calibration by Tierney and Tingley (2018). This does not differ much from previous calibrations for estimates below 24°, but over that value it corrects for the slope attenuation in the Uk'37-SST relationship providing temperature estimates several degrees differ-

ent from previous estimates. This calibration also provides error estimates and specific calibrations for regions where seasonality has a strong influence of alkenones-derived SST.

Minor comments: Line 28: Why there are more benthic δ18O time series than sites?

Line 96: What do you mean by homogenous? Please add continuous chronology.

Lines 122 and 123: the term “marine sediment sequences” might be more appropriate than “marine sediment archives”.

Line 132: Please state somewhere in this paragraph why the need for combining radiocarbon and benthic δ18O for the chronological control (e.g., radiocarbon limit).

Line 151-156: why so self-critical? Chronostratigraphies and age-depth models are the backbone of paleoclimate data. I find the choice of stating a robust and consistent chronological control as the conditioning criteria to build the stratigraphic framework an advantage rather than a pitfall, even if the database is not as comprehensive as it could be by using more flexible criteria for the time control.

Line 161: rephrase “by including more data”.

Line 181: rephrase “we followed the reduction approach”.

Line 218: Please mention tables in sequential order.

Line 233: in order to

Line 310: those cases when it is not possible to indicate the calibration are because this is not reported in the original paper?

Line 312: Species and number of individuals are reported as this is important information to assess foraminifera-derived proxy data. The same is true for foraminifera size, which indeed is reported in the database. Please add here the effect of the size of foraminiferal tests on derived measurements.

Line 321: The Uk’37 ratio is not based on alkenones with different chain lengths (both have 37 carbons) but with a different number of unsaturations (di- and tri-unsaturated). Please correct.

Line 323: Please replace: not constant by “seasonal”.

Line 326: C37 alkenone concentration is included in the database but its significance as productivity proxy is not included in the discussion. Is there any reason for this?

Line 364: Replace Table by table.

Line 383: Replace table by Table.

Line 428: Does this mean no more 14C-based chronologies? Why?

Line 430: Is there any plan for authors submitting their data for inclusion in the database?

Line 470: The observation time series?

Line 487: Based on planktonic foraminifera Mg/Ca. . . Please rephrase.

Line 513: Replace Pangaea by PANGAEA

Line 518: . . . data consists data not part of publication. Rephrase.

Table 4: it might be convenient to replace “detritus” by “debris” for searching purposes as the latter is more common in the paleoclimate field.

Tierney, J.E., Tingley, M.P., 2018. BAYSPLINE: A New Calibration for the Alkenone Paleothermometer. Paleoceanography and Paleoclimatology 33, 281-301.

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