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Climate of the Past Discussions

Interactive comment on "Quantifying the effect of seasonal and vertical habitat tracking on planktonic foraminifera proxies" *by* Lukas Jonkers and Michal Kučera

Anonymous Referee #2

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Review for Climate of the Past of: "Quantifying the effect of seasonal and vertical habitat tracking on planktonic foraminifera proxies". By L. Jonkers and M. Kuçera.

The oxygen isotope composition in planktonic foraminifera is considered to be primarily a function of the ambient temperature and the oxygen isotope composition of seawater (δ18Ow) during calcification. To a lesser extent the oxygen isotope composition of the tests (δ18Oforam) may change as a function of other environmental factors related to the (carbonate) chemistry of the seawater or biological controls. Previous studies have indicated that the production of planktonic foraminifera is not distributed uniform over the time span of a year, but that growth takes place during a season in which most suitable conditions prevail. The same holds for the calcification depth, which may vary

depending on the water column conditions such as temperature, (temperature or physical) structure (e.g. stratification) or may depend on the depth of food availability. As such planktonic foraminifera may have a changing season of growth and depth, both depending on the local / regional ocean-climate conditions. The study presented by Jonkers and Kuçera deals with the goal to unravel the seasonal/depth signal recorded in the oxygen isotope composition of planktonic foraminifera. The study aims quantification of seasonal & depth habitat tracking by some species of planktonic foraminifera. Oxygen isotope measurements from core top sediments, are used in this study to obtain insight in the habitat tracking of foraminifera.

The manuscript generally reads well, is sufficiently illustrated and referenced. Unfortunately, the data used / presented seriously lack estimates of variability and appropriate statistical testing. At this stage it is unclear if the same conclusion will be reached after more careful consideration of the error and variability of the data used. It is recommended to include an analysis of variability and error and investigate the consequences of this for the significance of the regressed slopes used on which the main conclusion is based. As such the manuscript in its present form is suitable for publication after revisions.

Comments and suggestions

In general readability / clarity can be improved by associating δ18O or $\Delta\delta$18O with the appropriate subscript i.e. indicating water(w), foram (f) or equilibrium(eq), foram minus water (f-w) or foram minus equilibrium ($\Delta\delta$18Oforam-eq) etc. At times this is unclear or even lacking, and therefore confusing and obstructing smooth reading.

In the first part of the study, the authors have investigated whether there is a trend between the δ18Oforam - δ18Oeq.am.0-50m - briefly referred to as $\Delta\delta$ 18O - and the mean annual temperature (MAT). If there is a trend, it can be concluded that species have recorded temperatures systematically deviating from the MAT. The analysis shown is key to the conclusion of the study, and it because of this importance and further implications for the study that more transparency is needed in showing these data, in combination with a more appropriate and sound statistical assessment.

Looking at the data represented in Figure 2, it is not clear what the original (δ18Oforam and δ18Oequilibrium) data are alike. The reason for this is that - in Figure 2 - only the difference between the foraminiferal δ18Of and the equilibrium value is graphically provided. I would say this showing the ("raw") data used for a study like this would be the first thing to do. Why I think this is important? It is so because it can be expected that the equilibrium value changes as a function of upper ocean temperature and δ18Ow and error associated with the data can be made visible (see below). As such, it is recommended to insert a new figure - between the present Figure 1 and 2 - showing the foraminiferal δ18O measurements and their associated equilibrium values including estimates of error / variability in the form of error bars (e.g. s.d. or c.i. intervals)!!

The data discussed and graphically represented in Figure 2 presently lack estimates of variability, that is, the data shown are not associated with an estimate of variability resulting from (measurement) error, environmental (i.e. seasonal) temperature variability (MAT is used, but the degree to which MAT is known varies as a function of seasonality), and variability resulting from the δ18Ow estimates (expected to be relatively high at high latitudes and used to calculate the δ18Oeq.am.0-50m) for which regressions vs. salinity - with error - have been used (LeGrande and Schmidt (2006). It is only later in the manuscript the authors refer to this as "some inherent noise" (line 206) indicating that the authors are aware their data should be associated with s.d. or alternatively with a confidence intervals to properly assess the information. The point here I want to make is that I disagree with the statement that this would be "...some inherent noise...". There is unfortunately, no effort or attempt made to provide any form of error / variability assessment, while in my view there's plenty of opportunity to do so (SST variability is known since atlas data were used, error in δ18Ow can be assessed via the regressions used etc. etc.). The assessment of the variability is key to the conclusion that

(line 143): "...five out six analyzed species appear to minimise experienced temperature / environmental change, consistent with our hypothesis that....". Just reporting the RMSE and intercept of the regression is not sufficient to support the hypothesis that several species show evidence of habitat tracking. The authors should make a serious effort to come up with a decent quantification of error and convincingly show that the conclusion drawn from the data is statistically sound and robust! Once "x y" variability is assessed, an appropriate statistical test can be used to find out whether the slopes shown in Figure 2 are indeed significantly different from 'zero.

In section 4 "seasonality", the log (flux) pattern is described as "...a sine wave of which the amplitude and phasing are changed as a function of the annual mean temperature...". Although this may - intuitively - be a reasonable approximation for the extratropics, I wonder if the approach followed also agrees with the flux patterns for species living in the tropical oceans where insolation is not a limiting factor and there is two maxima in the solar insolation during the course of a year. It seems that the authors do observe a problem with this model in predicting the seasonal flux pattern of species in the tropics (lines 154- 157), but it is not explained / clarified why this is so and what the implication would be for their conclusion! Likely the seasonality of species in the tropics is driven by other factors than temperature? Maybe this can be clarified better in the context of Lombard et al., (2009) (Mar.Mic, 70, 1-7) and Lombard et al., (2011) (Biogeosciences, 8, 853-873), where species growth rates are modelled as a function of temperature. If using an ocean model in combination with temperature dependent growth rates i.e. using an ecophysiological model, one can likely predict the oxygen isotope composition reasonably well. I wonder why such an approach, i.e. using an eco-physiological model, has not been chosen and preference is given to modelling the flux as "a function of a shifted the sine wave"? This should be clarified.

Similarly to the remarks above for Figure 2 and associated data, data shown in Figure 5 data should have variability indication. A test of slope should be performed to show the slope is not significantly different from "zero', further supporting the hypothesis that depth habitat migration may indeed occur.

line 63: Should read: "...a clear relationship with sea surface temperature .." line 64: Sentence unclear, consider rephrasing: "While the latter trend....will reflect". line 75: Vertical habitat? Recommended to change to "depth habitat". line 79: Geochemical data: mention Mg/Ca, δ18Oforam. Line 81: Start new paragraph. line 90: Seasonal sea surface temperature instead of seasonal temperature. line 97: dampening effect: i.e. reduction of the recorded range versus the environmental (observed) range. line 100: "foraminifera proxies". Better: "foraminiferal δ18O" line 133: Incomplete. Change "..high temperatures..." into "..high annual mean temperatures..". line 133: Change "...higher calcification temperatures..." into "...higher than annual mean calcification temperatures...". line 137: "...Nordic Seas outside of the direct...". Remove "of". line 136-139: "These observations...further analysis". This sentence is quite long. Consider making two. Second sentence may start after North Atlantic Drift. line 140-141: "..is the only species that..." can be removed. line 143: "...analysed species.." may be changed into "...species analysed...". line 163: "all of" can be removed

line 202: "Our analysis allows partitioning of habitat change in to changes in seasonality and calcification depth for .....". If statistically robust, and the same conclusion holds after analysis of variability, the analysis still does not allow (a full) partitioning in my opinion. I recommend to phrase more careful. line 204: use δ18Oforam instead of just δ18O. Note that the delta notation has been used in two forms. Indicate which one is applicable.

line 244-252: The effect of an nutrient depleted mixed layer quite typical for the tropical ocean structure is not considered as an option for deeper & colder growth. Simply the fact that species can find their food deeper in the water column (Deep Chlorophyl Maximum), just because the mixed layer is nutrient poor and as such contains less particulate matter, is not considered here. It would offer a very plausible explanation for deeper growth - at lower than SST - in the tropics.

line 261: "...foraminifera grow their test exponentially..." needs rephrasing. i.e. " ...shell mass increases exponentially as a function of shell size... "

line 303: The remark that the species $\Delta\delta$18O "at face value" holds the best promise of providing reconstructions of mean annual near surface conditions is may be a bit mystifying. As is explained in the next section, G. bulloides is characteristic for high nutrient waters and in (tropical) upwelling systems the species is associated with upwelling and hence calcifying at or close to the lowest SST's during the year. Since the SST's during upwelling are deviating from AM conditions, it may be better to say that right away that G. bulloides does not reflect AM conditions.

line 314: "assumption of constant seasonality and depth habitat" references? or leave out...

line 322: Rephrasing needed: "..not driven by mean annual temperature..". How can a mean temperature drive anything?? The mean is a statistic!

line 379: homeostatic behaviour? I'm quite sure that the term 'homeostasis' is applicable to humans/warm blooded animals. I guess there is not such a regulatory system present in uni-cellular zoo-planktic algae! I guess the ability of foraminifera to potentially "actively" seek optimal conditions may more have to do with their genetic and epigenetic (not investigated so far) profiles.

Please provide more informative Figure captions these are very brief!

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