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Climate of the Past Discussions

Interactive comment on "On the validity of foraminifera-based ENSO reconstructions" *by* Brett Metcalfe et al.

Anonymous Referee #3

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In this study, Metcalfe et al. aim to test whether the approach of using individual foraminifera analysis (IFA) can be used to assess ENSO variability. In order to accomplish this, they use the Foraminifera as Modeled Entities (FAME) model to calculate idealized foraminifera distributions across the tropical Pacific. These results are then combined with seafloor/ CCD depth and sedimentation rate to determine which regions of the Pacific Ocean are suitable targets for IFA approaches. Modeling of foraminifera populations in order to determine if ENSO change is detectable has been done before (e.g., Thirumalai 2013, White 2018), although these studies focus on the detection of ENSO from paleoclimate proxy records. This study's novel contribution is the inclusion of the FAME model and foraminiferal growth rates to the analysis of modeled response of biological calcite to tropical variability. However, the FAME portion of the model is

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not validated against core-top data from the tropical Pacific, precluding assessment of its utility. The application of these results is likewise problematic, as it focuses on determining whether ENSO events (El Niño, La Niña) and neutral conditions have distinct distributions (forward modeling) rather than on how one could detect ENSO change (inverse modeling). Further, the discussion on sedimentation rate and CCD is broad-based and does not take in to consideration local changes in seafloor topography, changes in bottom-water oxygen availability that may alter bioturbation depths, and the variability characteristics of different regions with regard to the seasonal cycle, decadal-centennial variability, and ENSO change (e.g., Thirumalai 2013, Ford 2015, White 2018). Finally, there are aspects of the model that are unrealistic (e.g., a 400m depth for symbiont-bearing foraminifera; assuming sample sizes of 1000 for binning) or unrealized (e.g., how many individuals were selected for generating these estimates and a lack of model-data comparison) that present significant issues to the overall utility of this model for paleoceanographic reconstruction of ENSO from IFA. The title of the article does not represent the content or main goals of the study, and the conclusions stated in the abstract are different than those in the main paper. The questions the authors raise are valid and useful, but the results as stated do not support their conclusions. In fact, the stated conclusions of the article are, in several places, contradicted within the paper itself. These contradictions are not well-explained, and thus a clear summary of the findings is difficult to parse.

General Comments

The study here focuses on forward modeling using FAME for IFA. However, the authors fail to prove whether existing IFA-ENSO reconstructions are valid or provide the tools for evaluating proxy data (e.g., the "inverse problem", as mentioned in other reviews, whereby foraminifera records are analyzed to infer ENSO). Thus the application of these results to the paleodata world is limited. The more relevant application here is in targeting locations for performing IFA studies, but this is limited as well, as the sedimentological and bioturbation properties of regions across the Pacific are much more variable captured here. The authors use their own definition of ENSO events, despite significant previous literature and established definitions that are commonly used. The use of single month anomalies does not adequately represent the actual ENSO phenomenon, which relies on ocean-atmosphere feedbacks expressed over a period of months, and thus their analysis of differences between El Niño, La Niña, and neutral conditions may be flawed and biased toward non-ENSO SST anomalies.

This study does not compare the results of their FAME analysis with existing IFA reconstructions of variability from the tropical Pacific. In the eastern Pacific, Rustic 2015 used δ18O IFA on modern-era sediments to show close correspondence with calculated δ18O from reanalysis data; in the central Pacific, White 2018 showed that the distributions of Mg/Ca-based SSTs from individual foraminifera in a 4ky coretop are statistically similar to modern reanalysis data.

Specific Comments

The authors focus on δ18O proxies for IFA, and discount Mg/Ca reconstruction and the modeling efforts done with those (White 2018, Ford 2015). To discount Mg/Ca ratios as a paleoproxy without the kind of analysis provided for δ18O seems premature . While changes in carbonate concentration, salinity, and preservation environment can indeed alter Mg/Ca ratios, significant study has been done and is underway to understand these roles. Species-specific calibrations and various corrections exist that are well quantified. Not using Mg/Ca for the Tc seems rather limited.

The number of foraminifera picked from a given sediment interval is an important component of IFA. Increasing bin counts to 1000 artificially (Page 6) does not represent the numbers typically used in such analyses; the numbers used for other analyses (Page 7) are not specified.

In the results, the first statistical test is to test whether the means of the FPen and FP-neu δ18O distributions are different and use this to determine whether ENSO events can be detected. Comparison of the population means does not necessarily reflect differences in the population distributions, and only provides a measure of mean conditions that may or may not be related to ENSO variability. The use of the Anderson-Darling test to assess differences in distribution is used later. It is unclear how these two different tests were related, and how the mean δ18O FPen/neu was utilized. The author's use of the Anderson-Darling test to assess differences in distributions is novel, but results of this test are not compared to those that have been used to assess IFA results in previous studies (e.g., std dev (Thirumalai 2013, Koutavas and Joanides 2012, Rustic 2015) or Q-Q (White 2018, Ford 2015)). Is this more sensitive, less sensitive, or does it measure different aspects of the distribution change NOT captured in the other analyses? Without such comparison, the ability to assess the validity of IFA reconstruction (the purported goal of this paper) is limited.

The specifics of sedimentation rate and bioturbation vary greatly across the tropical Pacific and rely on multiple processes. The role that oxygen plays in bioturbation is important, especially as bottom-water oxygen levels vary across the tropical Pacific. Likewise, seafloor topography is highly variable, with ridges and sea mounts that are not apparent at the resolution used.

On P8: "Similarly, the individual characters of El Niño events, which are very short in duration, become lost in the bioturbated sediment record " The purpose of IFA is not to discern the properties of an individual event. Change in frequency or amplitude of events over a period of time can be statistically detected using various means to compare the distribution of integrated conditions over the period of sedimentation. Bioturbation serves, then, to extend that integrated time and the range of conditions experienced.

Bioturbation will also not remove anomalous values (page 9) – rather, such values may be present as part of a distribution representing more integrated time. Likewise, bioturbation has the effect of smoothing the signal, but the "signal" is a function of all sources of variability (ENSO, annual, decadal, centennial). The relative expression of these forms of variability along with the amount of time integrated by a sample are both important in terms of the ability to capture ENSO signals.

On P.10, Cole and Tudhope (corals) and White et al (IFA) are cited in error when discussing lake colour intensity and precipitation-driven records.

Also on P10, the authors claim: "If the number and magnitude of ENSO events were reduced, the relatively low downcore resolution of marine records may not accurately capture the dynamics of such lower amplitude ENSO events using existing methods." – Which methods? Q-Q, std. dev, event counting, others? It's not entirely clear this is even referring to IFA reconstructions, as the records discussed previous are sedimentary, coral, and IFA (but noted as "precipitation driven", see above).

P.10: The possibility of a marine sediment archive being able to reconstruct ENSO dynamics comes down to several fundamentals: the time-period captured by the sediment intervals (a combination of SAR and bioturbation), the frequency and intensity of ENSO events, as well as the foraminiferal abundance during ENSO and non-ENSO conditions. This statement leaves out other key elements, including the relative expression of ENSO events, the seasonal cycle, and decade-and-longer variability. These elements are (arguably) more important for inverse modeling, where the ability to disentangle growth rates from other sources of variability is impossible, and thus the signatures of ENSO in such records need to be discerned.

A key point in the paper (P10) says "The results presented here imply that much of the Pacific Ocean is not suitable for reconstructing ENSO studies using paleoceanography, yet several studies have exposed shifts within std dev(δ18Oc) of surface and thermocline dwelling foraminifera. One can, therefore, question what is being reconstructed in such studies.". This study has, at this point, not tested whether the Std.dev of δ18Oc from individual foraminifera have reconstructed ENSO (also, the wording of this sentence is odd).

The first paragraph of the discussion (p9) purports to be about paleoclimatological archives that "have been used to indirectly and directly study past ENSO". However, the discussion is on mean-state reconstructions (Koutavas 2003, Dubois 2009). Koutavas 2003 is non-IFA mean-state reconstruction; likewise, the Dubois 2009 paper notes that “we prefer not to invoke any ENSO-like state for the glacial EEP based solely on our UK’37 SST.“ While it may be true that this result and Koutavas 2003 are at odds, this is not an issue of IFA or ENSO reconstruction, but rather aggregate analysis and mean -state reconstruction. Discussion of std.dev ENSO studies (modeled by Thirumalai, Koutavas 2006, Koutavas and Joanides 2012, Leduc 2009, Sadekov 2013, Rustic 2015) is not found, yet the following paragraph (see above) is largely about this approach. Further, significant discussion and analysis of IFA reconstructions of ENSO during the LGM is found in Ford 2015, which is not discussed here.

The main analysis uses an unrealistic mixed-layer depth of 400m for the models foraminifera. Symbiont -bearing forams (G. ruber and G. sacculifer) live in the photic zone, and thus modeling and analysis of these organisms should be constrained to these depths. The model results using the shallower depths and specific, photic zone depths (Figure 4, figure 5, Figure 6) show that much of the tropical Pacific is suitable for such analyses, provided adequate carbonate preservation. This is very much in contrast with the point made previously in the paper that much of the tropical Pacific is unsuitable. In these figures, confusingly, some figures show significant areas in white while others use gray for no discernable reason. The figures are also improperly labeled, according to the captions – in each figure, G. sacculifer is on the left, G. ruber is in the middle, and this is reversed in the caption. Which is which?

The conclusions are at odds with what is presented at various points in the paper. Specifically: “Overall, our results suggest that foraminiferal δ18O for a large part of the Pacific Ocean can be used to reconstruct ENSO, especially in an individual foraminifera Analysis approach is used, contrary to previous analysis (Thirumalai et al. 2013). This conclusion is contradicted in the abstract, and in various parts of the study (e.g., P10 – “the results presented here imply that much of the Pacific Ocean is not suitable for reconstruction ENSO studies with paleoceanography…”) Which is it?

Again, Koutavas 2003 is cited here, but that is not an IFA study. In general, clearly noting which studies are IFA/ENSO and which are mean state / aggregate / non-IFA studies will clarify the discussion surrounding the use of IFA and IFA techniques to identify ENSO signals.

This study does not directly address the Thirumalai 2013 study, as presented. The role of seasonality does not appear to be well addressed in this study (a key factor of Thirumalai 2013), the questionable definition of ENSO events confounds direct comparison, and the lack of clarity on sampling rates and other facts precludes a direct comparison. If this was a goal in this analysis, the Thirumalai study should be discussed in detail at the beginning (and should be discussed, in any case, earlier when discussing approaches for quantifying the suitability of locations for ENSO reconstruction), and the differences between their approaches (e.g., forward vs. inverse modeling). Suitable criteria for comparison should be noted (e.g., std. dev. Vs A-D tests).

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