

## ***Interactive comment on “Evidence for the non-influence of salinity variability on the coral Sr / Ca paleothermometer” by M. Moreau et al.***

### **Anonymous Referee #2**

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Moreau and co-authors investigate the influence of salinity changes on the Sr/Ca temperature proxy in shallow water corals. To the best of my knowledge, this is the first study that makes use of the large number of published coral Sr/Ca records to address such a potential influence that could complicate the reconstruction of past temperature changes at seasonal to interannual time scales. Therefore, I think the topic of this study is suitable for publication in a journal such as Climate of the Past. However, I suggest a number of substantial clarifications, additions and extensions in order to enhance the impact of the study.

#### **(1) APPARENT FORAMINIFERA Mg/Ca ANALOGUE**

I do not see the need to include a parallel discussion on the influence of salinity changes on the foraminifera Mg/Ca temperature proxy. This appears to be somehow

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odd, as this discussion addresses a different ion and a different group of organisms, and should be better eliminated from a revised version of the manuscript.

## (2) UNCLEAR CRITERIA FOR SELECTION OF CORAL RECORDS

It is not clear to me which criteria were used by the authors to select the coral records investigated in their study. Given the potentially larger number of coral Sr/Ca records stored in the NOAA Paleoclimate and the Pangaea databases, and the even larger number of records published but not archived, it seems that the authors have used only a small percentage of what is available. I strongly recommend to give a detailed explanation why these particular records were selected or, alternatively, to add some more records to enhance the overall impact of the study. Probably, a good strategy would be to confine to corals of the genus *Porites* and to those records that are seasonally resolved.

## (3) ACKNOWLEDGEMENT OF SHORTCOMINGS OF CURRENTLY AVAILABLE SALINITY DATASETS

There is a clear lack of reliable instrumental time series for sea surface salinity. This should be acknowledged by the authors. SODA salinity is a reanalysis product that involves a lot of modelling, and that in many oceanic regions does not include real salinity observations. The SODA SSS time series for a given location can differ strongly from one version to the next. However, SODA is the only gridded SSS product that is currently available. As the authors know, gridded SSS based on historical instrumental observations compiled by the French IRD team (Delcroix et al.) are available for selected oceanic regions. However, IRD SSS and SODA SSS can differ in terms of the amplitude of the annual cycle and interannual to decadal variability and trends. Consequently, the approach of the authors is prone to flaws in the accurate representation of the real SSS seasonality and interannual variability by gridded SSS products such as reanalysis SODA SSS at the selected coral sites. This is not the fault of the authors, but this should be clearly acknowledged. Testing the SSS influence for a few selected

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sites using both SODA and IRD SSS could enhance the impact of the manuscript.

#### (4) ACKNOWLEDGEMENT OF INTERLABORATORY OFFSETS IN MEAN CORAL Sr/Ca

The absolute values in average coral Sr/Ca are difficult to compare between different studies. A recent interlaboratory study for coral Sr/Ca has revealed large offsets between labs that are equivalent to several degC in terms of mean temperature, but which can be accounted for if a coral reference material is analysed in the different labs (Hathorne et al., G-cubed, 2013). However, these interlaboratory offsets definitely account for some proportion of the large range in calculated DeltaT (page 1790, line 1-4), which should be acknowledged.

#### ADDITIONAL COMMENTS:

Page 1785, line 9: 'limiting diagenetic effects', relative to what? Give more explanations, or delete.

Page 1785, line 15-17: Provide more information or delete. As currently written, this sentence is not understandable for the general reader (attenuation leading to overestimation?). It should be mentioned that Gagan et al. have demonstrated this for coral d18O, and have extrapolated their findings to Sr/Ca. Importantly, this is not a problem confined to the Sr/Ca temperature proxy.

Page 1785, line 19: de Villier 1994 and Sun 2005 are missing in the reference list.

Page 1785, line 20-21: There are probably equally or more important questions, such as the potential influence of growth rates (calcification rates) on the coral Sr/Ca temperature proxy and the correction for changes in the Sr/Ca of seawater on glacial – interglacial time scales.

Page 1786, line 11-17: The between-site salinity range of 7 psu should not be directly compared to the salinity range of 34-35.3 psu observed in the Western Pacific Ocean at seasonal to interannual time scales. This paragraph needs to be re-arranged.

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Page 1786, line 18: should be 'first . . . coral Sr/Ca record. . .' from Clipperton?

Page 1787, line 16: Peak matching using Reynolds (2002) SST?

Page 1788, line 10-20: Are the authors sure that the transformed Correge 2006 calibration, converted to SST, is correct:  $SST = \dots$  (line 11)? It might better anyway to calculate SST anomalies (using just the slope value of the Correge 2006 calibration) and not absolute SST, as the Correge 2006 calibration is representative for mean absolute SST of about 25 degC, which is probably not the mean SST at all coral sites investigated by Moreau et al. Therefore, this approach could probably lead to biases in the DeltaT calculation which, if true, should be acknowledged.

Page 1790, line 1-4: Please investigate and/or acknowledge that interlaboratory offsets in average coral Sr/Ca, vital effects resulting in different average Sr/Ca values for corals from the same reef site, and the use of the Correge 2006 regression to calculate absolute SST from corals that do not grow at mean SST of about 25 degC, could all contribute to the reported large range in calculated DeltaT.

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