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Interactive comment on "Calcification intensity in planktonic Foraminifera reflects ambient conditions irrespective of environmental stress" *by* M. F. G. Weinkauf et al.

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We are thankful for the helpful comments of referee #2, which touch some critical points of our paper. In the following we explain how we intend to address these issues in the revised manuscript.

We are responding to the issues raised by the referee one by one. The repetition of the referee comment is in each case followed by our response to it.

11214 Line 14: 'Calcification intensity' expressed by size-normalised weight. 'Intensity' seems to imply something about the rate but do we have good evidence that the life cycle or calcifcation time per chamber remains constant?

The referee raises an interesting question. In fact, we were choosing the term "calcification intensity" to contrast our measurements from "calcification rate". In our opinion, "intensity" represents the amount of calcite present in the shell at a certain time, whereas "rate" would imply the performance of repeated measurements during the life of a foraminifer and analysis of the increase in the amount of calcite per time-unit or chamber. As such, we were using the term "intensity" to clarify that we are not dealing with such measurements, nor have we reason to believe that the calcification remains constant during the life-cycle of the foraminifer. Based on those arguments, we would prefer to keep using the term "calcification intensity". We will, however, include a short explanation comparable to the one above, to make our point of view clear and avoid misunderstandings.

11222 Line 5 and throughout: The weight results are expressed as weight per unit area, rather than weight – needs attention. If the weights are to be normalised to size, then the relationship between volume and cross sectional area needs to be included.

We are aware of the fact, that normalizing the weight for the cross-sectional area rather than the volume of the test introduces a certain error. This error, however, is likely to be very small, because as long as the form remains the same, the volume of the shell is directly proportionate to its cross-sectional area. This applies within the individual species (especially in *O. universa*, whose shell is virtually spherical, and in *G. scitula*, whose shell is flat), but makes comparisons of absolute values between species impossible. Therefore, when comparing different species, where this problem would be significant due to the different shape, we use standardized/normalized data, which eliminate this influence.

Nevertheless, it is fair to say that the normalization we use assumes that shape remains roughly similar among the analysed specimens within each species and we will include a discussion of this assumption in the revised manuscript.

11222 Line 1: Perhaps it would be better to have δ^{18}O *measurements on a deeper*

dweller as well?

Such isotopic measurements have been performed at high resolution by Rohling et al. (2004), for numerous species of planktonic Foraminifera (including *G. scitula*) for the same time interval in the Eastern Mediterranean. Those analyses (fig. 5 in Rohling et al. 2004) show the same general trend in the δ^{18}O values in all species, with deep dwelling species like *G. scitula* generally showing higher isotopic values than shallow dwelling species. Those results show, that the deeper water column was influenced by the freshwater influx as well, which is also indicated by our data. We will include a more explicit reference to those data in the revised version of the manuscript.

11224 Line 19: Perhaps this can be the case for δ^{18}O but are there no vital effects for δ^{13}C?

The referee is correct that the δ^{13}C values of shells of *G. ruber* are not a straightforward environmental proxy. We will delete this term in the revised version of the manuscript.

11225 Line 16: Can the authors confirm that the 'modern' samples are both 'preindustrial' (i.e. not influenced by recent CO_2 change)?

Since there are no age models available for the cores, this question can only be answered on the basis of estimated sedimentation rates. We always used the coretop sample (0–1 cm) for our analyses. Atmospheric CO_2 levels were reconstructed from data from the Law Dome ice cores (spline fit, 20 year cutoff, http://cdiac.ornl.gov/ftp/trends/co2/lawdome.smoothed.yr20) for the time before 1959, and from measurements from Mauna Loa (NOAA) after that date (ftp://ftp.cmdl.noaa.gov/ccg/co2/trends/co2_annmean_mlo.txt). Pre-industrial atmospheric CO_2 values were estimated to have been 280 ppm (http://www.noaanews.noaa.gov/stories2005/s2412.htm). Therefore, on the basis of these data, CO_2 levels has been 10 % higher than pre-industrial from 1932 onwards, and 20 % higher than pre-industrial from 1979 onwards. This means, part of the investigated Foraminifera will have lived during the industrial era. By assuming constant sedimentation rates and constant sedimentation of foraminiferal shells (which

is reasonable over those time spans) we can approximate how many of our investigated Foraminifera will most likely have lived during times with elevated atmospheric CO_2 levels.

Seiter et al. (2005), doi:10.1594/PANGAEA.227904, published sedimentation rates of 3.6 cm/kyr for piston core RC08-18, at a comparable position to M34-3/3810-2. This implies, that our sample roughly spans 280 years. Given that core M34-3/3810-2 was taken in 1996, that means that about 77 % of specimens from the Atlantic modern reference sample will have lived before the atmospheric CO_2 was more than 10% higher than pre-industrial, 17 % during times with elevated CO_2 levels of up to 20 % pre-industrial level, and *c.*6 % of specimens during times with atmospheric CO_2 levels larger than 20 % higher than pre-industrial.

For the Western Mediterranean, close to the position of core POS334-79, Hayward et al. (2009), doi:10.1594/PANGAEA.773780, calculated sub-recent sedimentation rates of 7.2 cm/kyr. Our sample, which was taken in 2006, thus presumably spans about 140 years. This means that 47 % of specimens come from times before the CO_2 level rose by 10 %, 34 % of specimens from the timespan between 10 % and 20 % CO_2 rise, and 19 % from times with elevated atmospheric CO_2 levels of more than 20 % pre-industrial state.

In summary we can therefore not exclude the possibility, that the modern reference samples are partly influenced by elevated CO_2 levels in the atmosphere. Nevertheless we can assume this influence to be relatively limited on the basis of the calculations above, because a high proportion of the specimens in the sample has lived during times with CO_2 levels not significantly higher than pre-industrial. This parameter is also unlikely to have influenced the results, however. The modern reference samples show constantly smaller SNW values in *G. Inflata* (pre-sapropel) and higher ones in *G. ruber* (pink) (post-sapropel). This general trend of modern reference samples lying in between pre- and post-sapropel conditions can be directly observed in the two other species, for which data for both conditions exist, well correlated with the presumed

salinities in the respective water masses. If the influence of potentially elevated CO_2 levels in the modern references would be significant, one would expect the shells to be consistently lighter than the palaeontological samples. Furthermore, such an influence would in the worst case introduce a constant offset in the normalized weights, which would be the same for all species, and would not invalidate the observed trends in our data, since such an offset would be eliminated by the normalization process. We will discuss that point in the revision of the manuscript.

11228 Line 16: Why 'presumably'?

The referee is right that "presumably" in inadequate at that position. We will replace it by "the data indicate that. . . " in the revision.

11229 Line 19: The samples just prior to S5 are at most 1600 years older than S5 and as such are not from MIS 6. The suggestion that heavier weights at this time relative to the Holocene might be due to higher carbonate saturation as a result of glacial pCO_2 values is not well supported.

The referee is most likely referring to the expression "late glacial and deglacial times", which implies that the record would reach as far back as MIS 6. This is obviously not the case. We will rephrase this sentence.

Furthermore, we did not mean to imply that a higher pre-sapropel $\left[CO_3^{2-}\right]$, that we propose as the reason for the heavier calcification, was linked to glacial pCO_2 values. On glacial–interglacial time-scales, the alkalinity of the Eastern Mediterranean is heavily influenced by sea-level. During sea-level low stands, the basin was more restricted, which resulted in higher residence time and raised alkalinity and thus carbonate saturation. The presence of this glacial water body or its remnant prior to the enhanced freshwater influx leading to the onset of the sapropel deposition is a more likely explanation for raised carbonate saturation prior to the sapropel. We will clarify this point in the revised manuscript.

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