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Biogeosciences Discussions



# *Interactive comment on* "Calcification in the planktonic foraminifera *Globigerina bulloides* linked to phosphate concentrations in surface waters of the North Atlantic Ocean" *by* D. Aldridge et al.

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We greatly appreciate the time and effort that both reviewers have put in to provide us with feedback. A number of valid points and interesting considerations have been raised that we feel will greatly strengthen the manuscript. First we will address the general comments that have been made by both reviewers:

The need to check for other relevant environmental factors

Both reviewers suggested that we look at other environmental variables, notably tem-



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perature and chlorophyll. We accept that these data would provide useful additional information, and we will include SNW comparisons with each of these factors in a revised manuscript – where appropriate new information will also be added to the abstract, introduction, methods, results, and discussion, in order to fully incorporate temperature and chlorophyll into the revised manuscript. Surface chlorophyll values, measured from seawater collected in Niskin biottles attached to the CTD , will be used where available Where chlorophyll data from the CTD is not used, data from the surface underway fluorometer (calibrated to provide chlorophyll a concentrations [ug/L]) will be utilised. Reviewer 1 suggested including some of the key data in the form of a table. We agree that this would be useful and will include a table in the revised manuscript, providing values at each station for: location, volume of water sampled, time the net was deployed, G. bulloides abundance, average diameters, SBWs, MBWs, [PO43-], Nitrate, [CO32-], SST, and chlorophyll a.

Potential methodolological problems

As requested by reviewer 1, towing data (duration and filtered volume) will be provided. This reviewer proposed that the low flowmeter readings at three sites may in fact indicate 'clogging of the net due to high productivity' (which flowmeters are 'notably made for'). We believe this is unlikely for two reasons: 1) In order to avoid clogging, and knowing that longer deployments often result in clogging (as mentioned by this reviewer), we deliberately deployed the net for no more than five minutes at each station. 2) The chlorophyll data (surface underway and CTD bottle samples) suggests that algal biomass was not any higher at these sites. Reviewer 1 also suggested that there may have been an error in the calculation of the average flow rate (0.98 m3s-1), which appeared to be too large. We did indeed find that this value had been miscalculated – the revised value is an order of magnitude smaller (0.092 m3s-1). This revised value however does not affect any of the data trends in the paper and will be used in the revised manuscript. We can also confirm that when counting the forams, the sample was homogenised by gentle inversion of the sample bottle – this information will be



BGD

8, C4057–C4062, 2011



inserted into the methodology section.

The need for a better indicator of 'optimal growth conditions'

We agree with both reviewers that foraminifera abundance is not a perfect way to assess optimum growth conditions. The main reason that this was chosen was in order to compare our results to previous studies which have utilised the same proxy (de Villiers et al., 2004 and Beer et al., 2009). Reviewer 1 suggests we instead use a method to estimate G. bulloides growth rates from a model that takes into account temperature and chlorophyll. Aside from the fact that this would prevent direct comparison of our results with the studies mentioned above, the authors responsible for this model state that growth rate estimates are positively related with abundances from plankton net data. Therefore, we do not believe that the use of this model would significantly alter the findings of our study (relating to the effect of optimum growth conditions on SNWs). We do agree that the fact that we found lower SNWs at higher nutrient concentrations (thus at 'higher [potential] optimum growth conditions') needs to be discussed in light of temperature as well as potential inhibition by phosphate. This will be included in a revised version of the manuscript. Finally, reviewer 2 points out that we should discuss the fact that a 34 ug/kg range in [CO32-] is narrow compared to the range of PO43-. Although we allude to this in the paper we do agree with the reviewer, and this will be stated more explicitly.

Other considerations

Reviewer 2 states that we should mention the fact that weight increase may result from more than just shell thickening, using the example that glacial specimens show more crusting than Holocene ones. Whilst this may be true of specimens from sediment cores, we do not see how this would affect our samples. We will mention however the reason that our samples were taken from surface waters of the ocean - as when planktonic foraminifera are subjected to low temperatures, for example, when normally surface dwelling organisms sink into colder deep water, the secondary thickening of



BGD

8, C4057–C4062, 2011

the calcite wall (forming a so-called 'calcite crust') can occur (Hemleben et al., 1989). We accept that several different genotypes of G. bulloides exist that may have similar SNWs under similar conditions, and we will mention this in the revised manuscript, although the small latitudinal range that our samples were taken from will have minimised this effect.

Detailed comments – Reviewer 1

P6448, Line 10-11: This has been done.

P6448, Line 12: Whilst a positive relationship would be (arguably) minor evidence, we report a negative relationship between optimum growth conditions and SNWs. However, we do not mention this explicitly in the text. Therefore, we have added this information to the results and discussion sections.

P6453, Line 12-15: We considered it was important to show that once SBWs were converted to MBWs in our study that there was no longer any statistically significant relationship with test diameter. As this is central to all arguments we present here (we wanted to assess the response of test thickness, not test size, to environmental factors) we thought it was important to include Fig. 2 even though the theory behind it is well established. We will however clarify any potential confusion between SNW, SBW, and MBW by clearly stating that SNWs used from the results onwards are MBWs.

P6453, Line 19: Corrected.

P6453, Line 22: Corrected.

P6454, Line 6-7: 'Non-toxic' replaced with 'sub-surface intake'.

P6457, Discussion part 4.1: As this paragraph contains analysis of results, we think it is better placed at the start of the discussion section. However, we will indicate in the results section that SBW is not used to derive SNW. By 'adequately', we are referring to the fact that although there is still a positive relationship between MBW and diameter, this relationship is not statistically significant – in our opinion 'better than' does not



BGD

8, C4057–C4062, 2011



capture this key point.

P6459, Discussion part 4.2.2: We think this paragraph is useful in its current form as it suggests a number of reasons why the results of de Villiers et al. (2004) have not been repeated. However, we will discuss the fact that nutrient concentrations could be an indicator of optimal growth conditions, and why an inverse relationship in the current study could be due to phosphate inhibition and/or temperature.

P6460, Discussion part 4.2.3, Line 2-5: Agreed, we will mention this and the potential effect of temperature will be discussed in a new subsection within the discussion.

P6461, Line 2-3: sentence removed.

Fig. 3b and 4: Done (for three of the 10 points), although this is only necessary for Fig. 3b (Fig. 4 is nitrate vs phosphate).

Detailed comments – Reviewer 2

P6448, Line 21-22: Corrected.

P6449, Line 16-17: 'Rate', wherever used in the manuscript in reference to foraminifera, has been replaced with 'intensity'. The reasoning for doing so is explained upon first use.

P6450 – 6451, Section 2.1: This point will be mentioned in the revised manuscript.

P6452, Section 2.3: 22 % (2/9) of the flowmeter readings were obviously wrong (the other station where an average flow rate was applied, the flowmeter was not attached). These two volumes (1.47 and 3.7 m3) were a lot lower than the other 7 readings (average, 25 m3; range, 10-39 m3). We believe these 7 readings to be accurate based on the fact that the volume of water sampled at these 7 stations was positively, and significantly, related to deployment time.

P6453, 24-25: We accept the reviewer's concerns here, and have included the point that was raised. However, given the small number of these specimens used in the



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8, C4057–C4062, 2011



final analysis (42/309; 14 %) we do not believe that this will have had an impact on the overall findings, especially given the size-normalisation procedure that was applied.

P6454, Section 2.5: A lot more information has been added describing the protocol used for deriving [CO32-].

P6456, Line 23-24: We address the issue raised here briefly in the discussion (P 6461, Line 14-16). However, having read the suggested papers we have added some extra information on the link between G. bulloides abundance and primary productivity, as well as explicitly stating that algae forms an important component of this species' diet.

P6457, Line 9 and 12: Changed.

P6458, Line 14-15: Corrected.

P6461, Line 2-5: Sentence removed.

P6467, Table 1: Reference added to table.

P6470, Fig. 3: Having checked the statistical significance of this trend with the data point removed, we are not sure if mentioning this in the manuscript would be worthwile. The relationship of SNW with [CO32-] is positive even with this data point left in, and removing it does not make the positive trend significant.

P6470, Fig 4: The relationship between nutrients and primary productivity (chlorophyll a) will be explored in the revised manuscript.

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Interactive comment on Biogeosciences Discuss., 8, 6447, 2011.