**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.

Anonymous Referee #1

Received and published: 21 July 2011

Review of the manuscript "Calcification in the planktonic foraminifera Globigerina bulloides linked to phosphate concentrations in surface waters of the North Atlantic Ocean" by D. Aldridge, C.J. Beer, and D.A. Purdie

General comment:

This manuscript present new evidences on the different factors controlling planktic foraminifer calcification, and notably how size normalized weight of foraminifer shells (which are used to estimate $[CO_3^{2-}]$ from paleo-records) respond to various signals

including nutrient concentration and "optimum growth conditions". The major finding of this manuscript is that nutrients (phosphates and nitrates) are the main drivers of foraminifer calcification while $[CO_3^{2-}]$ and optimum growth conditions plays only a minor role. This paper present interesting data that respond to important questions: "do we miss some important processes when focusing only on ocean acidification?", and thus brings new pieces of information. As it, I think the manuscript presents interesting and novel data that could be published in Biogeosciences after strongly considering the points listed below.

However, I find also that the present paper focus too much on nutrient influence and suffer from some lack of data/analysis on other environmental factors which are even not presented. Some other aspects need also to be improved or better explained such as the method used to estimate the "optimal growth conditions", and potential methodological problems in the sampled volume by nets (notably the correction). I will detail all those points below. The language used sometimes in the manuscript looks a little bit too much "chatty" and needs to stick to the fact and avoid nice styling effect that brings no information. Few examples are "while this is by no means proof that phosphate is a major factor..." (line 2-3, page 6461); "More broadly, these results.... Leads us to echo the sentiments of de Villiers (2004)" (line 5-7, page 6462) or the last sentence of the manuscript.

The need to check the effect of other relevant environmental factors:

When looking for the results, it seems that some environmental variables were not considered at all while they have been sampled in this cruise (see the cruise report here (hypertext link if clicked)). This is notably true for temperature and chlorophyll a (or fluorescence) that has been sampled at each CTD cast in this cruise. I suspect that those environmental variables could also play a role in foraminifer calcification: temperature because it generally increases all biological and chemical reactions and chlorophyll a because it could be an indicator of available food for foraminifers and generally co-varies with nutrients. When looking the data, I suspect that regions with higher nutrient

concentrations are close to Iceland (see here (hypertext link if clicked) ; phosphate, climatology, summer, surface) and thus nitrate and phosphate are certainly also correlated with both temperature and chlorophyll a (see here (hypertext link if clicked)). Thus from the analysis, it could be possible that those environmental factors may also play an important role in foraminifers calcification. I then strongly suggest taking in accounts these environmental variables. The inclusion of this will maybe modify your results or interpretation. There is also a need to have access to the data itself (I suggest as a table) recapitulating location, volume sampled, foraminifer abundance, SNWs and environmental variables.

The need for a better 'optimal growth condition' indicator:

The present manuscript used foraminifer abundance as a proxy for optimal growth conditions. However, foraminifer abundance is not a perfect way to assess growth conditions: effectively at the beginning of the population growth (when growth conditions are optimal) population abundance is still low, while at the end of population growth (when growth conditions are not optimal anymore) population abundance is high. This process have been discussed recently (see Lombard et al 2011 (hypertext link if clicked)) and thus drive to the conclusion that the "optimal growth indicator" used here may be inadequate. In the same study is proposed a method to estimate G. bulloides growth rate from temperature and chlorophyll a data. Since those data have been sampled in the cruise (see above) maybe this could be used instead?

Additionally, one thing need to be discussed: because chlorophyll a and temperature are suspected to varies with nutrients (see above), the fact that nutrients seems to control SNWs could be an indication that nutrient concentration could be a better proxy for optimal growth conditions of foraminifer than just abundance. . . this relationship have notably been shown by Schiebel et al 2001 (Deep-Sea Research I 48 721-740). However, you found that SNWs are lower at higher nutrient conditions (thus at higher optimal growth conditions) which is contrary to de Villier (2004), Hecht (1976) and Schmidt et al (2004) findings. This could be explained either by a potential inhibition

of calcification by phosphate. . .. Or by an effect of temperature (which I suppose is lower when SNWs are lower) that slow down the calcification process. This needs to be discussed.

Potential methodological problems in sampled volume or sampled volume corrections:

Without any further indications on the towing data (effective towing duration and filtered volume), I suspect some methodological or calculation problems in this calculation and/or correction, thus casting some doubt about abundance data (which are arguing in a long paragraph , part 4.2.2, page 6459, to be more confident than previous measurements). First, towing duration and filtered volume should be indicated, this would allow understanding why a volume of less than 50m3 is considered to be an abnormal data.

Secondly, (depending on the filtered volume obtained on other sites) I slightly doubt about the fact that lower filtered volume may indicate a failure of the flowmeter (at least without mechanical indication of a failure). This on the contrary, may better indicate a clogging of the net due to high phytoplankton/zooplankton abundance, which is likely to have occurred knowing the small mesh used, the horizontal towing (that certainly occurred on a long period, c.a. 10 min). Flowmeter are notably made for this: taking into account the net clogging. I then wonder if this "filtered volume correction" is really needed or justified, thus enforcing the need to provide towing data.

Thirdly, when checking the few towing data presented here (towing surface: 0.196m2; estimated flow rate 0.98m3 s-1), I've got stroke by two points: This gives a towing speed of 5 meter per second (this is traveling speed, with such a speed your net should filter 50m3 in one minutes or more likely explode quickly; maybe this is a mistake and one zero is missing somewhere), and this gives EXACLTY 5 meter per second (is this really a result of the linear regression stated on part 2.3, line 15, page 6452). This again enforces the need to provide towing data.

Finally when counting the foraminifers from 1mL aliquots, did you shake the sample to homogenize it? Knowing that shells generally sinks to the bottom of the sample, this could be a problem for the abundance calculation (except if you counted the entire sample).

Detailed comments:

Abstract:

Line 10-11: maybe give the range together with "([PO42-]; range:...)"

Line 12: "minor evidence" rather than "no evidence"

2.4 SNW analysis (Page 6453)

Line 12-15: if you know that SBW is not adequate, why still using it in part of the manuscript (Fig2 notably). You also have to better state in the manuscript (certainly in results part), that The SNW used afterward in the manuscript are in fact MBW. In general the use of close acronyms (SNW and SBW) is confusing and do not help the reader.

Line 19: "transfer" rather than "transference"

Line 22: "measurement" rather than "measuring"

Page 6454, Line 6-7: "non-toxic seawater supply" (???). Did you have a toxic one on board? What is the purpose of stating this (notably knowing that you poison everything afterward)? If there is no use for this statement, please remove it.

Discussion part 4.1(page 6457): this should go in the result part to better indicate that SBW is not used in results afterward. Line 7: replace "adequately" by "better than SBW": size is still responsible for 35

Discussion part 4.2.2 (page 6459): I suggest reducing this long paragraph by less arguing about the confidence of your results. Other things needs to be discussed,

C2109

notably the fact that nutrient could also be an indicator of optimal growth and why the optimum growth vs SNW relationship follow an inverse trend to previously observed (which may confirm the phosphate inhibition if temperature have no effect).

Discussion part 4.2.3 (page 6460), Line 2-5: this could also be due to the effect of other co-varying factors (such as T°C) that are not currently taken into account.

Page 6461 line 2-3 "while this is by no means proof that phosphate..." please avoids this kind of sentence... rephrase or remove when this brings no information, same thing for page 6461 lines 6-12.

Figure 3B and 4: please indicate the datapoints where you used your filtered volume correction.