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Interactive comment on "Upper ocean mixing controls the seasonality of planktonic foraminifer fluxes and associated strength of the carbonate pump in the oligotrophic North Atlantic" *by* K. H. Salmon et al.

Anonymous Referee #1

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Salmon et al present new planktonic foraminifera (PF) shell flux data from a classic site in the North Atlantic. The combination of high-resolution shell flux and a wealth of hydrographic and biological data makes this an ideal site to investigate seasonality in the shell flux of PF. This is important work, not only for paleoceanographers who use the shells to reconstruct climate, but also to understand intra-annual variability in carbonate pump since foraminifera contribute significantly to the carbonate flux to the deep sea. The authors present shell flux data for 11 species and suggest that the total planktonic foraminifera flux (PFF) is related food availability, which in turn is dependent on mixed

layer depth dynamics, and the occasional occurrence of mesoscale eddies (vertical and horizontal mixing of the upper water column). Based on these observations they also suggest that the North Atlantic Oscillation might have an influence on the strength of the carbonate pump. This is a well-written paper that presents valuable new data that are of interest to many researchers in the fields of biological oceanography, micropaleontology and paleoceanography. Nevertheless, I do have some comments, which I am afraid require a significant restructuring/re-writing, but hopefully help to improve the paper.

Major comments

Species-specific vs total shell flux variability:

In the introduction the authors cite work by Kuroyanagi and Kawahata (2004) to highlight that different species respond differently to environmental factors and they use this as an argument for their study to better understand what controls PF fluxes. The data the authors present agree with this statement as they show clear differences in the flux patterns between species (even between those supposedly living in the same depth range). The authors even highlight the difference in the timing of the maximum flux between the deep dwelling species. Nevertheless, when attempting to explain what drives the variability in the fluxes they ignore this species-specific behaviour and only describe the total PF flux. This needs to be addressed. At the minimum the authors need to discuss the different patterns in the three groups of species they distinguish (surface, intermediate and deep dwellers), but ideally they also attempt to explain what drives the differences between the individual species in these groups. Moreover, the division into the groups in terms of depth habitat is in this respect perhaps not the most useful and the authors may want to divide the species into groups that share similar shell flux patterns. This will undoubtedly lead to less noisy relationships between the environmental parameters and the PFF and provide a clearer picture of the real drivers of PFF variability. When it comes to the species-specific flux patterns the difference between the timing of the peak flux of the deep dwellers is interesting and something the authors point out, but do not discuss. Similarly, there seem to be clear changes in variability in the flux between years for several species (G. sacculifer, G. ruber (white) and G. siphonifera) and (surprisingly) apparent absence of seasonal variability in some surface dwellers (large changes in temperature and other environmental parameters). A more species oriented discussion would also allow for a more detailed comparison with the seminal work by Deuser and others as well as the plankton tow work of Tolderlund and Be (1971) at this location.

Mixed layer dynamics and PF flux (PFF):

I find this section of the manuscript (§5.1.1 and 5.1.2) difficult to understand, or rather, difficult to understand the mechanisms that would underlie the observed relationships. At the beginning of the §the authors mention observations from the high latitudes that show a relation between MLD deepening rate and deepening timing and productivity. Subsequently the deepening rate and its potential influence on PFF is discussed and another parameter (shoaling rate) is introduced, but the timing of the MLD deepening is not discussed. I find this confusing and have several comments on this section:

1. First of all, the mechanism the authors use to explain how MLD can exert an influence on the PFF is indirect and via export productivity, which they derive from surface chl-a concentrations and carbon fluxes at different depths. Since the authors state that the foraminifera rely on the organic matter flux from the surface ocean for food (and hence growth and hence flux), it would make sense to explore this relation first and then focus on the relations between OM export and MLD dynamics. The relation between PFF and MLD is only indirect and correlations between PFF and MLD could therefore be misleading.

2. Secondly, while the observed relationships the authors identify in Fig. 9 are statistically convincing, I struggle to grasp what the actual mechanism(s) explaining these observation is (are). Does it mean that the springbloom has always approximately the same length (relation between Dr and MLD)? And if Sr has a major influence on pro-

ductivity and PFF why do fluxes increase when conditions are changing to less optimal conditions (stratification) more rapidly? This needs more explanation and the authors need to be clear about whether it is absolute MLD, Dr, Sr, or their ratio (or something else) that controls the PFF (see also P12233, L28 and beyond where in one year the large flux is suggested to be related to Dr and MLD and in another year only to MLD, whereas in Fig. 9 and on P12233, L 21-23 PFF is said to depend on the Dr/Sr, which mainly reflects the Sr).

Some additional minor comments on this section:

1. What is the basis of the 80m limit used to determine the relation between MLD and PFF? It seems arbitrary.

2. The shoaling rate in year 2000 represents only a minimum value since there is a month missing during the decrease of the MLD. This could affect the results of the regression.

3. The deepening rate is defined as the average of the rate between Oct and Jan. Why this interval and not the average rate over the entire period of MLD deepening (which extends beyond Jan)?

4. How are the start and end of the spring bloom period defined? And therefore the average winter-spring flux? This needs to be done objectively in order to compare different years. And in addition, wouldn't the integrated flux be a better parameter to investigate?

5. What is the physical reason the fit a logarithmic curve to the data in Fig. 9D? Would one expect such a relationship with decreased sensitivity at faster rates?

Link between PF flux mesoscale eddies:

The suggestion that eddies can under certain circumstances prolong and/or enhance primary productivity and PFF is very interesting. Discussing what these circumstances are exactly would contribute to the quality of the paper. There are several times when the passing of an eddy did not have an effect on the PFF (e.g. winter 98/99, 07/08). Where they too early? Or were other conditions not met? It is interesting that the eddy in 2010 seems to be associated with increased PFF and organic carbon fluxes, but not with higher chl-a (i.e. no bloom?). Could this perhaps point to lateral advection of foraminifera or of organic matter (which would fuel foraminifera growth without requiring an increase in primary productivity) within the eddy?

Influence of NAO:

The suggested link with NAO is very interesting. It is based on two years with positive-neutral NAO and one year of negative NAO. Of the years of positive/neutral NAO one is from a trap more than 1500m further down and it has often been shown that PFF recorded at different depths can differ significantly. Therefore the suggestion that NAO might have an influence on the PFF is effectively based on only two years of observations (what about the other years in this study? How do they relate to NAO?). While it is an interesting suggestion that the NAO can have an influence on the PFF and therefore on the C-pump I think it's only that: a suggestion and the conclusions should be toned down accordingly (also in abstract).

Minor comments

1. The shell flux data should be made available, either as a table in the paper or through an on-line database such as Pangaea.de.

2. P12224, L2: oligotrophic regions are by definition nutrient poor.

3. P12226, L20: warm temperatures should be high temperatures and similarly cold should be low. Please change throughout the manuscript.

4. P12229, L9-16: this should be part of the paragraph describing the OFP time-series (3.1).

5. P12229, L19: perhaps change 'relative' to 'in relation'?

6. P12229, L24: 'deep spring fluxes' is not clear.

7. P12231, L20: t is not clear that the timing of the maximum is really later. I suspect that given the inter-annual variability this difference is insignificant.

8. P12234, L11: mesoscale eddies are also an environmental control on the PF, the §should therefore be 5.1.3.

9. Fig. 2: please be consistent in the use of symbols for the parameters other then PF.

10. Fig. 3c,d: what is the criterion for splitting at 80m?

11. Fig. 4-6: please add SST curve to graphs to provide clearer information about when the peaks occurred. There is no gap in the time-series of G. sacculifer in Fig. 4.

12. Fig. 7: place G. crassaformis on its own axis to show the variability.

13. Fig. 9c: I assume that the extreme chl-a datum has been excluded from the regression. Please indicate this clearly.

Interactive comment on Biogeosciences Discuss., 11, 12223, 2014.