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Climate of the Past Discussions

Interactive comment on "Planktic foraminifera and structure of surface water masses at the SW Svalbard margin in relation to climate changes during the last 2000 years" *by* Katarzyna Zamelczyk et al.

Anonymous Referee #2

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This paper shows severe weaknesses in all parts of the text:

1) The methodology is generally not explained clearly enough, which makes the further interpretation rather unconvincing: - The following sentence (Chapter 2.1, page 5) raises doubts about the good faith of the authors... "The depth-sampling intervals were assigned based on the distribution of water masses recorded by the CTD". This is simply not true. How can one imagine that there is no change in the vertical water stratification between October and July....Of course there is (see Figure 2)! The 0-50m interval is only correct for the October sampling. In other seasons, the authors sam-

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Discussion paper

pled a mixture of populations living in the mixed layer and below the thermos/haloclines. The further interpretation based on living faunas is therefore severely compromised. - Planktic foraminiferal (PF) faunas are given in number of specimens per cubic meter (Chapter 2.1, page 5). How was the volume of filtered water measured? The WP2 device cannot be equipped with 2 flowmeters, able to record the "in" and "out" fluxes. Thus to my knowledge, it is difficult to accurately estimate the number of ind/m3 with a WP2 sampler. A large error margin has to be taken into account, which was not done at all in the paper. - Chapter 2.2, page 5: what kind of box core was used? Are the authors sure that the water-sediment interface was properly sampled? If the sampler is not completely closed, a washout may occur when the sampler is being retrieved. Consequently, to what extent are the PF counts in the surficial sediment correct? Since there I some uncertainty about the quality of this sample, the core top sample should perhaps be discarded. - Chapter 2.2, page 6: A sentence states "The small (100–125μm)- and large (150–180μm)-size shells represent different life stages, the juvenile and adult forms, respectively" I don't agree with this discrimination of different life stages on this basis of very similar test sizes! The size-fractions proposed here are not appropriate. For example, for T.quinqueloba, 125μm diameter is a "normal" adult size. It is a well-known small species... (e.g. Schiebel and Hemleben, 2005; Husum and Hald, 2012). - Chapter 2.2, page 6: Counting shell fragments is far from trivial. What did the authors count precisely? what sizes of fragments? how to be sure to count only PF fragments?

2) Some basic interpretations are not correct - Chapter 3.2.2: the authors consider fragmentation as an indicator of bad preservation. Which is correct, but the authors focussed only on CaCO3 dissolution to explain fragmentation, writing (page 17) "% fragmentation is low indicting well-saturated with respect to calcium carbonate…". Bad preservation can also be due to transport and/or reworking on the sea floor in areas of active bottom currents. Above all, CaCO3 preservation may be closely linked to early diagenetic processes within the sediment that have nothing to do with the bottom sea characteristics during the deposition! - Chapter 3.2.4, page 15: "the three

CTD casts taken during key-seasons for reproduction of planktic foraminifera at the core site". What are these key-seasons for PF reproduction at the studied site? The authors suggests that PF reproduction occurs 3 times a year, October, April and July. On which data is this free assumption based? What is the real timing of PF reproduction at the core site? To my knowledge, in this area there is absolutely no information available about PF reproduction seasons. - Chapter 3.1.3, page 11: One can read "..G. uvula was found at X. . . m water depth...., which could indicate that this depth is the calcification depth of the species". Unfortunately, the depth of calcification is generally not where you find most individuals! Calcification of PF starts at the reproduction level, possibly close to the pycnocline (for the studied species; Schiebel and Hemleben, 2005), and ends where PF are largest (i.e. end of calcification), just before the reproduction . . .. at the same pycnocline level! Calcification depth is still a matter of debate. Calcification depths could have been be discussed here with data of the modern fauna (see my point 3). - Chapter 3.1.3 - on page 12 is written: "This species is capable of living in salinities of 30.5–31, which appears to be the minimum salinity for planktic foraminifera (Boltovskoy and Lena, 1970b)". The authors should have a better look at the available, recent literatures! PF have a high tolerance to salinity changes (e.g., Bijma et al., 1990; Ortiz et al., 1995). They are not directly affected by low salinity (e.g., Fernandez et al., 1991), but rather by parameters that co-vary with salinity (e.g., Ufkes et al., 1998; Retailleau et al, 2009).

3) No isotope measurements were performed on the modern living fauna whereas the authors have good material to do this (plankton tows and Rosette CTD). They should have tested all basic interpretations from the literature with their own data set. For example, - compare 18O and 13C of the living fauna (trapped in the plankton tows) with measurements of the ambient seawater isotopes. This could help to identify the specific calcification depth. - compare the isotopic differences between living species with the observed water masses and subsequent stratification and thus verify the hypotheses presented on page 3 " N. pachyderma and T. quinqueloba ($\Delta\delta$18ONp-Tq) as an indicator of the subsurface-to-near surface Atlantic Water relative inflow and be-

tween T. quinqueloba and G. uvula ($\Delta\delta$18OTq-Gu) as an indicator of relative changes in freshening and stratification of the surface waters in the past."

4) Chapter 1.1 Oceanographic setting, gives in 10 lines a very vague and extremely general view of the studied system (without any reference to literature). It is more than necessary to explain here the seasonal and interannual variability of the intensity and location of the modern AF, on the basis of recent oceanographic measurements.

5) In chapter 4.2 Sea surface reconstruction..., the authors repeat/summarize their results and interpretation for (only) a single 30cm-long core. No clear comparison with other cores sampled in the vicinity (Eastern North Atlantic, west Svalbard) is presented. But a comparison is made with results from the broadly diversified North Atlantic Ocean domain! It would be very surprising if exactly the same processes influence PF faunas in the eastern Labrador area and along western Svalbard! Such comparison, without any reserve, suggests an ignorance of the functioning of the modern oceans. There is no real discussion, with sentences advocating the "success" of their study because it agrees with others! See in chapter 4.2.1 "The warm surface conditions... are in agreement with other studies from the North Atlantic Ocean Âż; in chapter 4.2.5 "There is a paleoproxy consensus of the progressive warming ...." Or the worst, in chapter 4.2.2 "Overall, taking into account differences in sedimentation rates, dating control, and marine reservoir age corrections, a warming ...in the Storfjorden Fan can be considered as a widespread phenomenon"

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