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**Biogeosciences Discussions**

# *Interactive comment on* "The Little Ice Age: evidence from a sediment record in Gullmar Fjord, Swedish west coast" *by* I. Polovodova Asteman et al.

**E. Alve (Referee)**

ealve@geo.uio.no

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Through detailed micropalaeontological, sedimentological, and geochemical analyses of sediment cores from the deepest part of the Gullmarfjord (Sweden), the present study aims to contribute to a better understanding of the climate variations in Northern Europe during the last millennium. Particular emphasis is on the Little Ice Age. The dataset is comprehensive with a very good time-resolution compared to many studies and seems to have a good potential for tracing short-term climatic variations. However, as it now stands, the interpretations do not always appear to be consistent and, in places, they seem to be self-contradictory (e.g., abstract lines 18-22: The authors state



that the onset of the LIA is indicated by a cold-water form (A. glomeratum), whereas the first phase is said to be characterized by a milder climate as indicated by N. iridea; lines 25-29: climax of LIA at 1675-1704AD (1550-1650 according to p. 15, line 13), whereas H. balthica is indicative of warming trends at 1600-1743. . .).

The introduction presents the views of different authors on when various phases of the LIA occurred in the North Atlantic region and what the causes might have been (please clarify p. 2, line 19 . . ..LIA started earlier..than..?). In order to put the present study into this broader context it would be useful if the authors, in the conclusions, include a few comments concerning how their results fit into the more regional picture.

A substantial part of the climate interpretations is based on the ecology of selected benthic foraminiferal species and their abundance/distribution through the appropriate time-intervals in the studied sediment cores. However, it is not always clear how well-founded the stated ecological information is and this leads to some unclear interpretations. E.g. it is not clear which refs are used to state that N. iridea indicates milder climate (warmer compared to what?). Is it possible that its abundance is dictated more by access to appropriate food than to a slight change in temperature (any literature with estimates $\sim$ how many degrees change we are talking about at the water depth in focus – or in other areas where a similar trend is recorded?)? H. balthica is suggested to be indicative of climate warming (based on what?) and A. glomeratum indicative of a cooler climate.

In order to avoid circular arguments, I would be careful to base interpretations of ecological characteristics on previous interpretations of fossil assemblages. E.g. (p. 11, lines 14-16), based on previous interpretations it is assumed that N. iridea responded positively to what is interpreted to be a warming event (1540-1610AD) and therefore it is considered to be a proxy of climate warming during the LIA. What is known about the temperature tolerance of this species? To sort out these unclear and sometimes apparently conflicting interpretations, it may be an idea to write a small section on the ecological characteristics of the species used. What do we really know about their

ecological “requirements” and how can this aid the interpretations?

Additional references on A. glomeratum include Schafer and Cole 1982; 1986, Kuhnt et al. 2000 (references below) and, for comparison with the development in Gullmarfjord, Alve 1991 found this species to dominate parts of Drammensfjord, inner Oslofjord, during the Little Ice Age.

To make the discussion easier for the reader to follow, the authors may consider to organize it chronologically and with a clearer connection between the time intervals mentioned in the text and those shown on the diagrams.

The present study is based on data from 3 different sediment cores, parts of the results have been published previously, and the results section is a mixture of previously published and new data. It would be less confusing for the reader if the results section only includes data new to this study and if the authors introduce the other data in the discussion.

Have the authors considered how changes in sediment accumulation rates affect the number of individuals/g dry sediment? There is a strong focus on temperature shifts being causal to changes in the foraminiferal assemblages. However, colder water may not be the main causal factor but rather reduced primary productivity/food availability as a result of decreased solar radiation and associated increased ice cover. The authors do touch on other possible factors but this may be explored more. Some more specific comments: p. 4, lines 23-25: what was the diameter of the cores? p. 4, line 32: Table 1 and Fig. 2 belong to the results section.

p. 5, line 8: should core GA113Aa be GA113-2Aa? p. 5, line 15: Can the authors please provide a short comment to justify the use of exactly Cassidulina laevigata (rather than e.g., H. balthica) for stable isotope analyses? p. 6, top: It is not clear why the number of species per sample is included in Fig. 5. Comparing the number of species in samples of different size is meaningless. This is the reason why the Fisher alpha index was introduced. Hence, as long as the same number of individuals (here



around 300) is counted in the samples the alpha-values and number of species will show the same pattern (as indeed they do in Fig. 5). Actually, I cannot see that any of this information is used in the discussion so, strictly speaking, it should not be included at all. (The same applies to planktonic forms). p. 6, line 6: analyses based on counting data only including species representing 5% or more in at least one sample. . ..?

p. 7, lines 5-6: available only for core 9004.... this is not consistent with p. 5, lines 16-17.

p. 8, lines 13-14: was the oxygen concentration measured? Is the position of the redox-cline relevant to the discussion?

p. 13, line 5: The authors need some more concrete arguments for their dissolution hypothesis – e.g., literature on the stability of CaCO3 vs temperature? Is it reasonable to assume that a decrease of e.g., 1-2 °C would affect the stability of calcium carbonate in this system? As for the organic linings, could they reflect increased transport from shallower water? E.g., as opposed to the shallow-water A. beccarii, most of the calcareous spp mentioned here do not have organic linings which easily survive drying of the samples. If the environment is that of cooler waters and a low phytoplankton productivity, this has been associated to be preferable to agglutinated forms. Therefore, a relative increase in agglutinated tests is not necessarily the product of dissolution. If dissolution is occurring, how can the presence of well-preserved N. iridea and S. fusiformis be explained? p. 13, lines 26-28: The authors state that three spp are reported in the recent fauna on the Fladen Ground. . .. According to the literature, these species are reported to occur in large parts of the NE Atlantic, so it is not clear what this information adds to the discussion. Besides, two of the mentioned spp (H. balthica and A. glomeratum) are interpreted by the authors to indicate “warm” and “cold” conditions, respectively. The basis for the author’s interpretations needs to be clarified.

p. 14, Line 8: When discussing land uses it should be made explicit how these land uses alter the fjord environment. p. 15-16, section 5.3: It would be useful with a short

conclusion as to whether using the sand sized fraction as a signal for storminess was successful (seemingly not?). Figs 3 and 4: Are the dates (yrs) for the starting and ending points of the time intervals on the right hand side (LIA, RW etc) results of the present study or taken from the literature? Fig. 4: dates on the right hand side of diagram – is 50BC correct? Where is the explanation of the abbreviations RWP, DA etc? Do F1, F2 etc represent the factor units? If so, this is confusing as the symbols in the legend are the same as for the "Herring periods". I guess 63mkm should be 63 $\mu$m? If the gray horizon at about 370 cm core depth (unit not shown) represents a turbidite, how come this is not reflected in the C/N-values, where did the C. laevigata shells used for stable carbon isotope analyses come from, and why is there no dramatic change in the faunal composition (e.g. presence of shallow water forams)? Finally, the authors should consider if this figure should have more emphasis on the time interval actually discussed in the paper rather than (as it now stands) on the time before the LIA.

References cited above: Alve, E., 1991. Foraminifera, climatic change and pollution: A study of Late Holocene sediments in Drammensfjord, SE Norway. The Holocene 1, 243-261. Kuhnt, W., Collins, E., and Scott, D.B. 2000. Deep water agglutinated foraminiferal assemblages across the Gulf stream: distribution patterns and taphonomy. In Hart, M.B., Kaminski, M.A., and Smart, C.W. (eds.), Proceedings of the Fifth International Workshop on Agglutinated Foraminifera. Gryzbowski Foundation Special Publication, 7, 261-298. Schafer, C.T. and Cole, F.E. 1986. Reconnaissance survey of benthonic foraminifera from Baffin Island fjord environments. Arctic, 39, 232-239. Schafer, C.T. and Cole, F.E. 1982. Living benthic foraminifera distributions on the continental slope and rise east of Newfoundland, Canada. Geological Society of America Bulletin 93, 207-217.

Although the text would benefit from some reorganization/clarification, the manuscript is generally well-written and all figures (with some modifications) and tables are needed and clearly presented. The English needs some improvement in places and the text could be slightly condensed. I suggest the manuscript should be accepted following



moderate revisions.

Oslo 16th November 2012

E. Alve

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