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

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Response to the interactive comments by Elisabeth Alve

We highly appreciate the valuable comments and criticism of both reviewers, which have helped to significantly improve the manuscript. Below we provide a detailed consideration of each comment and reply to the concerns of the reviewer #1 Elisabeth Alve (further referred as “EA”):

EA: 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

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(1550-1650 according to p. 15, line 13), whereas *H. balthica* is indicative of warming trends at 1600-1743)

REPLY: This has been corrected: “The onset of the LIA is indicated by an increase in cold-water foraminiferal species *Adercotryma glomerata* at ~1350 A.D. The first phase of the LIA was characterised by a stormy climate and higher productivity, which is indicated by a foraminiferal unit of *Nonionella iridea* and *Cassidulina laevigata*. Maximum abundances of *N. iridea* probably mirror a short and abrupt warming event at ~1600 A.D. The climate deterioration during the climax of the LIA (1675-1704 A.D.), as suggested by the agglutinated species and *Hyalinea balthica*, reduced primary productivity and caused a decline of *N. iridea* dependant on fresh phytodetritus.” For more detailed information on foraminiferal ecology of species *A. glomerata*, *N. iridea* and *H. balthica*, see our comments below.

EA: The introduction: please clarify p. 2, line 19 : : .LIA started earlier..than..?).

REPLY: This has been corrected to “Other studies, however, mentioned that the LIA started somewhere between ~1300 and 1400 A.D.“.

EA: 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.

REPLY: We added a sentence to the conclusions that our results are in agreement with previous studies based on benthic foraminiferal stratigraphy of the LIA from the Skagerrak region.

EA: 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

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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?

REPLY: To investigate this further, we added an additional table (Table 4, see in the supplement to this comment, we will also add this to the final version of the paper) summarizing the ecological preferences of this species. As it emerges from the Table 4, *Nonionella iridea* is a cosmopolitan species, which occurs within a broad bathymetric range and has slightly different temperature preferences depending on a shelf or a deep-sea habitat. However, if we look at temperature tolerance of this species within the Northern Europe and/or the depth range similar to deep Gullmar Fjord, it appears that *N. iridea* usually occurs at temperatures higher than 5.7°C, which represents a 100-year (ca. 1900–2000) average value for the deep basin water in the study area (SMHI, 2012). Also, according to Seidenkrantz (1993) and Seidenkrantz & Knudsen (1994), *N. iridea* was found in higher abundances during the Eemian (which was warmer than present day interglacial) and Schröder-Adams (1991) reported it as typical Holocene species. Since both, Eemian and Holocene are well-known interglacials, we assume that it is very plausible that *N. iridea* could be a proxy for climate warming during the LIA.

EA: 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 references are used to state that *N. iridea* indicates milder climate (warmer compared to what?).

REPLY: See above.

EA: Is it possible that *N. iridea*'s abundance is dictated more by access to appropriate food than to a slight change in temperature (any literature with estimates - how many degrees change we are talking about at the water depth in focus – or in other areas where a similar trend is recorded?)?

REPLY: We compared the temperature tolerance of this species reported in ecological studies from Northern Europe (Table 4) with a long-term temperature average typical

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for the study area (see above).

EA: *H. balthica* is suggested to be indicative of climate warming (based on what?).

REPLY: *Hyalinea balthica* was previously suggested as an indicator of warming for the parts of this record including Medieval Climate Anomaly & Recent warm period (see in Polovodova et al., 2011). We understand the reviewer's concern since the distribution of *H. balthica* and our conclusions sound somewhat controversial. Thus, the recent distribution in the oceans shows that *H. balthica* is present in higher latitudes and becomes less important in warmer temperate oceans to the south (Murray, 1971; Murray, 1973; Ross, 1984; Murray, 2006), but the decrease southward doesn't have to be related to temperature. In core 9004, however, it clearly attained higher abundances during the warm periods in the 1930s and at ~ AD 1100 (Polovodova et al, 2011) and increased towards the termination of the Little Ice Age. Nagy and Qvale, 1985 also suggested that immigration of *H. balthica* into the Skagerrak at ca. 7800 BP indicated a climate warming. At the same time it remains rather unclear why do we find high abundances of *H. balthica* during the climax of the LIA (1675-1710 A.D.).

EA: It is stated that *A. glomeratum* indicative of a cooler climate. Additional references on *A. glomeratum* include Schafer and Cole 1982; 1986, Kuhnt et al. 2000 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.

REPLY: The suggested references have been included into the discussion.

EA: 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.

REPLY: This has been done and we divided the foraminiferal part of the discussion into two parts 5.1.1) 1350-1650 A.D. and 5.1.2) 1650-1900 A.D.

EA: The present study is based on data from 3 different sediment cores, parts of the

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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.

REPLY: We think it is important to present the whole picture for the reader and therefore we briefly introduce the previously published data to the reader in the results section. Also, since the investigation of this record is still going on, it is important to introduce the changes, which have been made after the first data have been published (e.g. see comments of Christian Hass on the age model).

EA: Have the authors considered how changes in sediment accumulation rates affect the number of individuals/g dry sediment?

REPLY: Probably there is no any larger effect more than to decrease the concentration in the uppermost part of the record where we have higher accumulation rate. Anyway, it would not significantly change the interpretation.

EA: 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.

REPLY: We mention the possibility of changes in food availability and quality of organic matter, when discussing the appearance of agglutinated species after ca. 1600 A.D. (p.12, lines 30-23 – p.13, lines 1-9). In order to explore it further, we added the following text to the discussion: "On the other hand, the first increase of *H. balthica* unit after ca. 1650 A.D. (see F4: Fig. 4) coincides with the offset in C/N ratio values. After 1650 A.D. C/N values become lower than the long-term average, which is the opposite of the values present prior this date. This could imply changes in the quality of organic matter, which could potentially have effect on the species composition. *Hyalinea*

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balthica was found in the Bay of Biscay among faunas dominating areas with relatively low quality organic carbon mainly of terrestrial origin (Hess et al., 2005). According to Sweetman et al. (2009), the large-sized foraminifera (as *H. balthica*) usually display slow response to fresh phytodetritus deposition compared to small and thin-shelled foraminiferal species (as e.g. *N. iridea*). Given that *H. balthica* unit replaces the unit of *N. iridea*, a well-known fresh-phytodetritus-feeder (e.g. Gooday and Hughes, 2002; Duchemin et al., 2007; Alve, 2010; Goineau, 2012) during the climax of the LIA, this faunal shift may reflect reduced primary productivity and consequently dominance of partially degraded organic matter. However, even if both dinoflagellate cysts (Nordberg and Harland, unpublished data) and $\delta^{13}\text{C}$ values (fig. 4) show a simultaneous short decrease at 1650 A.D., it is too small to draw any firm conclusions.

EA: p. 4, lines 23-25: what was the diameter of the cores?

REPLY: The diameters of the piston and the Gemini cores are 7.6 and 8 cm, respectively. This has been added to the methods.

EA: p. 4, line 32: Table 1 and Fig. 2 belong to the results section.

REPLY: This has been corrected.

EA: p. 5, line 8: should core GA113Aa be GA113-2Aa?

REPLY: Yes. It has been corrected.

EA: 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?

REPLY: At the time of the sampling for stable oxygen and carbon analysis we were more confident to find a larger abundance of *C. laevigata* compared to *H. balthica* down-core and unpublished data also indicates that the species is a very good candidate for isotopic analysis with a small vital effect. Also, in contrast to *C. laevigata* which occurs >80 m water depth in the Skagerrak, *H. balthica* was recorded as living at much shallower depths (>20 m) in Kattegat (Nordberg et al., unpubl. data). Since

there could be a transportation of foraminiferal shells from the fjord slopes into the deep basin (Nordberg et al., 2009), $\delta^{18}\text{O}$ measured in *H. balthica* can thus be affected by higher summer temperatures at the shallower depths.

EA: 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).

REPLY: This information has been removed from both figure 5 and text.

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

REPLY: This has been corrected to the following: "The organic carbon samples from the core 9004 were run at Bremen University (Germany) using a Vario EL III CHN analyser and simultaneously were subject to the total nitrogen (TN) measurements."

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

REPLY: No, we did not measure oxygen concentration at the moment of sampling. The information about position of the redox cline has been therefore deleted.

EA: p. 13, line 5: The authors need some more concrete arguments for their dissolution hypothesis – e.g., literature on the stability of CaCO_3 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

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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?

REPLY: We removed the most part concerning dissolution hypothesis from the text and Fig. 5. and added the statement that increased abundances of a fragile *N. iridea* downcore reflect a low to moderate degree of calcareous shell loss in our record.

EA: 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.

REPLY: This has been deleted.

EA: 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.

REPLY: For *H. balthica* see discussion above. As for *A. glomerata*, it has been reported from the areas with temperatures $<4^{\circ}\text{C}$ (see references in the text). As we mentioned it above, the long-term average temperature in Gullmar Fjord is 5.7°C (SMHI, 2012), therefore occurrence of *A. glomerata* may indeed indicate the lower bottom water temperatures (a decrease of ca. 2°C as compared to the long-term average).

EA: p. 14, line 8: When discussing land uses it should be made explicit how these landuses alter the fjord environment.

REPLY: The intensification of land use through cultivation and deforestation would increase erosion of soils in Sweden, which generally have a high erosion risk due to presence of silt and marine clays (Ulén & Jakobsson, 2005). Soil erosion could lead to

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higher sediment transports by local streams and land runoff, as well as increase input of terrestrial organic matter into the fjord.

EA: 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?).

REPLY: We removed this section from the paper (see in response to reviewer #2).

EA: 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?

REPLY: Dates shown in ALL figures result from the present study. We added a sentence about that to the figure captures.

EA: Fig. 4: 1) dates on the right hand side of diagram – is 50BC correct? 2)Where is the explanation of the abbreviations RWP, DA etc? 3) Do F1, F2 etc represent the factor units? 4) If so, this is confusing as the symbols in the legend are the same as for the “Herring periods”. 5) I guess 63mkm should be 63 μm ?

REPLY: 1) The date 50 BC corresponds to the depth of 705 cm, according to the down most ^{14}C measurement (Fig. 2). Actually, in the table 2 this date is shown as 45 ± 225 BC, so we corrected it to 45 BC. 2-3) We explained these abbreviations in the figure capture. 4) We changed the symbols for herring periods from rectangles to hexagons, so they become easier to distinguish and added their description to the figure capture. 5) This has been corrected

EA: Fig. 4: 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)?

REPLY: It is indeed strange that a turbidite is not reflected in the C/N values. We

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considered that and therefore discussed briefly a reliability of C/N record in Gullmar fjord (see section 5.2). As for *C. laevigata* shells, we found that there was a mistake in the graph, so that the $\delta^{13}\text{C}$ curve has got displaced upward. Now as we corrected it, it is seen more clearly that there is a gap in $\delta^{13}\text{C}$ values. Also as it has been published before, there indeed was a dramatic change in faunal composition, when we found that more than 50% of the assemblage was dominated by shallow water *Elphidium excavatum* (see Polovodova et al., 2011: Fig. 4A).

EA: Finally, the authors should consider if figure 4 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.

REPLY: We modified the Fig. 4, so that now it encompasses only the ca. last 1000 yrs. It has been decided to show also the Medieval part of the record since we refer to its foraminiferal assemblages several times in the text.

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Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/9/C7438/2013/bgd-9-C7438-2013-supplement.pdf>

Interactive comment on *Biogeosciences Discuss.*, 9, 14053, 2012.

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