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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 Christian Hass.

We highly appreciate the valuable comments and criticism of both reviewers, which help to significantly improve our manuscript. Below we provide a detailed consideration of each comment and reply to the concerns of the reviewer #2 Christian Hass (further referred as “CH”):

CH: It is indeed difficult to write a paper on a section of a core that has already been published. I would propose to place results of earlier studies into the introduction section or into an extra section before the results section rather than in the "Results" sec-

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tion itself. Otherwise it is difficult to distinguish between new results and those that are already published.

REPLY: The results section has been rewritten (see comments below), the most of previous data has been removed and we refer in it to previous studies instead.

CH: p. 2, line 17: Moberg et al. 2005 don't even mention the LIA but the statement is ok. REPLY: This has been corrected.

CH: p. 3, line 5-6: LIA in same core/resolution was published by Filipsson and Nordberg, 2010. REPLY: This has been corrected to "This paper presents the first well-dated and high-resolved foraminiferal stratigraphy of the Little Ice Age from the sedimentary archives at the Scandinavian coast."

CH: p. 3, line 20: Filipsson and Nordberg 2004 a or b? REPLY: It was Filipsson & Nordberg 2004a. This has been corrected.

CH: p. 3, line 25: Qvale et al., 1984 is in the reference list but in the wrong place. REPLY: Corrected.

CH: p. 3, line 26: Filipsson et al., 2004 is not in the reference list. REPLY: Filipsson et al, 2004 as been added to reference list.

CH: p. 3, lines 26-27: Filipsson and Nordberg 2004a or b? REPLY: It is Filipsson & Nordberg 2004ab. This has been corrected.

CH: p. 4, lines 16-17: Filipsson and Nordberg 2004 a or b? REPLY: Corrected to Filipsson and Nordberg 2004a.

CH: p.5, line 1-9: The age data including the lab numbers have already been published twice (to my knowledge). Thus, they appear in the manuscript without a proper reference.

REPLY: This has been corrected in captures for Figure 2 and Table 1.

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CH: A reservoir correction of 500 years should be a ΔR of 100 and not 0. REPLY: Corrected.

CH: There is no explanation or reference for taking a ΔR of 100 years in this area.

REPLY: The agreement of using 500-yr reservoir age came from the dating of 14 pre-bomb mollusc shells of known calendar age coming from collections of Natural History Museums in Gothenburg and Stockholm and sampled in the deep basin of Gullmar Fjord and NE Skagerrak (Nordberg and Possnert, unpublished data). This is explained in Polovodova et al. (2011).

CH: There are tables showing different core depths for samples with the same Lab ID (e.g. Ua-23004: 645 cm in Polovodova 2011, 743 cm in Filipsson and Nordberg 2010, 705 cm in the present manuscript; the core is supposed to be 731 cm long). This must be explained. I wonder why there is no remark on that as I am certainly not the first person to notice.

REPLY: Generally our depths and depths published in Filipsson and Nordberg (2010) differ by 40 cm (see an explanation in comments below). Polovodova et al (2011) showed original/genuine depths for core 9004. Then, small differences (1-2 cm) between depths in both studies (743/703cm versus 705 cm) result from taking into account the microhabitat preferences of the ^{14}C -dated mollusc species. For instance, *Thyasira flexuosa* is infaunal and therefore seldom found at the sediment surface. That's why it has been decided to use the bottom value of a 2-cm sediment slice for depth designation (563 cm instead of 561-563 cm). However, in order to avoid the confusion and for sake of consistency it has been decided to use in this paper the depths published in Filipsson and Nordberg (2010), but corrected for -40 cm since there is no gap between two cores.

CH: p. 5, line 3: Reimer et al. 2004, and Bronk Ramsey, 2005 are not in the reference list. REPLY: Corrected.

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CH: p. 5, line 5: Polach, 1977 is not in the reference list. REPLY: Corrected.

CH: p. 5, line 9: Filipsson and Nordberg 2004 a or b? REPLY: Corrected to Filipsson and Nordberg 2004a.

CH: p. 5, line 25: Shouldn't that be Na4P2O7? REPLY: Yes, it has been corrected to Na4P2O7.

CH: p. 6, Chronology chapter: It is hard to separate between the data that were already published and the new data. The whole section including Tab. 1 and Fig. 2 is almost identical to the chronology section in Polovodova 2011 which is not even cited. I would propose to move the chronology section out of the results chapter as this is not a result of the actual study.

REPLY: We changed this section to the following: "The records GA113-2Aa & 9004 span from approximately 350 BC to 1999 A.D. (Table 1, Fig. 2), and comprise the Roman Warm Period (RWP), the Viking Age/Medieval Warm Period (MWP), the Little Ice Age (LIA) and the recent warming of the 1900s (Filipsson & Nordberg, 2010). For the chronologies of cores GA113-2Aa and 9004 based on 210Pb (Fig. 2) and 14C AMS datings, see Filipsson and Nordberg (2004a, 2010). Later the age model for core 9004 has been revised by Polovodova et al. (2011) and the updated version is used in the current study (Fig. 2, Table 1). For the core G113-091 the only intact mollusc shell of *Nuculana pernula* yields an age of 1665 ± 85 A.D. (Table 1; Fig. 2), which indicates that this core represents the time interval from 2009 to the middle part of the Little Ice Age."

CH: p. 6, line 24: years A.D. is not a unit for "age". REPLY: Corrected.

CH: p. 6, line 8 and 9: Filipsson and Nordberg 2004 a or b? REPLY: Corrected to Filipsson and Nordberg 2004a.

CH: p. 7, Section 4.2: Corg data have already been published by Filipsson and Nordberg 2010. It is questionable whether this section belongs to the results section.

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REPLY: We removed the short description of the Corg data from the section 4.2 and referred to previous studies instead. However C/N data were not previously published in Filipsson & Nordberg (2010) and therefore are presented in the current research.

CH: p. 7 sect. 4.3.2: Here are again results of earlier studies partly in the same wording and without proper reference. Polovodova et al. 2011 is cited, however the reference is apparently for the turbidite interpretation. Furthermore, the core-depth data given differs significantly from Polovodova 2011. There is no explanation for these differences, which makes it difficult to follow the manuscript when compared to the earlier study.

REPLY: We removed the previously published lithology data and referred to previous studies instead. The information on turbidite and exotic foraminiferal species, though, has been included according to wishes of the reviewer #1. Further (as we added it also to the section 4.4.2), based on a good isotopic correlation with a core G113-091, the depths of the core 9004 had to be corrected for -40 cm, as compared to the depths presented in Filipsson and Nordberg (2010) and Polovodova et al. (2011), which both used “+100 cm” depth correction in order to compensate for a suggested gap between two cores. This literally means that a 60-cm long GA113-2Aa core could be directly placed on top of the core 9004.

CH: p. 7, lines 17-18: Filipsson and Nordberg 2004 a or b? REPLY: Corrected to Filipsson and Nordberg 2004a.

CH: p. 8, line 16: there should be a reference to Fig. 3. REPLY: The reference was added.

CH: p. 8, Section 4.4.1: Results should be generally (this counts for all results) presented on the depth scale rather than on the time scale as the age model may change with more AMS datings added. Although there are significant changes on the depth scale (which must be addressed earlier in the manuscript) I would propose to present depth-related data in this section (text) of the manuscript. If necessary you might give

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additional age information in brackets as partly done in Chapter 4.5.2.

REPLY: We changed the presentation of the results to depth-related data and gave additional age information in brackets.

CH: Chapter 4.5: same problem with age data. Better present depth values. REPLY: See above.

CH: p.10, line 5-6 There is one benthic foram species that is neither agglutinating nor calcareous? REPLY: We corrected this.

CH: p. 11, line 12-24: *N. iridea* has been described from a number of cold environments (Arctic and Antarctic) so decreasing temperatures should not be the problem for this species. However, dissolution as stated might be the reason. Are there more references on the ecology of this species?

REPLY: Yes, but was it present there as one of the dominant species? See our response to comments by Elisabeth Alve.

CH: A short-term warming around 1600-1650 AD is visible in a number of studies, even in my own from Antarctica (Hass et al. 2010). Cage and Austin's fjord might not be such a good reference as the record of bottom-water temperatures is less clear than other records: the 1600 AD warm episode is clearly visible, however, the MWP, and the LIA are not.

REPLY: We refer to Cage and Austin's study originally because it is situated within the same climate system as the Gullmar Fjord. However, we included the suggested reference into introduction.

CH: p. 12, Chapter 5.1.2 *Adercotryma glomerata* is described as a species that occurs during sunspot minima and that could generally stand as a proxy for climate cooling (and associated oxygenated bottom waters). Indeed, *A. glomerata*, seems to have a preference for cool water environments but in the case of Gullmar Fjord it occurs during the Medieval Warm Period as well (with roughly the same abundances) (Polovodova,

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2011). This should be mentioned and discussed, shouldn't it?

REPLY: Though, the percentages are indeed roughly the same during both periods, in the MWP *Adercotryma* occurs in lower concentrations (up to 30 ind./g) as compared to the LIA (up to 70 ind./g). Also in core 9004 A. *glomerara* increases for the first time during the Dark Ages period (around 400 A.D.), which is known to be characterised by climate deterioration. (we included this in the text).

CH: p. 13, line 8: Murray and Alve, 1999 is not in the reference list.

REPLY: The reference is: Murray, J.W., Alve, E.: Taphonomic experiments on marginal marine foraminiferal assemblages: how much ecological information is preserved? *Palaeogeography, Palaeoclimatology, Palaeoecology* 149, 183-197, 1999. We added it to the list.

CH: p. 13, lines 28: In fact Core 15535-1 in Hass 1997 shows the increased abundance of *H. balthica* during the LIA maximum but the other three cores of that study don't show that. Erbs-Hansen's core also shows a *H. balthica* increase during the LIA maximum. But all the cores (with the exception of I KAL in Hass, 1997) reveal a general decrease of *H. balthica* after the LIA.

REPLY: We agree with the reviewer's point and included this information in a subsection of 5.1.2. However, it still remains unclear why, in contrast to other Skagerrak studies, in our record *H. balthica* increases towards LIA termination and peaks during the warming of the 1930s? Several studies found it associated with the "warmer" climate (Nørvang, 1945: *H. balthica* as boreal-lusitanian form; Feyling-Hanssen, 1980: as Eemian species; Nagy and Qvale, 1985: as immigrant to the Skagerrak after opening of the English Channel at ca. 7800 BP). What if other factors (e.g. food) controlling *Hyalinea*'s distribution in Gullmar Fjord were more important in the LIA? To investigate this further, we added a discussion on possible feeding preferences of *H. balthica* and changes in quality/quantity of organic matter, according to comments of Elisabeth Alve (see response to reviewer #1).

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CH: p.13 lines 18-21: How can there be "high numbers of well preserved and shiny calcareous specimens and thin-shelled species" in a corrosive environment? If this is really so I fear the hypothesis of enriched agglutinates due to dissolution of calcareous taxa should be dropped.

REPLY: We dropped this hypothesis, see response to reviewer #1.

CH: p.14, lines 2-7: *H. balthica* peaks during the less stormy but probably colder phases of the LIA. Why should this species indicate warming? Most of the evidence is against this assumption. Even the data by Leijonhufvud et al. 2010 referred to in the paper indicate that the *H. balthica* peaks occur during colder phases (the Stockholm climate might be slightly different anyway).

REPLY: See above.

CH: p.15, chapter 5.3: It is indeed interesting to find out where the (rather small) amount of sand-sized material comes from. Although the cause for the deposition of sandy materials might be similar, the conditions in Gullmar Fjord are not comparable to those in the open Skagerrak. The open Skagerrak circulation is a large-scale circulation coupled to the driving winds and to conditions in the adjacent seas (North Sea, North Atlantic, Baltic Sea). To find out about the transport mechanism and to gain information about the processes involved in Gullmar Fjord an analysis of the complete (carbonatefree) grain-size spectrum is necessary. Such data would certainly be a benefit for the manuscript. Otherwise there is too much room for speculation. Fig. 6 is hardly suitable to shed sufficient light on that problem. Background oceanographic (i.e. current speed) information of Gullmar Fjord is needed to use the data shown in Fig. 6. At least the figure should be equipped with data on the wind direction. The sea-ice explanation appears to be an interesting issue. Grain-size statistical parameters should be able to provide information on this.

REPLY: We removed this section from the text.

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CH: p. 15, line 25: Alexandersson et al., 1998 is not in the reference list.

REPLY: The reference is: Alexandersson, H., Schmith, T., Iden, K., Tuomenvirta, H.: Long-term variations of the storm climate over NW Europe. The Global Atmosphere and Ocean System 6, 97–120, 1998. It has been added to the paper.

CH: p. 16, lines 17-19: The authors should decide to re-word the description of the first phase of the LIA (also in the abstract and possibly in other places of the manuscript). Otherwise it might be understood that this first phase was stormy and milder (!) in the sense that it was milder than before. Since the Medieval Warm Period was before a massive warming must have taken place which is certainly not what the authors want to express.

REPLY: This has been corrected to: " 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*."

CH: p.16, lines 28-30: I see no reason to interpret climate warming on the basis of *H. balthica* occurrences. By the way: you interpret that the climax of the LIA (1675-1704) and a "general climate warming" (1600-1743) happened at the same time. Indeed, a (cold) climax can only be followed by a warming and both intervals given are during the recovery from the LIA maximum. Maybe *H. balthica* is reacting sensitively to this but at the same time other ecologic factors such as food supply and oxygen conditions might be the stronger processes.

REPLY: See above.

CH: The references are not always in alphabetical order, therefore some of the missing references might be hiding somewhere in the list.

REPLY: This has been corrected.

CH: Sometimes the last author is separated with "and" but mostly not. REPLY: Corrected.

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CH: Some formatting for consistency is necessary (e.g. abbreviations, volume numbers, issue numbers yes or no). REPLY: Corrected.

CH: p. 20, line 22: Powell is not editor of this volume. REPLY: Corrected.

CH: p. 24, line 20: reference is fragmentary. REPLY: Corrected.

CH: p. 27, line 11: In the reference cited (Polovodova, 2011) the mentioned gap is 40 cm deeper in the core. It is important to explain that (as mentioned above). REPLY: See in reply above.

CH: p. 27, line 15: With "Recent Warming" (in title case) most people would understand the period from 1985 AD. Maybe the authors should consider to give the period from 1900 AD a different name.

REPLY: We referred previously to the Recent warm period in Polovodova et al. (2011; section 4.2.2, p. 100) and indicated that by that we meant the 20th century (1900s). This has been corrected in the text.

CH: p. 28, lines 7-8: Are there reasons for the capitalization of the storms? REPLY: Their original swedish names are capitalized, but we corrected it in the text.

CH: p. 28, Line 3: Alexandersson et al., 1998 is not in reference list. REPLY: Corrected.

CH: Tab. 1: I would propose to either show the reservoir-corrected or the uncorrected data. To show both is not necessary. If you show the uncorrected data you should mention the reservoir applied in the caption.

REPLY: Corrected.

CH: Ua-35969 has a mistake in the error (25 or 55 years?).

REPLY: Yes, there was a mistake (55 yrs), now it is corrected to 25 yrs.

CH: I take it that the last column shows the usual BP years. Then 1950 minus BP years should give the AD years. If so, the AD years given in the second last column are 50

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years too high. Did I overlook something important here?

REPLY: There was a mistake and we corrected it.

CH: The font size of Ua-39016 is bigger than that of the other lines and "AD" in the second last column for this sample is not necessary.

REPLY: Corrected.

CH: Tab.2 : What means "raw foraminiferal data"?

REPLY: It is corrected to "foraminiferal counting data".

CH: Fig. 2: Dating Ua24043 is not in the figure. What is the reason for this?

REPLY: We removed it from the figure 2, because it clearly represented an outlier. Now however, we included it to the figure, so there is no confusion about that.

CH: The y-axis lable should be "Core depth (cm)" as in figs. 3 and 4. REPLY: Corrected.

CH: Fig.3: Should there be a reference to Filipsson & Nordberg 2010 for the $\delta_{13}C$ and sand-content data? There are decimal commas that should be replaced by decimal points ($\delta_{13}C$ axis).

REPLY: Both remarks have been considered and corrected.

CH: Fig. 4: There should be a lable on the y-axis: Core depth (cm). REPLY: This has been corrected.

CH: Fig. 5: >63 mkm should be $>63 \mu m$, there are decimal commas on the Corg and $\delta_{13}C$ axes. REPLY: This has been corrected.

CH: Shown are the factor loadings rather than the factor scores. Corg units should be given as Corg (%) like for the other parameters.

REPLY: Both have been corrected.

CH: The y-axis lable should be "Core depth (cm)" as in figs. 3 and 4. The x-axis labels

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should be made consistent (sand content, org linings, shell loss).

REPLY: This has been corrected.

CH: The Fisher _-index should be reflecting the species number. Thus the species number is not necessary here. REPLY: See in response to reviewer #1.

References

Feyling-Hanssen, R.W.: Foraminiferal indication of Eemian interglacial in the northern North Sea. Bull. Geol. Soc. Den. 29, 175-189, 1980.

Nagy, J., Qvale, G., 1985. Benthic foraminifers in upper Quaternary Skagerrak deposits. Norsk Geologisk Tidsskrift 65, 107–113.

Nørvang, A: The zoology of Iceland. Foraminifera 2 (2), 1-79. Munksgaard, Copenhagen and Reykjavik, 1945

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