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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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The manuscript by Polovodova Astemann et al. entitled "The Little Ice Age: evidence from a sediment record in Gullmar Fjord, Swedish west coast" is on the reconstruction of the Little Ice Age (LIA) based on three sediment cores from the deep Gullmar Fjord (Sweden). Parameters investigated include benthic foraminifers (focus), organic carbon data, stable carbon isotopes and coarse fraction data. At least one of the cores has a solid age model based on AMS radiocarbon and radiolead data. The authors present a two-phase LIA based on micropaleontologic evidence. The first phase being "milder" and stormy until AD 1650 and the second phase being colder. Around 1600

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AD the authors identify a short episode of warmer climate.

The authors present most interesting and novel aspects on short-term climate variations of the recent past (here the LIA) and the response of the Gullmar Fjord. The data, interpretations and conclusions shown are relevant on a regional as well as on a broader scale.

The study is related to another study (Polodovoda 2011) in which the authors focus on the Medieval Warm Period using basically the same sediment sequence. Thus, part of the data is already published. Through the past years corrections on the depth scale of Core 9004 have been carried out, which are not mentioned/explained in the manuscript. This poses problems because comparison with results of earlier papers on the same sediment core (9004) becomes very difficult (see below).

Some of the interpretations in this manuscript seem to be contradictory (e.g. p.1, lines 25-29: climate deterioration during the coldest phase of the LIA [meaning it is getting even colder] and still there is a warming trend that covers the entire LIA climax).

The focus of the manuscript matches the focus of Biogeosciences very well.

The manuscript is well organized and well written but some language/grammar correction is necessary in places. All figures and tables are necessary and well designed; all references are necessary and the reference list is up to date. The title reflects the contents of the manuscript and the abstract provides a concise summary.

“Moderate revisions” can make this manuscript recommendable for publication in Biogeosciences. If the problems with the age model cannot be adequately solved the revisions to be carried out will be “major”.

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" section itself. Otherwise it is difficult to distinguish between new results and those that are

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already published.

p. 2, line 17: Moberg et al. 2005 don't even mention the LIA but the statement is ok.

p. 3, line 5-6: LIA in same core/resolution was published by Filipsson and Nordberg 2010.

p. 3, line 20: Filipsson and Nordberg 2004 a or b?

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

p. 3, line 26: Filipsson et al., 2004 is not in the reference list.

p. 3, lines 26-27: Filipsson and Nordberg 2004 a or b?

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

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. A reservoir correction of 500 years should be a ΔR of 100 and not 0. There is no explanation or reference for taking a ΔR of 100 years in this area. There are tables showing different core depths for samples with the same Lab ID (e.g. Ua-23004: 645 cm in Polodova 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. See also comments to Table 1 (below) regarding the AD conversion.

If you have used the CALIB software you should refer to it in this section.

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

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

p. 5, line 9: Filipsson and Nordberg 2004 a or b?

p. 5, line 25: Shouldn't that be Na₄P₂O₇?

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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.

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

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

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.

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 differ 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.

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

p. 8, line 16: there should be a reference to Fig. 3.

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

Chapter 4.5: same problem with age data. Better present depth values.

p.10, line 5-6 There is one benthic foram species that is neither agglutinating nor calcareous?

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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?

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.

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

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

p. 13, lines 28 ff: 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.

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.

p.14, lines 2-7: *H. balthica* peaks during the less stormy but probably colder phases of

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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).

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 (carbonate-free) 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.

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

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.

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)

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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.

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

Sometimes the last author is separated with "and" but mostly not.

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

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

p. 24, line 20: reference is fragmentary.

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).

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.

p. 28, lines 7-8: Are there reasons for the capitalization of the storms?

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

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. Ua-35969 has a mistake in the error (25 or 55 years?). 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 years too high. Did I overlook something important here?

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.

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

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

The y-axis label should be "Core depth (cm)" as in figs. 3 and 4.

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

Fig. 4: There should be a label on the y-axis: Core depth (cm).

Fig. 5: >63 mkm should be $>63 \mu\text{m}$, there are decimal commas on the Corg and $\delta^{13}\text{C}$ axes. Shown are the factor loadings rather than the factor scores. Corg units should be given as Corg (%) like for the other parameters. The y-axis label should be "Core depth (cm)" as in figs. 3 and 4. The x-axis labels should be made consistent (sand content, org linings, shell loss). The Fisher α -index should be reflecting the species number. Thus the species number is not necessary here.

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