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Interactive comment on "Atlantic Water advection vs glacier dynamics in northern Spitsbergen since early deglaciation" *by* Martin Bartels et al.

Anonymous Referee #1

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Review of manuscript cp-2017-53 by Bartels et al. "Atlantic Water advection vs glacier dynamics in northern Spitsbergen since early deglaciation" submitted to Climate of the Past.

The manuscript deals with a marine core record from the mouth of Woodfjorden, northern Svalbard margin from 171 m water depth and 275 cm long. The core comprise the time intervals c. 15.5-7.8 ka and c. 1.8-0.4 ka. The proxies are benthic foraminiferal fauna analysis, stable isotopes, grain size, IRD analysis, alkenone surface temperature and sea ice proxy IP25 and brassicasterol (PIP25 index). The purpose is to study the inflow of Atlantic Water and its link to the behaviour of local glaciers during different climatic phases.

The manuscript is overall well-written and well-structured and of potential interest.

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However, it has also some issues, which must be considered before publication. The major issues are that this is a very local study that mainly compares to other local records from the Svalbard margin. Also, changes in reservoir age through time is not dealt with or discussed and in addition are resulting in erroneous correlation to the ice core records. Furthermore, the results rely to a great deal on the ecology of benthic foraminiferal species, which is not discussed in any detail with just a few references and without revealing any details about species from these references. Below I outline these and other concerns in more detail:

The present ecology of foraminifera is very sporadically discussed or described and with only few references. Actually, studies such as by Hald and Korsun, 1997 and Korsun and Hald, 2000 of the distribution of living benthic foraminifera in Svalbard fjords have characterized species in terms of 'ice-proximal', 'ice-distal' (which are useful terms for the present study from a fjord affected by glaciers) and also related them to influx of AW and meltwater to the fjords. There are references to foraminiferal studies from Iceland – I would think living foraminifera in Svalbard fjords today and from East Greenland and the Arctic Ocean would be more relevant (e.g. Ahrens et al., 1997; Newton and Rowe, 1995; Jennings refs from the Greenland margin; Wollenburg refs from north of Svalbard). Often the ecology of the foraminifera is presented as cf. (ref) (e.g. p.14, line 18, p. 15, line 10, p. 17, line 12). I would like to know what these references contain – not having to read the whole publication to find out what is meant here. There is in general too many 'cf.'s' in this manus – one or two is acceptable, but not more. It has to be written out clearly what exactly in all these published papers that is referred to here.

'Abstract': line 6: "spanning the last c. 15,000 years". Your record has a six thousand-year gap - this should be made clear from the start. Also, please mention the core name, length and water depth in the abstract.

'Introduction': p. 4, lines 8-11. This is a bit of over statement: the records you refer to are also multiproxy comprising planktic and benthic stable isotopes and planktic and

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benthic foraminiferal faunas, diatoms, various grain sizes and IRD and are most often also of much higher resolution than your record. Better to state that your record is in a more ice proximal setting than the others, and that you can add low-resolution biomarkers (with one or two data points per 1000 years) and a different method for IRD-analysis in fairly high resolution.

p. 5, line 11: give the time interval range between the 5 cm samples and the biomarker samples (also distance between the latter) and the range of time within the 1-cm samples.

Chapter 3.1 'Computer tomography': must be written clearer: what is the resolution of counts of the IRD > c. 1 mm and the volume %? I am also unfamiliar with this method and suggest that a simple count of IRD is performed and shown. This would not be a great effort as there are sieved residues from foraminiferal samples and I suggest IRD > 1mm and also 0.5-1 mm be counted for comparison with the computer analyses and for the sake of comparison with published IRD counts.

p. 6. 'Grain size measurements'. Why are sortable silt data not presented and discussed? The manuscript mainly discusses the silt size fraction in terms of ice rafting from sea ice and/or supplied from glaciers. This size fraction can come from glaciers and sea ice yes – but can be current sorted as well. To me the coarsening from the Holocene onwards is because of current sorting – bottom currents are only mentioned on p. 17, line 4. I cannot see why presence of coarser silt should indicate sea-ice rafting, when definitely there is bottom current activity as indicated by certain benthic foraminiferal species. This may also explain the low sea-ice biomarker concentration mentioned on p. 16, lines 32-35. The maxima in 20 my could be more due to current sorting, than ice rafting in my opinion.

p. 6, lines 36-37: "several specimens show signs of dissolution or transport" – any particular interval or throughout the record? "Small fragments were not counted since these specimens might be allochtonous" – still, it would be good to see the % fragmen-

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tation data and how they compare to species distribution patterns

p. 7, line 20: "the endobenthic foraminifera Nonionellina labradorica" – there is no documentation at all in this manuscript about the life mode of N. labradorica – is it shallow infaunal or deep infaunal? What kind of food does it require? Fresh or partly degraded, refractory? Or? Any vital effects on 13C? Later the differences in 13C values between C. lobatulus (epibenthic, ref.?) and N. labradorica are used as an indication of export productivity. You have a low-resolution brassicasterol flux record (that do not match the benthic foraminiferal AR) – how does your 'export production delta13C' record compare to the brassicasterol flux and benthic AR? (they are not shown in the same figure). N. labradorica is a deep infaunal species, and changes in 13C may well be that the species moves up or down in the sediment in search for food, more than a direct signal of the amount of food.

p. 9, line 18. I believe is it not necessary to write 'BP' or 'cal.' I suggest to make a statement in this section (4.2) that all mentioned ages hereafter are in calendar years.

p. 10, lines 20-22: Why not show I. norcrossi and S. loeblich in Fig. 3? I. norcrossi could be added to I. helenae (since most other studies referred to have done that). S. loeblichi tend to have fairly confined peaks that may aid in stratigraphic correlation (see comment below on Younger Dryas). Also, I see in the species list in the appendix that there are many Elphidium species (apart from E. clavatum) – if added together they may point out events of lower salinity/freshwater input (see Polyak et al., 2002). I would also like to see the % agglutinated species. Rarer species could be plotted in an appendix figure or supplement.

p. 10, line 29: "(dominant) species generally follow the pattern of the total benthic foraminifera flux". Yes – evidently – better to show total benthic accumulation rate (AR) (the word 'flux' is for something raining down from above) and remove the accumulation rate for individual species in the figure – it does not really mean anything, because accumulation rates only relate to productivity, while species percentages relate to a

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high number of other ecological factors. Show only total AR along the species percentage distribution - also because the values can differ by several orders of magnitude between different species so that they are not really comparable. Finally, the accumulation rates depends on the age model, which may not be accurate. . ..(and please plot the total AR as a line).

p. 14, lines 32-36: I believe this has been written before concerning a record from Isfjorden in a publication by Rasmussen et al. 2012? 2013? with the same references. Minimum refer to this publication.

Section 5.2 Younger Dryas: The main question here is: how much of this interval belongs to the Younger Dryas stadial – or is it the Younger Dryas at all? This study has used a standard modern reservoir age correction deltaR of 98+/-37 years. However, it is well known that reservoir ages have varied through time – and the Younger Dryas interval is one of the best investigated periods for changes in reservoir age. DeltaR ages of 300 years have been recorded off UK (several Austin et al. refs), 800-1000 years for the central Atlantic (Waelbroeck et al., 2001), 150-200 years in coastal western Norway (Bondevik et al., 2006) – see also Butzin et al., 2005. More importantly, Hanslik et al., 2010 performed an exercise experimenting with different reservoir age scenarios for the Younger Dryas interval in a record from the Arctic Ocean and settled for 1000 years. With a higher deltaR the date calibrated to 11.9 ka is probably of Holocene age, the age of 12.6 ka may also belong to the Holocene – and the age of 13.2 ka may actually fall within the YD interval. If so, the rise in N. labradorica will occur in the Holocene, which makes more sense, because it signifies retreat of the polar front and less sea ice cover. The following decrease in percent of this species could then be the Pre-Boral Oscillation. Your data just below the suggested YD-bar indicate low productivity and a maximum in sea-ice cover – could this be YD instead? What other benthic foraminiferal speices are present? I pointed out in the beginning that the discussion is very local and only compares and discusses shelf records around Svalbard. This is partly acceptable for the Bølling-Allerød and Holocene intervals, which have been discussed and com-

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pared to Nordic seas and Atlantic records on many occasions (but do take a look at Wollenburg papers from the Arctic Ocean). But for the YD, I will request an in-depth comparison to 'outside records' and discussion – records from the Arctic Ocean, Nordic seas and Atlantic Ocean. Further south the YD is easily defined by presence of tephra or other means (see e.g. Austin and Hibbert, 2012, where in addition to tephra, there are 0% N. pachyderma before and after the YD, and 100% in the YD interval). I especially request that the Hanslik-paper and some of Wollenburgs papers are discussed and compared to. This means that the Younger Dryas in core GeoB10817-4 should first and foremost be defined and identified with certainty (as much as possible). This can be done by looking for patterns in the distribution of benthic foraminifera (this is why I asked above to plot more species as they may better indicate the location of YD), but also concentration and accumulation rate data, IRD and grain sizes, isotopes etc. I believe a detailed and thorough discussion of the Younger Dryas interval can lift this manus above being merely a local study comparing mainly with other local studies.

Given the above consideration about reservoir age changes – Figure 7 show some records from the GeoB10817-4 core plotted along the NGRIP ice core. Since your record is not corrected for true reservoir age changes the apparent synchroneity with the ice core is false – marine calibrated 14C ages and ice core years from layer counting are not expected to match (except maybe for parts of the Holocene and depending on the marine core location). As HS1 is known for the same reservoir age problems as the YD (actually even worse, see refs above) the correlation to the ice core here may also be obsolete.

Minor points:

p. 4, line 18: 'West' Spitsbergen Current p. 5, line 28: 'Additionally,' p. 6, line 34 and line 35: "at" change to 'to' p. 7, line 22: 'ice volume effect' p. 9, line 36: missing 'the' after "abundant" p. 10, line 28: "flux rate" – flux is a rate – use accumulation rate for benthic foraminifera p. 10, line 32: 'ice volume effect' p. 10, line 33: "lighter" – use the term 'lower' – values cannot be 'light' (or 'depleted' (e.g., p. 14, line 36)) p. 11,

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line 36: add ‘interstadial’ after Bølling p. 13, line 16: ‘shelf’ p. 14, line 11: missing ‘the’ after “from” From p. 14 onwards: some awkward wordings here: “synchronously,”, ”benefitted”, “unveil”, “a fading”, “risen”, “go along”, “diminished down”, please edit. p. 16, line 19: ‘slope’, not “shelf edge” p. 17, line 37: delete “the” after “into” p. 18, line 23: “on” – I think should be ‘onwards’

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