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Reply to the Interactive comment on "Regional seesaw between North Atlantic and Nordic Seas during the last glacial abrupt climate events" by Mélanie Wary et al., by Anonymous Referee #2, Received and published: 22 March 2017

RC2: Wary et al present an interesting compilation of (and new) sea surface temperature, salinity, sea ice cover reconstructions from the Norwegian Sea and northern North Atlantic based on dinocyst analyses for Marine Isotope Stage 3 as well as an ensemble of freshwater hosing experiments run under preindustrial boundary conditions. The paper is well written and data are very well presented and adds to the debate about the stadial/interstadial evolution of the Nordic Seas circulation during the last glacial and its role in the abrupt climate change. However, the paper needs moderate/major revisions before it could be accepted for publication.

Reply: We want to thank Anonymous Referee #2 for his/her careful and constructive review of our paper. We will take into account all his/her precious advice for the revision of the manuscript. Below are our replies to his/her comments.

1- First of all the authors need to elaborate on how summer SST up to 14°C in the Norwegian Sea during stadials compare with other previous reconstructions. In this regard, the following points need further discussion:
- The authors stated 'Furthermore, the few direct but qualitative sea-ice reconstructions based on lipid biomarker analyses (Müller and Stein, 2014; Hoff et al., 2016) yielded contrasting results'. Looking at Figure 4 in Hoff et al 2016, it does not seem that those two studies are at odd. In contrary, the sea ice cover records in Müller and Stein, 2014 and Hoff et al., 2016 seem for me to correlate well.

We totally agree, this figure (enclosed below) clearly shows the same results, i.e. low PIP25 values during Heinrich event 1 (and likely Heinrich event 2 in-between GI 3 and 2 – "IS3" and "IS2" in this figure). However, the interpretations of authors significantly differ:

- Müller and Stein (2014) interpreted the sharp decrease down to very low PIP25 values at the beginning of HE1 as the "**sudden break-up of the ice cover and the concentrated release of high amounts of sea ice and icebergs trapped therein**" (page 453, Section 6).
- In Hoff et al. (2016), as Heinrich event 1 is "characterized by IP25 and brassicasterol (as well as dinosterol) values of close to or zero", "PBIP25 and PDIP25 values of 1 were assumed" (page 4, legend Figure 4) and interpreted by the authors as "**a very prominent period of perennial or near-perennial sea ice cover**" (page 6).

The use of the word "results" was for sure inappropriate, and we will replace it by "interpretations".

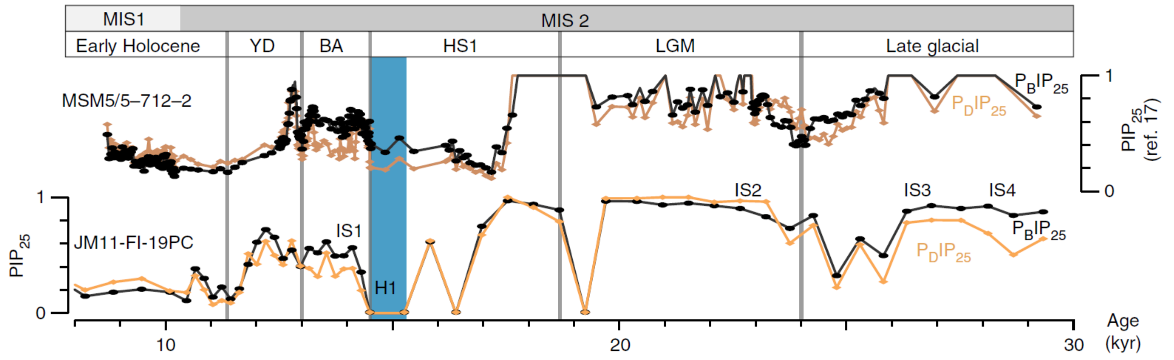

Figure 4 | Sea ice proxies for the time interval between 30 and 8 kyr ago. Comparison of P_BIP_{25} and P_DIP_{25} signals of sediment core JM11-FI-19PC (GICC05-timescale) and sediment core MSM5/5-712-2 (calibrated age scale) from the Svalbard margin published by Müller and Stein[17]. For H-event 1 (blue bar), characterized by IP_{25} and brassicasterol (as well as dinosterol) values of close to or zero (that is, 'zero divided by zero'), per definition maximum P_BIP_{25} and P_DIP_{25} values of 1 were assumed (for background see refs 15,26). For core locations, see Fig. 1. BA, Bølling and Allerød interstadial; H, H-event; HS, Heinrich stadial; LGM, Last Glacial maximum; YD, Younger Dryas.

Figure 4 from Hoff et al. (2016)

I think it is critical to discuss why sea ice cover reconstructions in the southern Norwegian Sea in this study (dinocyst-based) and in Hoff et al., 2016 (lipid biomarker- based) significantly differ. I suggest you plot IP25, brassicasterol- and dinosterol concentration (not the PBIP25 and PDIP25 indexes) with your dinocyst-based data and see if you can reconcile between them or at least make the apparent disagreement between the two results clear, so future investigations may take it further.

We also think it is critical to understand why our dinocyst-based sea-ice reconstructions and Hoff et al. (2016) biomarker-based sea-ice reconstructions significantly differ. But to do that properly, we think it is first needed to clarify why the same biomarker-derived signals lead to opposite interpretations (Hoff et al., 2016 *versus* Müller and Stein, 2014), and why the same biomarker proxy (IP25 abundances) can lead to opposite results in the same studied area (Hoff et al., 2016 on core JM11-FI-19PC *versus* Sicre, unpublished data on core MD99-2285, see figure p.194 of Wary, 2015: www.theses.fr/2015BORD0316/document). Unfortunately, we are not allowed to provide you with the plot of these MD99-2285 IP25 data along with Hoff et al. (2016)'s data, and we are truly sorry about that. But if you look at Figure 4b on p.194 of Wary (2015) and compare with Hoff et al. (2016)'s IP25 abundances (in µg/g of dry sed. for more coherency) you can see that both signals exhibit similar trends on the 39-36 ka BP section (i.e., higher IP25 abundances during GI8 and at the end of HS4, and lower ones during GS8), but rather different ones on the 41-39 ka BP interval (i.e. for core MD99-2285 (JM11-FI-19PC), lower (higher) IP25 abundances during GS10 (i.e. the "H4" IP25 peak in Hoff et al., 2016), higher (lower) IP25 abundances during GI9, and zero (low/moderate) IP25 abundances during HE4 interval as defined on the basis of our IRD data). In core MD99-2285, IP25 measurements were realized at very high temporal resolution (54 years on average) and they reveal a very noisy signal (but yet with clear trends), so the difference might maybe (or maybe not) come from this given the lower resolution in Hoff et al. (2016). But, whatever the reason(s) might be, we think that we are not the most appropriate co-author team to discuss that (no biomarker people among us), that our paper is not the most appropriate to discuss that (no biomarker data), and thus that it would be quite improper to discuss such discrepancies in our paper.

- The authors may need to explain why the %subpolar planktic foraminifera (e.g., T. quinqueloba and G. bulloides) did not increase during stadials if summer SST was that high in the Norwegian Sea. I think the conditions at the average calcification depth of N. pachyderma may be best recorded in the isotopic and elemental composition of its shells, whereas the %

N. pachyderma is also controlled by the abundance of other planktic species. For example, Mg/Ca in N. pachyderma shows different pattern from % N. pachyderma for Heinrich Stadial 1, also in the southern Norwegian Sea (Ezat et al., 2016).

We suggest that the % subpolar planktonic foraminifera did not increase during GS because: (i) in the surface layer, SSS were apparently too low (summer SSS_{dino} between 30.9 and 31.6 on average) according to these species tolerances (e.g., Tolderlund and Bé 1971), and (ii) in the subsurface layer, temperatures were too low (below 5-6°C according to *N. pachyderma* s. optimal temperature range – e.g., Tolderlund and Bé 1971) according to these species tolerances, but these subsurface temperatures could have varied between 1°C and 5-6°C as shown in Ezat et al. (2016) and also suggested in Wary et al. (2016), in both cases for stadial intervals characterized by 100% NPS.

- Notably, the reconstructed summer temperatures during glacial stadials in the southern Norwegian Sea in this study are similar to or even higher than modern temperatures. It is not plausible that we ignore an observation just because it does not fit with what we may expect.

Modern SST values are indicated on Figure S4. We did not indicate them on Figure 2, because the SST range is too low to do so. But we can add these modern SST values on Figure S5 where annual, summer and winter SST are presented, and we can also mention it in the text as we already did in our previous papers.

Furthermore, comparable warm anomalies (as high as modern temperatures) were obtained (also on the basis of dinocyst reconstructions – but not exclusively – and of a multicore compilation) during the last glacial maximum (see de Vernal et al., 2006 for instance), suggesting an atypical and a much more complex functioning of the GIN seas at that time, quite different from the existing modern polar schemes we have in mind.

However, more discussion needed regards the temperature at the source of these water, were the stadial temperatures at lower latitudes higher than modern? In addition, the inflowing water may have had to mix with more cold polar water than in the modern case in its way to the Nordic seas.

According to our simulations, the initial freshwater flux induces a mid-latitude subsurface warming due to the meltwater lid, and the water mass transport is increased along the eastern North Atlantic boundary, potentially through the Continental Slope Current (flowing poleward along the European margin) as suggested in Wary et al. (2016). The source of this water is quite enigmatic, even if we consider that it is advected through this near-surface current whose origin is still debated (see Section 2 in Wary et al., 2016). Nonetheless, Peck et al. (2008) reported unexpectedly warm SST (sometimes warmer than modern ones) during Heinrich stadials at the Porcupine Seabight (~51°N) on the basis of Mg/Ca measurements on *G. bulloides* (see their Figure 4, enclosed below). They proposed several hypotheses to explain these too warm SST during Heinrich events, one of them being transient advection of subtropical surface waters through the Continental Slope Current.

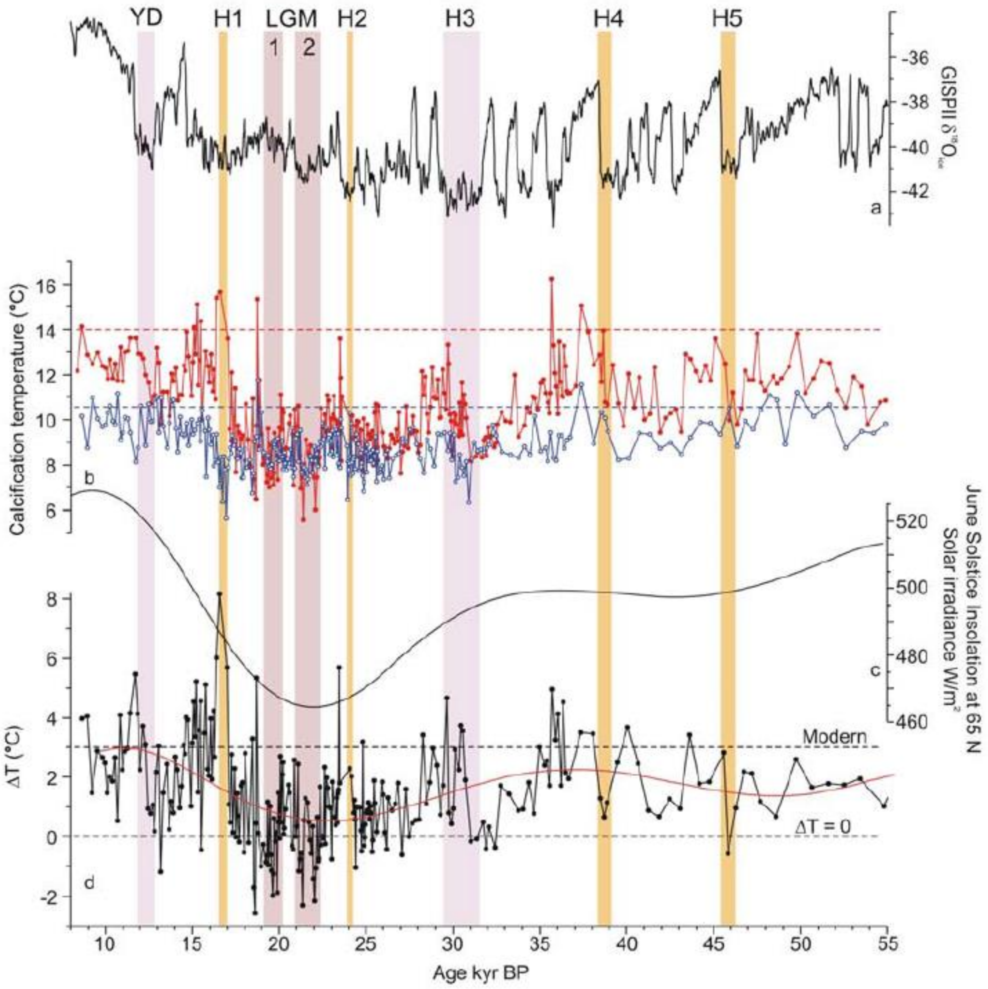

Figure 4. Temperature estimates and seasonality of the upper water column at MD01-2461 over the last 55 ka. (a) GISPII δ^{18}O ice core record. (b) Summer SST inferred from Mg/Ca$_{G.\ bulloides}$ (red solid circles) following *Peck et al.* [2006]. Modern summer SST shown by red dashed line. Mean annual subsurface temperature inferred from Mg/Ca$_{N.\ pachyderma\ sinistral}$ (blue open circles) following *Peck et al.* [2006]. Modern mean annual water temperature at 150 m shown by blue dashed line. (c) June solstice solar insolation at 65°N (La90) [*Laskar*, 1990]. (d) ΔT (summer subsurface temperature (SST), °C). A 6th-order polynomial fit is shown in red. Modern ΔT and ΔT = 0 shown with dashed lines.

Figure 4 from Peck et al. (2008)

2- It is important to clearly clarify in the methods (in section 2.2) what new data have been generated in this study and what have been used from previous studies. I think most of the dinocyst analyses from cores MD95-2009, MD95-2010 and MD99-2285 are already published, or? I may have just missed the referring to previous studies, so I hope this comment does annoy the authors if that is the case.

MD99-2281, MD95-2009 and MD95-2010 dinocyst assemblages are already published, as well as for core MD99-2285 on the 41-35 ka BP interval. For the present study, the published dinocyst counts of those cores and the new data of core MD99-2285 were used to generate new SST, SSS and SIC estimates thanks to the extended modern database used for the transfer function. We will move these information from the Supporting Information section to Section 2.2.

Related to this, I would suggest removing the word 'Surprisingly' from the following sentence "Surprisingly, the three Norwegian Sea cores record higher SST and shorter SIC durations during the cold North Atlantic GS, and lower SST and longer SIC durations during the warm North Atlantic GI.” This is not a surprise as previous dinocyst- based studies have already showed this stadial/interstadial SST pattern in the Norwegian Sea (e.g., Eynaud et al., 2002; Wary et al., 2016).

Our formulation is probably awkward, we do not mean that the results are surprising in the sense that they are new, but that the scheme described here at a regional scale is surprising because different from the accepted scheme. We will therefore reformulate our sentence.

3- Line 139-144: Weakening of the subpolar gyre has been employed to explain relative warming in the eastern Nordic Seas under interglacial conditions for example during late Eemian (e.g., Born et al., 2011). However, a key difference here (in addition to many others), is the likely significant suppression of deep water formation in the Nordic Seas during MIS3 stadials, which has an impact of the inflowing surface water. So, adding more lines of discussions here is merited.

In Born et al. (2011) deep water convection remains active in the Nordic Seas during the episode (119-115 ka BP) of weakening of the subpolar gyre circulation / increased heat transport in the Nordic Seas through the Norwegian Atlantic Current (NwAC, i.e. probable continuation of the Continental Slope Current). According to the authors, this is because even if “enhanced salt transport counteracts the general freshening […] locally but does not reverse it” and “despite increased heat transport by the NwAC, no absolute warming of the Nordic Seas is expected at 115 ka because of the counteracting large insolation forcing”.

In our MIS3 case, it seems to be the same situation for the surface freshening since our reconstructed GI-GS SSS anomalies are quite small (0.0 to 0.4 psu). However, the scheme appears to be different concerning SST, with reconstructed SST anomalies of 0.9 to 3.7°C. Due to these relatively low SSS/high SST (and associated changes in stratification), deep convection was apparently strongly reduced in the Nordic Seas during GS, in agreement with all previous studies (e.g. Rasmussen et al., 1996a,b, 1999; Kissel et al., 1999; Van Kreveld et al., 2000; Ballini et al., 2006; Rasmussen and Thomsen, 2009).

Hence, in our MIS3 case, deep convection was likely weaker than during the 119-115 ka BP interval as described in Born et al. (2011). The northward baroclinic volume transport by the NwAC could also be lower in our MIS3 case, even though the northward barotropic transport could have increased, with compensation through a larger export at Fram Strait for instance. Furthermore, subsurface temperature anomalies transported from the south (cf. Fig. 1.d) could imply a larger heat transport due to these warm anomalies, counteracting a potential weaker volume transport. Finally, the larger insolation forcing (insolation at 65°N of ~ 490-500 W/m² during the 30-48 ka BP interval versus ~ 440 W/m² during the 119-115 ka BP interval) could further enhance the impact of this northward heat transport on Nordic Seas SST

We agree that we need to better discuss these different hypotheses in the corrected manuscript.

4- Lines 193–203: I think the discussion that enhanced contribution of moisture from the Norwegian Sea towards Greenland (inferred from SST reconstructions) may played a role in the increase in the deuterium excess recorded in Greenland ice cores during stadials......has discussed in Wary et al. 2016. If so, please summarize and add Wary et al., 2016 as a reference.

The discussion about that in Wary et al. (2016) is quite less explicit, it only concerns the 41-35 ka BP interval, and Greenland deuterium excess data are not directly compared with SST reconstructions. So, if RC2 and the Editor agree with, we would prefer keeping this part in its actual form.

5- Minor issues:

- Please make sure that Müller and Stein, 2014 is included in the reference list.

Sorry about that, we will add it for sure.

- Supporting Information (Line 8): the authors may consider the use of shallow subsurface reservoir age estimates from the northern North Atlantic (e.g., Stern and Lisiecki, 2013; Thornalley et al., 2011) and from the Norwegian Sea (Ezat et al., 2016; Thornalley et al., 2015) to correct for past changes in reservoir ages.

We chose to simply use the Calib 7 automatic reservoir age correction for several reasons:

- Reservoir ages very likely differ depending on the areas and their oceanographical context (in terms of oceanic circulation patterns, stratification/mixing/convection, influence from proximal ice-sheets, other freshwater inputs, …). Hence using reservoir age corrections established in other areas (northern North Atlantic) might not be accurate.
- Reservoir ages have also very likely changed with, and within, the millennial climatic events. Hence, accurate corrections for reservoir age changes implies to use high temporal resolution data of reservoir age variations available for all millennial events of the 40-10 ka BP interval. Such data are available in the North Atlantic sector (Stern and Lisiecki, 2013), but unfortunately only for the last deglaciation in the Norwegian Sea (Thornalley et al., 2015; Ezat et al., 2017).
- Using reservoir age corrections would not have changed anything for the present paper, where age models are firstly constrained with magnetic susceptibility-$\delta^{18}O$ NGRIP tie-points on the time interval discussed.

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