

Reply to the **Interactive comment on “An Ocean – ice coupled response during the last glacial: zooming on the marine isotopic stage 3 south of the Faeroe Shetland Gateway” by J. Zumaque et al., by Anonymous Referee #1, Received and published: 26 September 2012**

REV#1: This paper reports new data from a sediment core taken from south of the Faroe Islands. The authors present magnetic, XRF, foraminiferal and dinocyst measurements in order to investigate oceanographic changes during a part of MIS 3. The paper is well referenced and includes a good introduction to the study and reasonable arguments throughout. **However, I think that a combination of unsatisfactory temporal resolution, age model ambiguities and difficulties with specific proxies makes the interpretations uncertain.**

(...)

Two principal points are raised by Reviewer#1 concerning our study: criticisms concerning the age model (including sampling resolution) and the reliability of the dinocyst proxy. Our reply will thus be organised in two parts each dedicated to these two points. For convenience we have underlined some specific comments made by Reviewer 1, which are all in black whereas our replies are in dark red.

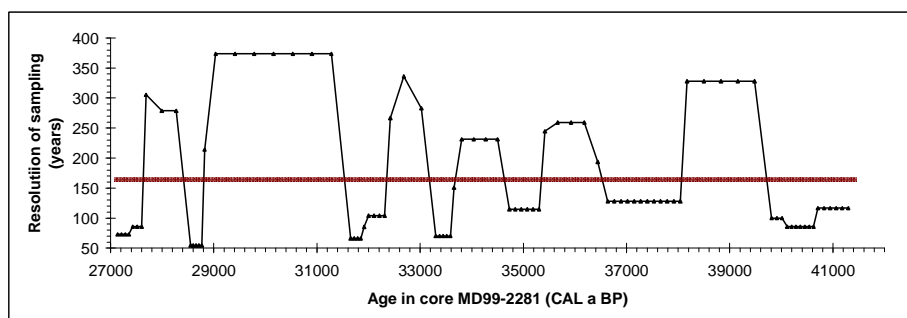
1. the age model/ resolution of sampling.

(...)

The study makes use of a high sedimentation rate core, which has the potential to give a highly focussed view of the changes of interest here. And yet the authors have chosen to sample with a frequency of 10 cm. Looking at their records, it strikes me that they would have done better to have either focussed on fewer analyses at higher resolution (e.g. at 2 cm resolution), or narrowed the window of their multi-proxy study to attain higher temporal resolution (in this respect it strikes me that use of the word ‘zooming’ in the title is not really appropriate). With so many high quality records out there, it is surprising that better advantage of this core has not been taken. The result is several (hard won I am sure) records that show rather inconsistent patterns of variability and as such pose a problem for straightforward interpretation. Without delving too far into the detailed interpretations I will comment on some of the more immediate concerns I have with the study.

(...)

Reply: Rev# 1 recommends a higher sampling resolution for our study. However, our sampling strategy provides us with a mean age resolution of 160 years (from 55 to 374 years between two points at worst) over nearly 15 000 years (see the Figure below). Very few studies exist in the literature giving a comparable resolution in the marine environment for this time window, i.e. MIS3. Our aim to conduct a multiproxy study, thus implying multiple analyses on each sample also limits the number of samples to study. We chose to cover the whole MIS3, which represents a zoom within the last glacial period (to justify our Title).



Rep. Figure 1:
variation of the sampling resolution along time.

(...)

Obviously **the need for a reliable age model is of central importance to a study of this nature**. The authors follow previous work in tying their record of magnetic susceptibility to the Greenland ice core temperature record. I do not have a problem with this approach per se and the resulting age model does 'make sense' for some (although not all) of the proxy records obtained from the core. However, the age model developed here does not strike me as particularly convincing, especially in the older section of the record. The authors show the record of mag sus in Fig 3, which shows repeated oscillations that are claimed to parallel D-O oscillations over Greenland (basis for the age model). But many of the variations outside of their study window appear not to have equivalents in the Greenland record. Can the authors explain why the "1:1" correlation between mag sus and Greenland temperature breaks down outside their study window? Or at least argue that it doesn't break down.

(...)

Reply: Rev# 1 seems to be not convinced by our age model construction and especially doubts about the use of peak-to-peak correlation between MS and NGRIP. It has been shown a long time ago (Kissel et al., EPSL 1999) that in this area as along the path of the ISOW and DSOW, the variations in the magnetic concentration (here illustrated by susceptibility and ARM on Figure 3 & 6) mimic the changes of temperature over Greenland. The mechanisms of that have been described and discussed in different articles since then. This synchronism has also been proven by the comparison in the same cores of the changes in the earth magnetic field intensity with the variations in production of the cosmogenic isotopes as measured in ice (Laj et al., 2000, 2004, Wagner et al., 2000; Svensson et al., 2006; 2008).

Our interest here is for the time interval between about interstadial 11 and interstadial 3. For older periods (i.e. below 2200 cm), we have not constrained the age model because we did not work specifically on that period. If needed, the correlation could be pursued using the earth magnetic field intensity as correlation tool and it would show for example that H5 is at 2300 m, H6 at 2592 cm. However, again, the other proxies are not yet generated for this time interval (we prefer to focus on a shorter interval, see the answer to the first comment). For the topmost part of our record, the stratigraphy includes radiocarbon dates (calibrated) in addition to the comparison with NGRIP.

(...)

Secondly the authors chose to throw out a ^{14}C age at 1820 cm depth because it does not fit with their tuning strategy. How do the authors explain the presence of NPS with a (calibrated) calendar age of >2000 older than that predicted by their tuning approach? One cannot simply throw out a constraint because it doesn't fit within the paradigm.

(...)

Reply: Age dating uncertainties are growing with age. Beyond 30 ka, the accuracy of ^{14}C dating is limited and affected by two major biases:

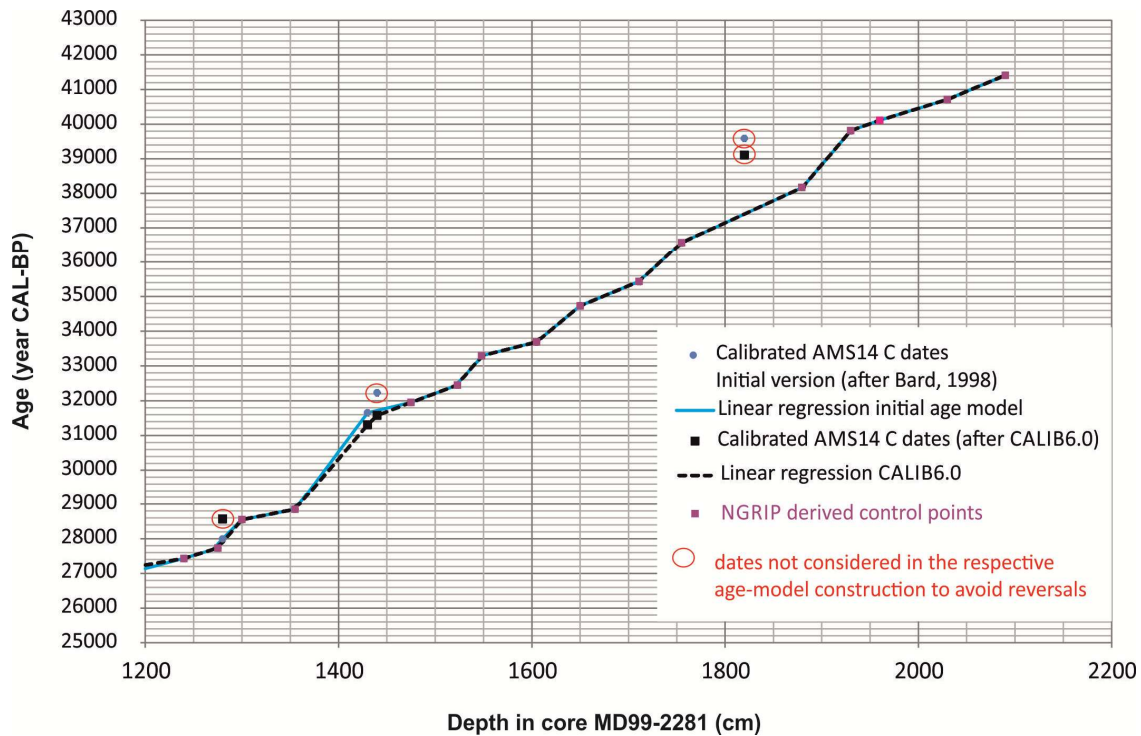
1. the inherent higher uncertainties linked to the ~ 5.7 ka half-life of ^{14}C (practical limit of the ^{14}C method = 45,000 yr BP, e.g. Bard, 1998. *Geochimica & Cosmochimica Acta* 62)
2. the uncertainties in the existing calibration methods beyond 25 ka (e.g.; Bard, 1998; Stuiver et al., 1998; Bard et al., 2004a and b)

That is why we have preferred to not retain the oldest age (i.e., at 1820 cm = 34610 +/- 290 ^{14}C years BP) and to refer rather to the NGRIP tuning method. This protocol furthermore follows the "INTIMATE" recommendations (e.g. Austin & Hibbert, QSR 2012, Austin et al., QSR 2012).

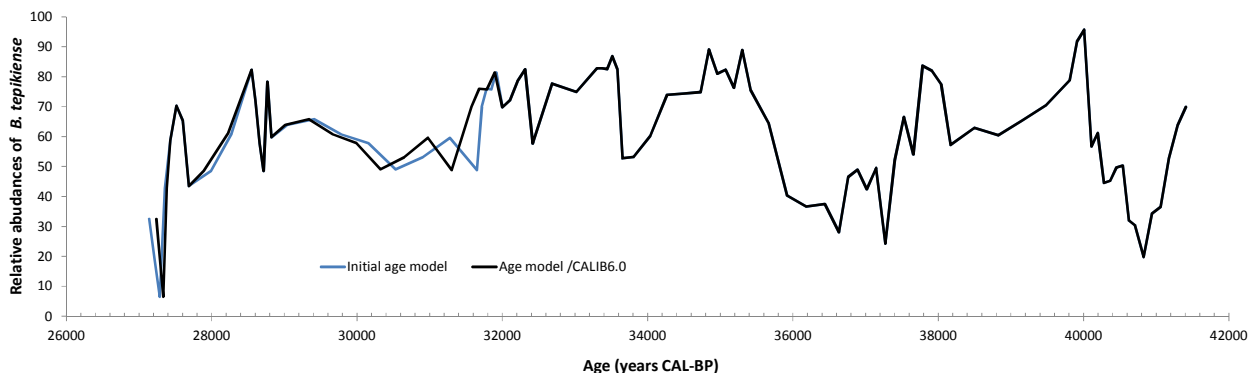
However, to validate the consistency of our age model we have tested it converting our AMS ^{14}C ages with the new version of CALIB6.0 extending the calibration back to 50 Ka (Reimer et al., 2009, instead of the Bard 1998 glacial polynomial conversion) and thus reducing the uncertainties in the oldest sections. Very few changes do occur (see below). The date at 1820 cm (34610 +/- 290 ^{14}C years BP) still remains 2 ka too old and can not be included in the age model construction. The revised version of Table 1 now includes the two calibration results.

In addition, we have added the profile of the earth magnetic field intensity for this time period (see following comment), well calibrated in time on the ice age models and which has no ambiguity for this time period.

Our conclusion is that even with an updated calibration conversion set, some inconsistencies remain from the ^{14}C dating in the older part of the record. Probably age reservoir offsets have to be considered also but no ΔR reference exists in the literature for this area and this period (a value of ca 1000 yr (± 250 yr) over the period 15,000 –11,000 cal. yr BP have been suggested by Björck et al., QSR 2003, for the Norwegian sea). We thus decided to keep our age model as initially constructed.



Rep. Figure 2: comparison of two age models built with different calibration conversion curves: initial version built with Bard, 1998 conversion curve in blue (see methods), and version with CALIB6.0 conversion in black. In the two cases, some ages constitute reversals (red circles).



Rep. Figure 3: incidence on our set of proxies (*B. tepikiense* abundances): comparison of the two age models (after Bard, 1998 in blue / CALIB 6.0 in black).

Depth (cm) in core MD99-2281	AMS ¹⁴ C Age uncorrected (a BP)	Calendar Age corrected (CAL -a BP) Bard, 1998	Calendar Age corrected (CAL -a BP) CALIB 6.0	1 σ ranges: [start]	1 σ ranges: [end]	Coherency of the two Calibration method (Δ)	Tie points (Age in CAL -a BP)	Dated Material / Age control points / References
40	10 260 +/- 100	11 165	11 261	11 137	11 384	95	11 165	<i>G. bulloides</i> , S.Boulay (2000)
170	12 970 +/- 110	14 729	14 657	14 156	15 158	72		<i>G. bulloides</i> , S.Boulay (2000)
270	10 800 +/- 100	12 081	12 229	12 070 12 478	12 388 12 517	148	12 081	<i>G. bulloides</i> , S.Boulay (2000)
400	11 060 +/- 110	12 400	12 431	12 392 12 520	12 470 12 675	31	12 400	<i>G. bulloides</i> , S.Boulay (2000)
510	15 290 +/- 130	17 529	18 037	17 931 18 261	18 142 18 490	507	17 529	<i>N. pachyderma</i> s., S.Boulay (2000)
580	15 760 +/- 170	18 092	18 219	18 209 18 494	18 228 18 817	126	18 092	<i>N. pachyderma</i> s., S.Boulay (2000)
650	16 040 +/- 140	18 427	18 777	18 651	18 902	349	18 427	<i>N. pachyderma</i> s., S.Boulay (2000)
780	18 060 +/-160	20 830	20 630	20 562 20 903	20 697 21 361	200	20 830	<i>N. pachyderma</i> s., S.Boulay (2000)
1180	23 340 +/- 240	26 993	27 144	27 037 24 522	27 251 28 087	151	26 993	<i>N. pachyderma</i> s., S.Boulay (2000)
1240							27 430	Correlation NGRIPGICC05 , GI3 termination, Wolff et al., 2010
1275							27 730	Correlation NGRIPGICC05, GI3 warming, Wolff et al., 2010
1280	24 210 +/- 140	27 992	28 567	28 326	28 807	574	27 992	<i>N. pachyderma</i> s., ARTEMIS 2010
1300							28 550	Correlation NGRIPGICC05, GI4 termination, Wolff et al., 2010
1355							28 850	Correlation NGRIPGICC05, GI4 warming, Wolff et al., 2010
1430	27 430 +/- 140	31 652	31 306	31 211	31 400	346	31 652	<i>N. pachyderma</i> s., ARTEMIS 2010
1440	27 940 +/- 140	32 226	31 576	31 411	31 740	650		<i>N. pachyderma</i> s., ARTEMIS 2010
1475							31 950	Correlation NGRIPGICC05, GI5 termination, Wolff et al., 2010
1523							32 450	Correlation NGRIPGICC05, GI5 warming, Wolff et al., 2010
1548							33 290	Correlation NGRIPGICC05, GI6 termination, Wolff et al., 2010
1605							33 690	Correlation NGRIPGICC05, GI6 warming, Wolff et al., 2010
1650							34 730	Correlation NGRIPGICC05, GI7 termination, Wolff et al., 2010
1711							35 430	Correlation NGRIPGICC05, GI7 warming, Wolff et al., 2010
1755							36 570	Correlation NGRIPGICC05, GI8 termination, Wolff et al., 2010
1820	34 610 +/- 290	39 586	39 105	38 722	39 488	481		<i>N. pachyderma</i> s., ARTEMIS 2010
1880							38 170	Correlation NGRIPGICC05, GI8 warming, Wolff et al., 2010
1930							39 810	Correlation NGRIPGICC05, GI9 termination, Wolff et al., 2010
1960							40 110	Correlation NGRIPGICC05, GI9 warming, Wolff et al., 2010
2030							40 710	Correlation NGRIPGICC05, GI10 termination, Wolff et al., 2010
2090							41 410	Correlation NGRIPGICC05, GI10 warming, Wolff et al., 2010
2110							42 290	Correlation NGRIPGICC05, GI11 termination, Wolff et al., 2010
2170							43 290	Correlation NGRIPGICC05, GI11 warming, Wolff et al., 2010

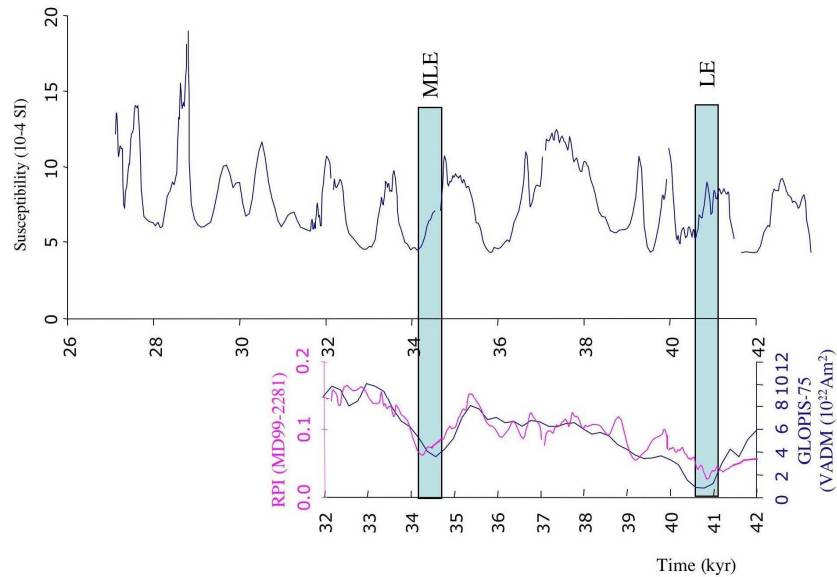
Table 1 (revised version)

(...)

The authors claim that they observe the effect of the Laschamp (and Mono Lake) excursions within their records and use this to support their age model. Then they must show evidence of these excursions (e.g. a record of normalised paleo-intensity) if we are to be convinced.

(...)

Reply: We add now the relative paleointensity (RPI) record (see below) from this core in the revised version on Figure 4, together with the reference RPI curve for this period which is GLOPIS-75.



Rep. Figure 4: relative paleointensity (RPI) record from core MD99-2281, together with the reference RPI curve for this period which is GLOPIS-75

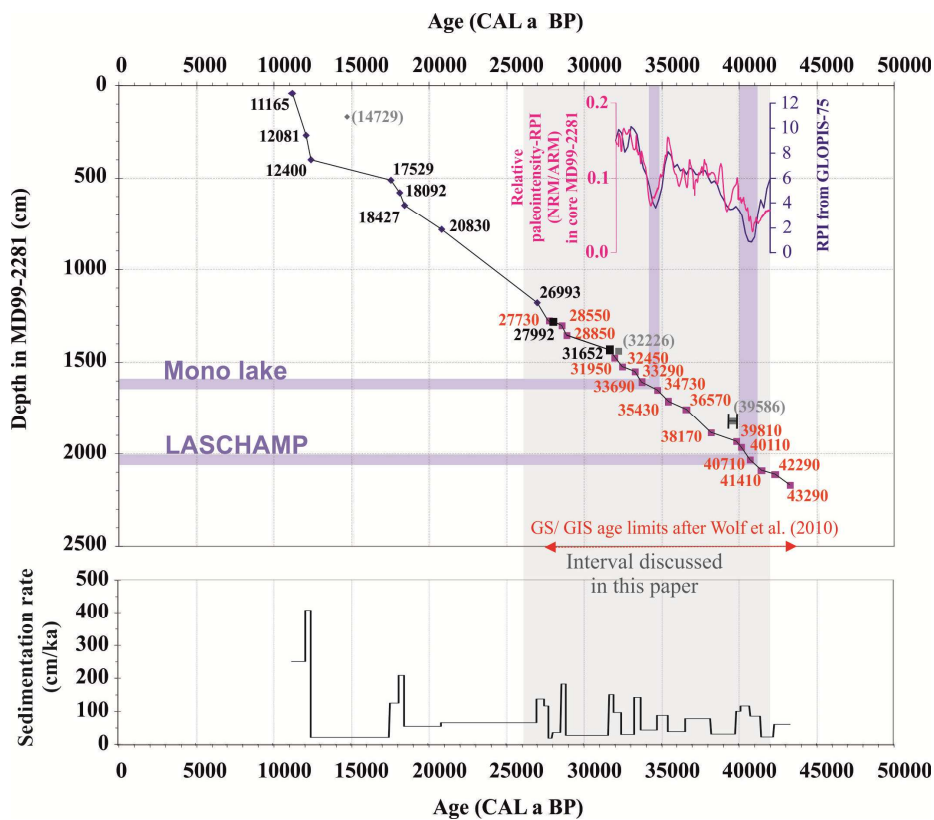


Figure 4 (revised version)

The RPI record from core MD99-2281 is the same using either the susceptibility, the ARM of the IRM as normalizers of the NRM and clearly shows the two minima around 34 ka (Mono Lake excursion (MLE) intensity minimum) and around 41 ka (Laschamp excursion (LE) intensity minimum). It is known that the MLE is in phase with the end of interstadial 7 (stadial between 7 and 6) (Laj et al., 2000; 2004; Kissel et al., 2011; Svensson et al., 2008) and the LE is in phase with the end of interstadial 10 (Laj et al., 2000; 2004; Kissel et al., 2008) and it is precisely dated from lava flows (Guillou et al., EPSL 2004; Singer et al., EPSL 2009). Glopis-75 (Laj et al., 2000; 2004) is also reported as the reference curve for this time interval in order to show how much the records are similar so not only the two excursions can be used as tie points but also the entire profile is consistent.

(...)

The apparent weakness of the age model (in my eyes) presents a serious problem with the interpretations of the other records presented here. (...)

Reply: As we show above, the arguments used to construct the age model of core MD99-2281 for the time interval investigated here i.e. ^{14}C dating, earth magnetic field paleointensity profile (including the two excursions) are internally consistent. They all lead to high similarity between the variations in magnetic concentration and changes of temperature over Greenland as already shown a while ago by Kissel et al. (EPSL, 1999) for this area. We can start with a wiggle matching adjusting between two ^{14}C dating and *a posteriori* check with the earth magnetic field intensity record (as we did) or do the other way around (if the referee prefers that, we can change it in the paper) but we wish to emphasize that, given these consistency, the final result on the age model will be exactly the same. It is not so often that we have such a conjunction of consistent tools to construct an age model through MIS3 and we hope to have convinced the referee about its robustness.

2. the proxies.

(...)

To makes things more complicated, **the proxy records obtained from foraminifera and dinocysts show rather inconsistent trends** and do not lend valuable support to the age model (even if we believe the age modelling strategy). **The record of %NPS looks reasonable and IF the corresponding $\delta\text{A}_d^{18}\text{O}$ record is dominated by temperature variations rather than local salinity, then that too makes sense (within the age model approach).** The dinocyst records appear less sensible. I will admit that I am no expert on these latter proxies but observation of such warm summer conditions associated with the coldest ‘mean annual’ conditions (according to the foraminifer assemblages) seems to make little sense. If summer is so warm then why are there so few foraminifer species other than NPS?

(...)

Reply: Analyses are carried out in the same samples, so, the correlation in the core is the same considering the construction of an age model or not...it means that the anomalies detected in the response of some proxies do exist independently of the event. **This is the striking and interesting feature of our record.**

Many reasons could explain the observed discrepancies, **primarily because dinocysts and foraminifera represent two “planktonic” groups which have very different ecological requirements.** The first group belongs to the organic phytoplankton whereas the second one is a calcareous zooplanktonic dweller. It is now well established that foraminifera could migrate within the water column, and that apart for symbiotic species, they could thrive below the thermocline/pycnocline (up to 700 m, e.g., Fairbanks et al., Science 1980). The polar species *Neogloboquadrina pachyderma* sinistral (NPS) form, which represents the dominant species in our planktonic foraminifera record (between 25 and 100%), is especially known to live at or below the pycnocline, notably at sites where low salinities characterize the surface layer (e.g. Simstich et al., Marin. Micropal. 2003; Jonkers et al., Paleoceanography 2010). Hillaire-Marcel et al. (QSR, 2004) have suggested that in the Arctic domain large specimens of this species rather

thrives in subsurface waters, at warmer and deeper temperature habitats which characterise the underlying waters originating from the North Atlantic. Evidence thus demonstrates that NPS rather thrives and calcifies in favourable -principally mesopelagic- environments. It implies therefore that the isotopic composition of its shell could also be likely related to sub-surface conditions rather than carrying an “absolute” surface signal. Additionally, during cold episodes of the Quaternary, monospecificism of NPS, if decidedly signing migration of cold water tongues, biases the sensitivity of the planktonic foraminiferal proxy (see for instance discussion in Kucera et al., QSR 2005, de Vernal et al., QSR 2006).

Dinoflagellates preferentially occupy the topmost fifty meters (e.g. de Vernal & Marret, 2007). Many species are highly tolerant toward low salinity (from Wall et al., 1977 to de Vernal & Rochon, 2011). This specificity could explain why they develop in surface waters where adverse conditions are recorded for foraminifera (for some then probably living deeper in the water column ...).

Episodes characterized by "warm summers with no foraminiferal bloom" as pointed out by Reviewer 1 could thus be realistic considering the respective ecological tolerances of foraminifera and dinocysts and especially, if we envisage as suggested in our manuscript a strong stratification of the water column.

Our study illustrates the need to compile a maximum of different proxies. The interpretation of one kind of signal, derived from one unique source (from planktonic foraminifera for instance, used classically for geochemical or paleoecological investigations) is too equivocal.

Finally, to reply to the remark "...the coldest ‘mean annual’ conditions (according to the foraminifer assemblages) seems to make little sense." Traditionally, transfer functions sensu lato derived from foraminifera provide reconstructions considering the coldest and warmest periods of the year: this is a compromise done to relate at the best foraminifera ecology and quantification used for past climate reconstructions (see Pflaumann et al., Paleoceanography 1996 for a review).

(...)

Furthermore, the ‘anti-phase’ nature of the Feb versus August SST records seems to hint at a problem.

(...)

Reply: The SST reconstructions obtained are generated by the modern analog technique. As cited in the material and method section: "...This statistical tool principally uses the statistic distance between fossil (paleoceanographic record) and current (modern data base) assemblages. ... Calculations rely on a weighted average of SST values (compiled from the 2001 version of the World Ocean Atlas) from the best five modern analogues, with a maximum weight given for the closest analogue in terms of statistical distance / i.e. dissimilarity minimum (e.g. Kucera et al., QSR 2005; Guiot and de Vernal, 2007). ...". A threshold in the dissimilarity values limits the search of analogues (some kind of security) to avoid unrealistic reconstructions. In the case of our study, 5 analogues were systematically found. It thus means that the weighted average does represent modern existing situations. In the case of “antiphase” between February and August SST, i.e. **in fact warmer winter (above 6°C) and cooler summer (10 to 12°C), and thus conditions closest to the modern ones**, modern analogues have been found in the subpolar domain of the North Atlantic / Nordic seas.

(...)

Why are the coldest summer temperatures associated with interstadial events?

(...)

Reply: The coldest summer temperatures are not exactly associated with interstadial events but are recorded in the last part of the GIs as explained in the manuscript. This raises the question of some sort of paradoxical response from the southern Norwegian sea-surface environments during those transitions. The same observations have been done previously at a lower resolution from two cores from the same region (Eynaud et al., JQS 2002). In the present paper, the resolution is not as low as claimed by Reviewer 1 (see the beginning of this reply), and

trends detected in some proxies, if spiky, are coherent with the higher resolution records from the MD99-2281 core (XRF ratio for instance at a 2 cm resolution).

Part of the problem I have with these records again relates to the resolution – more specifically, the apparent noise to signal ratio. Some of the interpretations made by the authors appear to hang on single data points – if they were robust variations, then increasing the resolution would provide critical support for the interpretations. As it stands I have difficulty believing that all of the variations plotted here are real. It doesn't help that the authors have not plotted any data points in Fig. 6.

(...)

Reply: Data points are visible on Figure 5 but will be also included in a revised version of Figure 6 for some proxies (not all as it is the same sampling for each used in the destructive study of the core, see below).

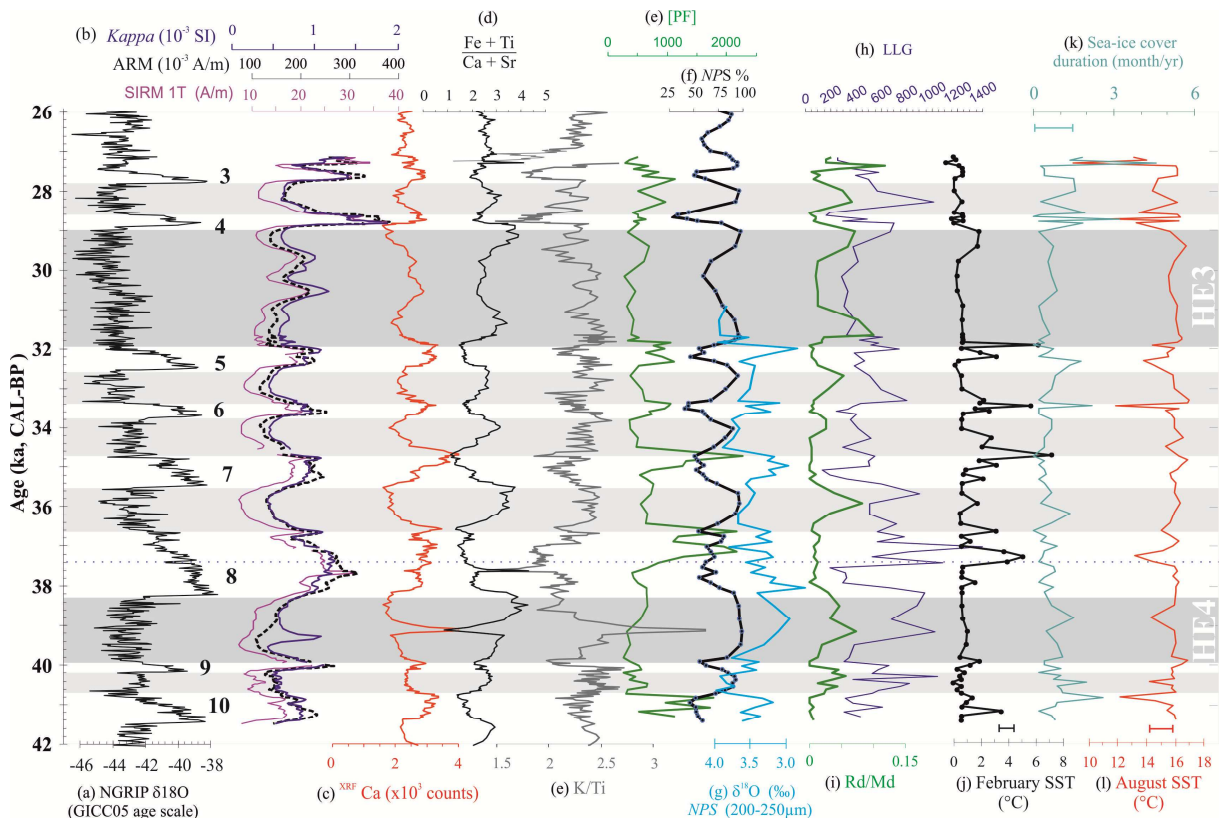


Figure 6 (revised version)

(...)

Other points:

Dinocyst assemblages: Surely modern dinocysts could also be reworked? If so then how can we know that the assemblage represents contemporaneous conditions at all? Especially in such a sedimentologically dynamic setting

(...)

Reply: Transport could affect all the set of microfossils which are used for paleoceanographical reconstructions... It is true for dinocysts as well as for foraminifera, benthics as planktonics even those commonly used for geochemical analyses (¹⁸O, Mg/Ca...). The reworked species identified are dated from the Mesozoic and/or the Cenozoic and have been advected during events of massive terrestrial sourced deposits. Their ratio versus modern dinocysts indicates phases during which abundant weathering affected the continent (from ice-flow or river processes, see Zaragosi et al., 2001).

Regarding transport of dinocysts the question has since long been tackled by several contributors once this proxy have been integrated in (paleo)ceanographic studies. Efforts have been made to

evaluate the biases due to the transportation of cysts (e.g. from Dale, 1976. "Cyst formation, sedimentation, and preservation: Factors affecting dinoflagellate assemblages in recent sediments from Trondheimsfjord, Norway". Review of Palaeobotany and Palynology 2, to Zonneveld & Brummer, 2000. "(Palaeo-)ecological significance, transport and preservation of organic-walled dinoflagellate cysts in the Somali Basin, NW Arabian Sea". Deep-Sea Research Part II-Topical Studies in Oceanography, 47, or again Marret et al., 2004 where a specific section is dedicated to this issue): dinocysts are belonging to the classical set of microfossils (i.e. diatoms, foraminifera, pollen, radiolarians) and are in the range of silt to fine sand sized grains. They are thus submitted to transport processes affecting this category of grains. The confrontation of results derived from their analysis to those of foraminifera is not requiring more caution (and in fact even less) than those numerous studies which compare stable isotope measurements in planktonic foraminifera to various geochemical compound indexes (UK37 ...for instance). Biases due to transport or either preservation are much more consequent regarding these later extreme categories of proxies, sowhy dinocyst results should be more questionable?

Finally, the dynamic setting is exceptional for the area. It is indicated page 6 of the present version of the paper: "It has been retrieved at a location where seismic continuous parallel draping internal reflectors (see seismic section C in Boldreel et al., 1998) have been attributed to pelagic sediments deposited in a low-energy, deep-water environment, unaffected by the strong current activity (Boldreel et al., 1998)." Reworking could of course occur from processes occurring upstream within the regional active current system but again the whole sediment fraction would have been concerned...**does it mean that marine sedimentological archives should not be investigated any more, even if they have been retrieved from selected and pondered depositional environments, circumscribing at the best taphonomical processes?**

(...)

Smaller points:

p3045 line 8: Specify when this sector was under the proximal influence

p3045 line 10: How can a sector record a response? Perhaps use 'responded to'?

p3045 line 21: Don't use 'typify', perhaps 'suggest'?

p3046 line 5: 'millennial', not 'millennium'

p3046 line 23: replace 'resulting in' with 'corresponding to' – we don't know what drives what

p3053 line 11: data generated every 2 cm with resolution close to 4 cm – need to explain this better

p3054 line 11/12: This needs better explanation

p3055 line 16: subscript 2 for CO₂

p3060 line 5: 'If the D-O structure is clearly recognizable' – I would say that it is not, but perhaps the authors would disagree

p3061 line 19: do not overstate it – for 2 records to perfectly mirror one another they would need to do just that – they do not.

p3063 line 21: 'classically' would relate to a large number of other studies – perhaps replace with 'otherwise'

Those points will all be corrected in the revised version of the paper.

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