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Interactive comment on "Climate variability and long-term expansion of peat lands in Arctic Norway during the late Pliocene (ODP Site 642, Norwegian Sea)" *by* S. Panitz et al.

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We would like to thank Timme H. Donders for his very constructive and helpful comments. In response to this we have taken the following actions:

Reviewer #2 (T. Donders): An important advance in the study is an improvement of the age model, but unfortunately the status of the Risebrodbakken in prep. paper on the age model is still very unclear, and therefore some more information on the dating approach and resolution is needed in the Pantiz et al. manuscript, preferable in a figure. Given the aims of high resolution documentation and targeting of the KM5c isotope stage this is important information for the study. Also, while isotope stratigraphy

is referred and correlated to it is important to also show a compilation (e.g LR04) or relevant record for comparison so readers can assess the consistency of both pollen and isotopes in the summary figs 6 or 7. This is especially important for the position of M2 and KM5c.

R1: We fully agree with reviewer #2 and will add an additional figure with the age model, showing the correlation of the LR04 benthic oxygen isotope stack with the δ18O curve from ODP Hole 642B (after Risebrobakken et al. in review). The LR04 stack will also be added to Figure 6.

The paper focuses on a climatic interpretation of the record, which might very well be justified, but little attention is given to taphonomic issues, roles of shifting depot centers and run-off patterns which might also explain the observed changes. For example, the uppermost zone 2B-II is part of the mid Piacenzian warming yet seem to indicate substantially drier conditions, which is counter-intuitive in this setting and latitude as warm surface waters would bring both heat and moisture. In fact, other low precipitation anomalies in the record indicate cooling . . . could this be a taphonomic bias? Consistently higher diversity, rare types and higher concentrations in upper section (zone 2B-Ib) suggest increased river runoff and/or proximity of river outflow, is anything known about paleo river development along the coast and how does this impact the climate curve? How is this related to uplift phases (section 2.3) Compare e.g. Eidvin et al., 2014, Marine and Petroleum Geology. To eliminate potential overrepresentation of Pinus due to sea level and other transport factors it might be useful to calculate abundances excluding Pinus from the percent sum. Although Pinus is rather constant, exclusion might produce a more realistic representation of the vegetation cover.

R2: Reviewer #2 raises a valid point and we have added the following sentences: p. 5763, line 9: Pollen preservation is generally very good. P. 5763, line 19: Uplift phases and shifting depocentres as documented in Eidvin et al., 2014, Marine and Petroleum Geology, are restricted to the basins off the Norwegian coast and should not have affected the site. We added a sentence to the manuscript to make clear that there is no indication for increased river runoff and/or proximity of river outflow (see reviewer #1, R10). As already mentioned in the method section (Page 5763, lines 22-23), Pinus has been excluded from the calculation of pollen percentages.

Regarding the climate calibrations: the climate estimates seem to hit a plateau value in many samples, is this a feature of the calibration dataset and method or the data? And how do the Utescher and Mosbrugger, 2013 calibration data compare to the Greenwood et al. 2005 calibration datasets, and why was one calibration dataset preferred over another? Also, how do the quantitative estimates based on the recent samples in Fig. 3 compare to present climate data? Did the authors test this? Independent tests of the calibration methods on modern vegetation samples have revealed significant mismatches of the climate estimates which are not discussed here (see Grimm and Denk, 2012 Rev Pal&Pal). For example, how do the reconstructed low precipitation values compare to the often high Sphagnum values (e.g. zone 2B-Ia) during these periods which the authors attribute to higher humidity?

R3: Reviewer #2 raises several important issues regarding the suitability of our record for applying quantitative climate estimates and the Coexistence Approach (CA). As mentioned in the manuscript, the very wide range of the climate estimates, including the "plateau" mentioned by Reviewer #2, are partly an artefact of the relatively low number of taxa used for this approach. The "modern" samples only show a modern "cool" temperature if we exclude single rare taxa (i.e. "long distance transport") from the dataset. For that reason, we set in our manuscript an initial threshold of 1% for the Pliocene climate reconstructions (see page 5771, line 16). However, with the exclusion of taxa we increased the range and uncertainty of our climate estimates even further. Our overall number of tree taxa is too low to apply the Greenwood et al. (2005) calibration dataset, which in addition does not include Asian taxa, such as Pterocarya and Sciadopitys. We therefore used the dataset from the Palaeoflora database for our quantitative climate estimates. Given the great uncertainty and wide ranges of our climate reconstructions, which have been discussed in section 5.7, we do not think that

it is useful to discuss in more detail the accuracy of the CA method and/or fluctuations in climate reconstructions as suggested by reviewer #2. However, we think reviewer #2 raised very important issues here and we therefore decided to remove Figure 7 and the stratigraphic climate reconstructions from the final manuscript. We changed section 5.7 which now only provides a single qualitative and quantitative estimate for the entire Pliocene record and discusses the uncertainties mentioned above.

P. 15, lines 28-31: Abundant Asteraceae in combination with dominant Pinus also occur in open wet prairie communities in Southern Florida, indicating fire-controlled moist conditions in warm (subtropical) climates with, in areas, salt influence. While not necessarily a climate analogue, it does provide a comparable palynological signal (see Willard et al, 2001, Rev. Pal &Pal) and the possibility of a coastal-derived signal should be at least discussed (although likely dismissed) in the explanation of zone 2A.

R4: High Asteraceae percentages do not only occur in modern pollen records from Florida but also in Pliocene records from Africa. We do not agree that the modern pollen surface samples from the sub-tropical biomes of Florida show a "comparable" palynological signal. Apart from high Pinus pollen percentage in both records, the boreal – cold temperate forest biome signal is distinctly different. The Asteraceae signal in our record seems to have no modern analogue, and we would prefer not to compare our record with any modern subtropical "pseudo-analogues", as this could be highly misleading in regard to Pliocene climate reconstruction.

Technical & minor comments: Improve subdivision scale x-axes figure 7

R5: Figure will be removed from the final manuscript.

Fig 4: Lycopodium clavatum or annotinum or other type?

R6: Changed to Lycopodium spp.

Inclusion of total group curves (conifers, trees, herbs, spores) in fig 4 would be very informative The authors use an uncommon zonation coding; while hierarchy of CONISS zonation seems strictly used, this is a little artificial and not really realistic. It is better just use increasing numbers with a code relevant to the site or Pliocene stage.

R7: Total groups curves will be added to Figure 4. The zonation coding will be changed to increasing numbers and referred to as Pollen Zone (PZ) 1, 2, etc.

P.2, Line 20: "exceeds" is unclear here, in which way does is exceed: due to higher detail (hence shorter variability) or longer due to the entire length of the interval. While the authors seem to point at the first case this excludes influence of long cycles such as eccentricity and long-term (1.2 /2.4 Ma) nodes in obliquity variation.

R8: As the length of the mPWP exceeds orbital timescales...

p. 2, Line 29: magnitude of warming relative to present

R9: Changed

P.3, line 6: Arctic

R10: Added

P.3, line 27: magnitude of past warming

R11: Changed

P.5, line 10: precipitation does not follow a gradient, but forms it.

R12: Changed

P.5, line 17: tree birch (Betula pubescens)

R13: Changed

P6., line 6: upper limit of raised bogs: upper limit of % land cover, or absolute height, or is northernmost extension meant?

R14: Extensive, domed raised bogs

P.7, line 8: Sieving approach (< 63 micron) likely removed Abies and potentially part of Picea fraction

R15: The loss of large Pinaceae pollen due to sieving through a 63 μm mesh has been discussed in R3, reviewer #1. The following changes were made to clarify (p. 5762, line 23): "All samples were pre-sieved through a 63 μm 0.063 mm mesh to retain foraminifera for oxygen isotope analysis in Bergen, Norway (Risebrobakken et al., 2015). In order to identify a potential bias in the pollen data resulting from larger Pinaceae pollen grains exceeding 63 μm, we compared sieved and unsieved samples, and no difference in the overall pollen count has been found."

P.7, line 28: Reference of CONISS is Grimm et al., 1987: Grimm, E.C. 1987. CONISS: a FORTRAN 77 program for stratigraphically constrained cluster analysis by the method of incremental sum of squares. Computers & Geosciences 13, 13-35.

R16: Changed

P.7, line 30: did diversity estimate also take into account the varying count sums (rarefaction of the richness, see Birks and Line, 1992)? This is implemented in PAST as well and should be used to normalize and intercompare palynological richness between samples in variable counts sums prior to calculation of the Shannon index.

R17: We thank reviewer #2 for this comment and have added a rarefaction curve using PAST software to Figure 6.

P.9, line 27 correct Querucs

R18: Changed

P. 11, line5/6: pollen taxa . . . are absent. See also lines 26/27

R19: Changed

P.12, line 5/6: Higher sedimentation rates generally lower ("dilute") the concentration, so how can they here explain the opposite? Again an increase in run off could be a reason, is there indication for enhanced river run off?

R20: We agree with reviewer #2 and deleted the respective sentence. However, as already explained in response to reviewer #1 (R10), we have no indication for any influence from river run off.

P. 13, line 28: correct "reaches is"

R21: Done

P. 16, line 18: I don't understand to what observation this statement refers to; in what way does site 642B represent "stable cold conditions", it seems to be quite the opposite? And why these figures are indicated as the climate reconstruction is in fig. 7"?

R22: We removed the word "stable" in order to prevent misunderstandings.

P.17, lines 13-15: I cannot follow the line of reasoning here, please explain some more.

R23: We revised the respective sentence to clarify the interpretation of a wetter climate. It now states: "Around 3.3 Ma the high relative abundance of Sphagnum spores reflects a spread of peat lands and high precipitation."

P.17, line21: double comma

R24: Changed

P.17, line 30: explain "modern-like ice configuration"

R25: In the Northern Hemisphere a modern-like ice configuration with glaciation in Greenland, Iceland, the Barents Sea and Scandinavia is suggested for MIS M2 (De Schepper et al., 2014).

P.18, line 22: I would say Pinus remains approximately stable after the 3.26 Ma decline

R26: Changed to: Thereafter, percentages of Pinus are quite variable, suggesting repeated warmer phases.

P. 18, diversity is directly relate to climatic warming which is not necessarily the case

R27: We agree with Reviewer #2 and revised the respective sentence to prevent misunderstandings: "Taxa diversity is higher during the warmest climatic conditions within the Piacenzian record (3.26 and 3.18 Ma) when compared to the preceding cool intervals (3.47–3.29 Ma) (Fig. 6)."

Interactive comment on Clim. Past Discuss., 11, 5755, 2015.