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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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General comments

The Panitz et al. study of high resolution Piacenzian vegetation and climate from the Norwegian Sea ODP Site 642B is a very valuable contribution to understanding high latitude climate variability, vegetation zonation and long-term dynamics that fits the scope of Climate of the Past very well. The results help dedicated model-data comparisons and in particular document a higher temporal climate variability for the mid Piacenzian Warm Period, which preceded the cooling towards substantial northern hemisphere glaciation. The record gives new detailed insight in the climatically

important high latitude regions and provides some interesting suggestions for direct climate-vegetation feedbacks, although these are not quantified in detail. The paper is generally well written, the data seems of high quality and is documented well but in my view some important interpretation details and discussion points are missing. I am however confident the authors are able to correct these issues.

Specific comments:

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.

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

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abundances excluding Pinus from the percent sum. Although Pinus is rather constant, exclusion might produce a more realistic representation of the vegetation cover.

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?

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.

Technical & minor comments:

Improve subdivision scale x-axes figure 7 Fig 4: Lycopodium clavatum or annotinum or other type? 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.

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

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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. p. 2, Line 29: magnitude of warming relative to present P.3, line 6: Arctic P.3, line 27: magnitude of past warming P.5, line 10: precipitation does not follow a gradient, but forms it. P.5, line 17: tree birch (Betula pubescens) P6., line 6: upper limit of raised bogs: upper limit of % land cover, or absolute height, or is northernmost extension meant? P.7, line 8: Sieving approach (< 63 micron) likely removed Abies and potentially part of Picea fraction 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 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. P.9, line 27 correct Querucs P. 11, line5/6: pollen taxa ... are absent. See also lines 26/27 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? P. 13, line 28: correct "reaches is" 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"? P.17, lines 13-15: I cannot follow the line of reasoning here, please explain some more. P.17, line21: double comma P.17, line 30: explain "modern-like ice configuration" P.18, line 22: I would say Pinus remains approximately stable after the 3.26 Ma decline P. 18, diversity is directly relate to climatic warming which is not necessarily the case

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