

Interactive comment on “Vegetation and climate development on the North American Atlantic Coastal Plain from 33 to 13 million years ago (IODP Expedition 313)” by U. Kotthoff et al.

Anonymous Referee #2

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The paper presents an Eocene/Oligocene to Miocene vegetation and climate reconstruction based on palynological analyses of marine sediment cores taken on the New Jersey shelf during IODP Expeditions 313. The authors added one single Pleistocene sample of questionable value to the reconstruction which I suggest to remove from this paper. The interpretation of the pollen record is detailed, and the authors apply a multitude of quantitative and qualitative methodological approaches. Unfortunately this is also one problem of this manuscript as the scientific relevance of the applied methods for the interpretation is often not clear. E.g. the authors present several palynological methods to identify mass wasting events and separate transport-related changes from “real” vegetation changes. However their identification of samples which are “more

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reliable” (6567, line 20) for paleoenvironmental reconstructions does not seem to influence their paleoenvironmental reconstruction which is based on all, reliable and less reliable, samples. The manuscript could also be clearer in regard to the pollen taxa and their nearest living relatives (NLR) used for the palaeoclimate reconstruction.

Additional comments:

Title The title does not include the single Pleistocene sample.

Introduction The introduction contains too much general “textbook” knowledge along the “Zachos-curve” and needs to be revised with a clearer focus on time periods and questions relevant to the palaeo-record presented in this paper. Essential information on previous more regional (i.e. North America related) climate/vegetation change throughout the Eocene-Miocene is missing. The introduction is in parts wordy and the existing text can be considerably shortened. The authors could also elaborate more on research questions and hypotheses.

Geographical and geological setting This section shortly describes core and coring site, whereas essential information on marine and terrestrial geographical and geological setting (hinterland/coastal plain) is missing. This section needs to be expanded in order to fully understand the interpretation of the pollen record.

Material and Methods 6559, Line 5-10: It must be clearer how the pollen taxa have been assigned to extant botanical groups. 70 pollen types have been identified, but only 54 are listed in table 1. How many and which taxa have been used for estimating palaeoclimates? See also comments to section 3.7 “Quantitative climate reconstructions” 6559, Line 14-15: Please provide total number of pollen and spores, including TPS for non-saccate pollen used for percentage calculation. I would also like to see the complete pollen diagram with all identified taxa (possibly in SI) in addition to the summary diagrams. 6559, Line 16: Is the reference to Fig. 2 correct here?

Transport validation 6559/6560, line 28 and 1: The authors state that they have ex-

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cluded bisaccate pollen from the reference sum on which climatic analyses are based. I do not understand: the climate analysis method normally refers to presence/absence data and should not be linked to the total reference sum or percentage data. Please explain. 65560, line 12: The authors explain a method to identify “real” changes in conifer-forest development from transport-related changes. Could this method be systematically applied to this study in order to identify “unreliable” samples?

Pollen differentiation The authors discuss important aspects of pollen identification in this section. However, I wonder if the information in such detail is relevant to the wider readership of *Climate of the Past*. I would therefore suggest to only focus on those part which are relevant for the understanding of taxa grouping/climate analyses and to move the rest to the Supplementary Information.

Vegetation types/Statistical Methods Both sections contain inconsistencies which confuse the reader/reviewer rather than improving and supporting the interpretation of the pollen record. The authors describe the advantages of the assignment of pollen taxa to vegetation types shown in table 1 (6563, 24), but state in the first line of the next section that this assignment can be arbitrary and therefore apply PCA. The PCA does not necessarily seem to support the previous grouping shown in table 1. This results in inconsistencies in the interpretation section, where the authors group *Artemisia* and *Asteraceae* into “herbaceous taxa indicating deforestation/steppic conditions” whereas Table 1 assigns these taxa to a mesophytic understorey. There are also other inconsistencies in this section between Table 1 and the PCA results which need to be resolved. 6564, Line 15-20: The significance of the “understorey factor” is not clear. But it raises an important question: If *Quercus* could be also part of the understorey and shows a diversification in the Miocene, why do the authors assume that the bioclimatic range remained stable over the last 33 million years? Please clarify.

Quantitative climate reconstructions The use of a quantitative climate reconstruction approach to pollen records spanning the last 33 million years is challenging as it a) assumes that the bioclimatic envelope of plant species remained largely unchanged,

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and b) the uncertainty in pollen identification increases. It has been shown with multi-proxy studies, however, that this approach can provide to some extent reliable temperature estimates. The approach the authors present in this paper could be better documented. Please provide information (in SI) in regard to: a) Which fossil pollen taxa or taxa groups have been used for the climate analysis. b) Which NLRs can be potentially assigned to these fossil pollen groups (normally more than one) and which NLR has been used for the climate estimates.

A good example is shown in Pross et al. (2012). 6565, line 9 – Why did the authors refer to Pross et al. (2012) to identify climate ranges for non-arboreal taxa? Pross et al used the Australian National Herbarium database, which is certainly not ideal for a Northern Hemisphere herb and shrub flora. 6566, line 7-9: Why has *Pinus* and *Podocarpus* pollen been excluded. At least *Pinus* was surely part of the local/regional vegetation as later discussed. Overrepresentation should not play a role when using absence/presence data, as also stated by the authors in 6576, line 4.

Sedimentology/Taphonomy Certainly of high importance for a meaningful palynological interpretation. But the identification of “more reliable” and less reliable samples does not seem to impact on the following palaeoenvironmental interpretation (see also general comments in first paragraph). Quantitative Palynology 6568, 15-16: “verification process” not clear 6570, 2: Where is *Ginkgo* in Table 1? Was it included in the temperature estimates. 6570, 15: What is significant? Provide percentage. 6570, 26 Typo: For all Sites Statistical Analyses and palaeoenvironment Please clarify inconsistencies between grouping shown in Table 1 and PCA groups presented here.

Discussion 6576, Introduction paragraph repetitive, please shorten 6577, 10 -25. I find the approach of labelling selected samples as “reliable” and others as “unreliable” confusing. This makes the entire interpretation questionable and I would recommend that the authors first identify and remove the samples which do not show “real vegetation signals” (6577, line 20). This would make the interpretation and discussion much clearer. 6578, line 25 – What does “probably, partly caused by” mean? The mix of

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too many factors makes the discussion too speculative. 6579, line 1-4. – if increase in large grains such as *Carya*, *Nyssa* etc indicate transport effects, why do the even larger conifer grains remain relatively stable? 6580, line 1-5. These distinct changes occur at a depth where samples from another core have been included. Could problems with the age model have caused this? Please discuss. Pleistocene The one page discussion on a single, isolated Pleistocene sample, with attempts to relate the temperature estimates and vegetation reconstruction to MIC7 or MIC5e, is very speculative.

Further Comparison with global signals and outlook 6584, line 3-8: I am surprised to read about these age model problems in the concluding statements. Has this been mentioned elsewhere in the method and interpretation section? 6584, line 20-30: It would be helpful if the authors could relate the discussed modelling results and hypothesis to their own findings. Such a rapid uplift would have surely altered the conifer percentages and erosion rates.

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