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Climate of the Past Discussions

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 #1

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Overall assessment:

The manuscript presents a new pollen record from marine drillcores of latest Eocene(?) to middle Miocene age recovered by Integrated Ocean Drilling Program (IODP) Site M0027 off the New Jersey continental margin. The authors have augmented these new data with previously published pollen data from IODP Site M0029 and one (1) isolated sample from the Pleistocene with uncertain age. After considering taphonomic effects and concluding that their pollen dataset is partially compromised by the influence of mass wasting or reworking, they utilize all samples analysed in the study to reconstruct Eocene–Miocene palaeoclimate and vegetation change along the East Coast of the United States. While in principle the topic of the study is in line with the

thrust of 'Climate of the Past,' I am concerned that the data presented and the conclusions reached are not of the necessary quality to warrant publication in this journal. Firstly, the manuscript suffers from a lack of focus. There is a tendency throughout the manuscript to embark on a considerable number of digressions which make for a difficult and frustrating read. With regard to this aspect, a very serious rewriting effort would be required to make the manuscript publishable, including a sharpening of the manuscript focus, a general reorganisation of sections, rigorous shortening and substantial rewriting. Irrespective of the shortcomings related to writing style and focus, the manuscript also suffers from serious scientific problems that have direct consequences for the validity and the interpretation of the data. The authors elaborate extensively on the taphonomy of the palynological assemblages without reaching substantial or reproducible conclusions. Ultimately, all samples are incorporated into their palaeoclimatic interpretations, including those previously identified as influenced by mass wasting/reworking. From a scientific point of view, this rationale is quite disconcerting, and I see only one way to potentially salvage the manuscript: To rigorously exclude all samples that the authors consider to suffer from mass wasting/reworking as well as the ones that the authors describe not to record 'real vegetation signals'. At the same time, the extensive musings on taphonomy, which contain only very limited relevant information, must be removed. The outcome will be a lower resolution record than the one presented in the current manuscript, but the remaining data will be more trustworthy.

There are a number of additional problems both with the overall approach and documentation of the methods used in this study. Given the stated uncertainities in the age models, the methodology for correlating between Sites M0027 and M0029 should be described in greater detail. The methods for age dating for all samples should be also be specified. Multiple hiatuses are identified in the previously published age model for Site M0027, which are a result of this site's location in a shallow shelf environment and high-amplitude fluctuations in sea-level during the Oligo-Miocene interval. Regarding this issue, I disagree with the authors' statement that 'the New Jersey Shelf is an ideal research area to study the palaeovegetation and palaeoclimate development in coastal Eastern North America during the Oligocene and particularly during the Miocene' (page 6557, lines 12-14): Considering the strong sea-level dynamics during that time, which have made the New Jersey shelf a textbook example for the effects of sea-level change on sedimentary sequences, I would argue that this setting is in fact poorly suited for any such palynological research. A much better setting would have been further offshore: Even if it had meant an increase in the transport distance of the pollen grains, hiatuses could have been largely (if not completely) avoided, and age control (through calcareous microfossils) could have been expected to be much better. In the notes below I comment on specific parts of the manuscript that require minor to substantial clarifications, corrections or additions.

Title:

The present title lacks the terms 'late Eocene,' 'Oligocene' and 'Miocene.' A potentially more comprehensive title might be "Late Eocene to middle Miocene (33–13 million years ago) vegetation and climate development on the North American coastal plain (IODP Sites M0027 and M0029)."

Abstract:

Page 6553, Line 2: Unclear. What is the aim of this study?

Line 5: Delete the sentence on the isolated Pleistocene sample – this only distracts from the goal of the study. Delete all musings on this isolated sample throughout the manuscript.

Lines 7-9: 'Transport-related ... from the pollen data' – is this worth mentioning in the abstract? Delete.

Line 14: 'in annual temperature' – this holds true for MAT and CMMT. The warming is due to an increase in cold-season temperatures, whereas warm-season temperatures remain more or less constant. I find this reminiscent of Quaternary climate change

(where the temperature differences between glacials and interglacials are also mainly based on changes in cold-season temperatures) and hope to find more on this observation later in the manuscript.

Line 16: 'MEAN annual temperature'

Line 23: 'Surprisingly, ...not show extraordinary changes' – this is a very diffuse statement. What the authors may consider 'not extraordinary' may be 'extraordinary' for others and vice versa.

Introduction:

Although this section is nearly four pages long, it falls short of providing a concise summary of start-of-the-art knowledge of Oligocene–Miocene climate history. In particular, there is no information on the Eocene/Oligocene boundary (although the presented record most likely extends into the Eocene), no mentioning of Oi events (although these and the related sea-level changes are essential for the study) and only superficial information on Mi events. What were the driving mechanisms behind these events, how long did the individual events last, what were the magnitudes of sea-level change, what are the most important references (notably from the New Jersey margin, which is THE classical research area for many of these questions)?

Page 6554, Line 22: Citing just a textbook is not enough – original reference?

Page 6556, Line 2: Do the authors mean 'surface-water temperatures'? Also, delete 'related'.

Page 6556, Lines 18-21: This message is already contained in the previous paragraph (Lines 7-9).

Page 6556, Lines 21-24: If such a scenario were correct, the non-saccate/bisaccate pollen and the terrestrial/marine palynomorph ratios should show a strong, statistically significant correlation. I do not see such a correlation in Fig. 3, which leads me to believe that attempts of identifying a statistically significant relationship between both ratios would be futile as well. In any case, the scenario invoked by the authors is overly simplistic (with a possible reason being that it neglects climatically and oceanographically driven processes). Therefore I consider the approach advocated here to be of little use for the study, and I find it unecessary to repeatedly dwell on such or similarly questionable taphonomical issues and 'solutions' throughout the manuscript.

Page 6556, Lines 25-27: The authors consider it a 'drawback of climate and sea-level reconstructions based on marine palynomorph records' that there is an 'alteration of the palynological record due to differential preservation and transport characteristics of pollen taxa.' I disagree with this statement. What the authors appear to consider a 'drawback' is in fact a prerequisite for any such reconstructions. How would they be able to see different pollen groups being more or less abundant depending on sea level if all the pollen taxa involved had identical transport characteristics? Again, I cannot help questioning the concepts that the authors base their study on.

Page 6557, Line 1: I disagree with the statement that 'sites sufficiently proximal to the coastline to minimize transportation bias' are a good choice for the Oligocene–Miocene time interval. Shallow shelf regions in this interval were subjected to pronounced (i.e., on the order of up to 70 m) sea-level fluctuations, resulting in discontinuous sedimentation histories and (as a consequence of such a dynamic sedimentation regime) are inherently prone to mass wasting. Unfortunately, the record presented in this manuscript supports this view.

Page 6557, Line 6: I fail to understand what is meant here – a better explanation is needed.

Page 6557, Lines 12-14: Again, I strongly disagree with the authors on this point: As (unfortunately) demonstrated later in this manuscript, the New Jersey shelf is by far not 'an ideal research area to study the palaeovegetation and palaeoclimate development in coastal Eastern North America during the Oligocene and particularly the Miocene' as claimed by the authors. Instead, it is rather poorly suited for any such study in light

of the hiatuses and mass wasting that is to be expected in the Oligocene and Miocene. A more distal setting would have yielded a more complete record with a more constant taphonomic bias. I understand that the authors would like to present their study in the brightest light possible, but they should not ignore the problems.

Geographical and geological setting:

I am admittedly surprised that not a single one of the following factors that are essential for the evaluation of marine pollen data is being discussed here: Palaeogeography? Palaeolatitudes? Constraints on source region for pollen? Wind directions? Marine currents? As this information is not given, it does not appear that the authors have considered these factors. Page 6557, Line 26: I do not understand – if the depth is 631 mbsf and the drilled interval is 547 m, what happened to the rest? Do the authors mean 'cored' or 'recovered' instead of 'drilled'?

Material and methods:

Page 6558, Lines 10-20: This needs to be rewritten: First present (in an older to younger fashion) for which parts of the geological column (near-)continuous records are available, then elaborate on hiatuses.

Page 6558, Line 22: Dry weight?

Page 6558, Line 25: Concentration of HF?

Page 6559, Lines 1-3: I find it very unusual to give every little detail on the processing protocol, but then not to state how many palynomorphs were counted per sample. This holds particularly true considering the fact that the authors use very small changes in palynomorph percentages to draw far-reaching conclusions. If the counting sums are low (i.e., below $\sim$300 individuals per sample), the conclusions are strongly weakened.

Page 6559, Lines 6-10: Trivial – delete.

Page 6559, Line 11: Analysed 'with similar methods' by the same analyst? If yes, add this information because it underscores the homogeneity of the taxonomic concepts used. If not, explain how it was determined that the different datasets are consistent. I am stressing this point because the yellow samples in the figure have strongly different values (notably when it comes to the authors' dinocyst/non-saccate pollen ratios). The most straightforward explanation (besides two different analysts having been at work) is that this represents a signal from a spatially and/or temporally different setting!

Page 6559, Line 13: What is the correlation between the different sites based on? Obviously, the exact position of the Fang samples within the record is crucial for the validity of the results. The authors need to show convincingly that such a correlation is possible. This is doubtful in light of the available age model – see also my previous point).

Transport validation:

I find this section of little merit – it is one of the "sideline stories" that the authors tend to get lost in. The entire 'transport validation' issue, while ultimately adding nothing to the study, dilutes strongly what the thrust of the manuscript should be. In addition, it is not truly scientifically sound as it comprises numerous unconvincing, if not dubious statements (see also comments above). For the sake of scientific clarity and correctness, the authors should delete this section in full.

Pollen differentiation:

This is a long section with many taxonomic details. I realize that this is important information, but I wonder if such details should be part of a typical Climate of the Past paper. This extra information makes the main text very long and gives the manuscript a taxonomical twist. I would expect such information to be included as online SI, in which case there should also be plates showing all the pollen types that the authors defined for their study (instead of the highly selective, incomplete mini-plate shown in Fig. 4), plus a rigorous description of all the criteria of all their taxonomical concepts. Alternatively, the taxononmical angle of the current text could also suggest that the

entire manuscript may be better suited for a more specialised palynological journal.

Page 6561, Line 4: 'rich in species' – do the authors mean 'diverse'?

Page 6561, Lines 6-7: No details are given on which cutoff values this differentiation is based on – neither here nor in Section 4.2.1. This makes it impossible to reproduce the results.

Page 6563, Lines 9-10: 'This approach is justifiable ..., and have previously been used for palaeoclimate reconstructions ...' – strictly speaking, this is not a scientifically valid argument. Delete.

Vegetation types:

I do not understand the interpretive strategy taken by the authors. First they establish groups of taxa based on the ecology of the respective nearest living relatives, and in the next section (3.6) they state that this approach 'can be in some case arbitrary', which prompts them to follow yet another approach (i.e., PCA). Why not follow one well reasoned and most applicable concept? A consistent strategy needs to be followed throughout the manuscript. Instead, the discussion is diluted by numerous, partially contradictory digressions. This criticism applies to many parts of the manuscript – here I only point out one of the more prominent examples.

Statistical methods:

Please see general comments above. Page 6564, Lines 13-21: Needs to be condensed considerably.

Quantitative climate reconstructions:

This methodological section is the most convincing part of the entire manuscript – it is scientifically sound and well written.

Page 6566, Lines 7-10: I would argue that the over-representation should not be an issue here because the method is based on presence/absence patterns rather than on percentages (which is again a strong argument against the inclusion of seemingly endless, partially contradictory lecturing on taphonomy in the manuscript).

Sedimentology/taphonomy:

Scientifically, it has remained unclear to me what the benefit of the taphonomy discussion should be – it is quite clear that its deletion would make for a first, important step towards a better manuscript. Also, the authors cite exclusively their own publications when it comes to taphonomy, and I wonder why this is the case.

Page 6567, Lines 3-6: This has been abundantly covered earlier in the manuscript. Please shorten considerably.

General palynology:

It is not clear what the thrust of this section is supposed to be – it represents mostly a mixture of unnecessary information that is partially brought across in a lecturing fashion (such as is the first sentence – please see comment below).

Page 6568, Lines 3-5: Another example of the authors' taphonomy fixation, without relevance for the 'Results' section. Delete.

Page 6568, Line 8: '...may also be characterized by common mass transport deposition' – in other words, the lower Burdigalian does not yield reliable information. If the authors identify considerable mass wasting/reworking across this interval, why do they then continue with an interpretation of palaeoclimate and palaeovegetation history? Do they not realise the consequences of these processes on their own dataset?

Page 6568, Lines 14-16: If this assumption could not be verified, why bother the readers? There is no need to confront the readers with all the assumptions made during the course of the study that then turned out to be wrong.

Page 6568, Lines 24-25: If Pinus was separated into two types, the characteristics/threshold value(s) underlying this separation should be mentioned somewhere in

the manuscript, preferably in a methodology section ('Pollen differentiation') rather than here. I could not find this information anywhere in the manuscript.

Page 6569, Lines 4-6: The authors state that 'In most cases, the relative abundance of foraminifer test linings correlates very well with those of the dinocysts, indicating that the signal of marine vs terrestrial palynomorphs is consistent and can be used as a proxy for site-shoreline distance.' I disagree with the authors on the consequences of this observation, and I find it difficult to follow their logic. This is another disconcerting example of how the authors get lost in taphonomic discussions, thereby compromising the scientific soundness of their study. Also, I fail to see that the relative abundances of foraminifer test linings and dinocysts 'correlate very well' in the first place. If the authors insist on this statement, they would have to substantiate it by means of a simple statistical analysis. What is the correlation coefficient? Is it statistically significant?

Page 6569, Line 10: '... obscuring the normal taphonomic signature' – what is a 'normal taphonomic signature'?

Section 4.2.2:

Page 6569, Lines 16-17: It is quite unorthodox to postulate a 'decrease in marine palynomorphs' based on looking at the percentages of foraminifer test linings and the dinocyst/pollen ratio. Again, the authors apply their own taphonomic concepts. What is gained through these highly debatable measures?

Sections 4.2.3 to 4.2.6:

Page 6570, Lines 6-7: The first sentence needs to be deleted. It does not belong here as it is not a result of the study, and it does not add anything of importance.

Page 6570, Line 7: While it reads '539 m' here, it reads '540 m' in Line 4.

Page 6570, Lines 19-21: In a way, the procedure outlined here is representative of larger problems with the manuscript: The authors focus on this interval because the pollen grains are well preserved and abundant, not because this interval is per se scientifically important or interesting. This is the exact opposite of hypothesis-driven research. Also, the authors point out earlier in the manuscript that the Burdigalian is characterized by mass wasting/reworking. This observation, which is obviously highly critical for the results and their interpretation, does not seem to be considered here.

Page 6571, Lines 2-3: I fail to recognize the logic behind this statement – delete. Also, do the authors refer to percentages here? How about absolute numbers?

Section 4.2.7/Pleistocene:

It makes little sense to portray one (1!) single Pleistocene sample in the context of a study on the late Eocene to middle Miocene. Delete this section completely.

Pollen-based climate reconstructions:

Page 6575, Line 1: Based on what is shown in Fig. 7, the values vary between $\sim$1000 and $\sim$1400 mm, not between $\sim$1100 and $\sim$1250 mm.

Page 6575, Line 10: I had noticed this before, but these sequences (and what they mean) have not been introduced properly.

Page 6575, Lines 22-24: Delete.

Discussion and comparison with other vegetation records:

Page 6576, Lines 1-2: The catchment area has never been characterized for this study – please see also my comments on Section 2.

Page 6576, Lines 7-11: This statement, which is yet another example of taphonomic digressions, describes the fundamental weakness that the entire study suffers from: The pollen record generated by the authors suffers from limitations related to sea-level change, climate change, vegetation change, mass wasting/reworking, and hiatuses. These limitations are partially connected to the shelfal setting of the record. The authors try to disentangle these influences via the application of quite unique, controversial taphonomic concepts, but to no avail.

Page 6576, Line 15: Why do the authors not include other marine (notably SST) data in their comparison?

Section 5.1:

Page 6576, Lines 20-21: According to the authors, 'the lowermost sample ... implies that conifer forests were restricted to mountainous areas during the very late Eocene.' I disagree with this unfounded statement: The (presumably low) percentages of bisaccates do not imply imply that conifer forests were restricted to 'mountainous areas'. It only means that, irrespective of where the conifers grew that produced the conifer pollen, the pollen was not transported to the site of the record. The authors should stay clear of storytelling here.

Page 6577, Lines 1-4: The authors should compare their data with Atlantic SST data compiled in Liu et al. (2009, Science).

Page 6577, Lines 1-12: The authors state that there is a 3°C decrease in MAT, but at the same time they argue that there is little change in the pollen assemblages. If taken at face value, wouldn't this mean a decoupling of vegetation and climate change? The authors need to clarify this issue.

Page 6577, Lines 17-23: Based on the authors' hard-to-follow rationale, there are many samples that yield no 'real' (to quote the authors) vegetation signals. I have long started to wonder why the authors have not excluded rigorously all of these samples (i.e., the ones that have not yielded 'real' vegetation signals)? Why bother the readers with information that is obviously not reliable or even wrong?

Page 6577, Line 24: Where is this piece of information on 'lowland vegetation' from and what is it based on?

Section 5.2:

Page 6578, Lines 8-9 and Lines 19-20: The authors invoke a 'long site-shoreline distance (probably paired with a sea-level high stand)' and further state that 'according to

McCarthy et al. 2013), the shortening in site-shoreline distance was coupled with a fall in sea level.' I am admittedly flummoxed by these notes – which are really just stating the very basic principles of sequence stratigraphy. Why do the authors consider such trivia worth elaborating upon? How can a 'shortening in site-shoreline distance' NOT be coupled with a sea-level fall?

Page 6578, Line 17: So what happened climatically?

Section 5.3:

Page 6579, Lines 4-6: How would a ïĄď13C curve (measured on what? Benthic or surface water?) indicate a 'turn to more humid conditions?'

Page 6579, Lines 13-16: Again, why bother with samples that represent reworking?

Section 5.4:

Page 6580, Line 1: What is this 'congruency' supposed to mean and what is the scientific rationale behind this comparison? I am confident that for most of the temperatures I reconstruct I will be able to find a place on Earth where the same (or at least a similar) temperature prevailed. How is this supposed to help the discussion?

Page 6580, Line 6: The authors state that 'such a decrease is not revealed in records from Europe.' Why should it? I must admit that I am slowly, but steadily getting fatigued by the authors' approach of simply using their results in order to reconfirm observations that have been made elsewhere before. So far, not a single new observation has been presented in this manuscript that has been exploited towards potentially unearthing something substantially novel.

Section 5.5:

Page 6580, Lines 20-23: I would argue that the finding of a low pollen concentration contradicts the scenario of a shortening in site-shoreline distance. Because I find these (again taphonomy-based) interpretations rather arbitrary, I suggest to delete

them. What is the measure of 'pollen concentration' based on? There is no mentioning in the methodological part of the manuscript that pollen concentration values (i.e., number of pollen grains per volume or gram of sediment) have been generated.

Page 6581, Lines 9-18: From a palaeoclimatic perspective, I do not consider this comparison worthwhile (please see also above). This appears not to be based on a hypothesis-driven scientific rationale, but instead seems to be carried out as an end in itself.

Section 5.6:

This section, which is exclusively on the interpretation of one single Pleistocene sample of uncertain age, should be deleted in full.

Section 5.7 – Further comparison with global signals and outlook:

Page 6583, Line 7: This postulated 'shift to less humid conditions' is not at all reflected in the MAP reconstructions that the authors provide in Fig. 7. They suggest a very stable precipitation regime across the E/O boundary.

Page 6584, Line 1: The statement that the global ïĄď18O stack 'should imply particularly warm temperatures' is not correct. The ïĄď18O curves used by the authors are BENTHIC ïĄď18O curves and are only partially a reflection of (high-latitude) temperatures.

Page 6584, Lines 11-15: There is no clear logic in this sentence.

Page 6584, Lines 26-28: Shouldn't this uplift phase also be documented in increased sedimentation rates in the depocentres? Can the authors find any indication for this (i.e., higher sedimentation rates along the margin during that time)? If yes, this would lend higher credibility to the scenario that they propose.

Wouldn't there be other explanations that the authors do not consider? The following issues come to mind:

- The counting sums might be particularly low in the respective interval, which might increase the probability that some of the warm indicators are not recorded (I notice that the authors do not give any information on the counting sums).

- Sea level was particularly high during that time (in line with the ïĄď18O data), which could also lead to particularly pronounced sorting and hence lower pollen diversity, again with the result that warm indicators may not become registered if the counting sums are on the low side.

Section 6 – Conclusions:

Page 6585, Lines 9-11: The authors claim that their 'approach of including marine-palynomorph assemblages into our analyses to identify transport-related bias allows separation of seeming from real shifts in the palaeovegetation...' I strongly disagree with this claim – in fact, the authors' taphonomy-related claims and the quite unique taphonomical concepts employed are not convincing. Instead, they are partially contradictory and difficult to reproduce, thereby weakening the manuscript to an extent that I cannot recommend its publication. As a consequence, the authors should strip their manuscript of the present "taphonomy overload" and only use the taphonomy-based information necessary to discern samples that are compromised by mass wasting/reworking from samples that are not.

Interactive comment on Clim. Past Discuss., 9, 6551, 2013.