



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.

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Dear Editor/Professor Haywood, dear referees,

Thank you very much for your detailed reviews and the decision concerning our manuscript “Vegetation and climate development on the North American Atlantic Coastal Plain from 33 to 13 million years ago”.

We are ready to submit a revised version of the manuscript following most suggestions of the reviewers. Some points raised by the referees have previously been discussed among us internally, e.g., how/whether to incorporate samples from other Sites and from sequences with preservation issues. We appreciate the suggestion to

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decrease the number of ecologically/climatically-interpreted samples and to focus on samples/intervals delivering most reliable data. The admittedly quite strong emphasis on taphonomy in our manuscript can certainly be reduced. The description of the statistical analyses will be removed. The sample from the Pleistocene will be excluded from the dataset as well as the samples from Site M0029. With these changes, the manuscript will be significantly condensed. The focus on certain intervals will certainly also result in a more precise discussion and better-focused conclusions. We will improve the introduction, among other aspects by adding information on the Oi-events. The conclusions concerning the Miocene climatic optimum will be carefully revised.

We discuss below how we plan to incorporate major suggestions. Detailed answers to all points mentioned by the referees will be added to the revised version of the manuscript.

Anonymous Referee #1 Received and published: 31 January 2014

Ref #1: 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,

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including a sharpening of the manuscript focus, a general reorganisation of sections, rigorous shortening and substantial rewriting.

Authors: As described below, we are ready to re-organize the manuscript. The focus in the re-vised version will be on samples from the middle Miocene, the early Miocene and the early Eocene.

Ref #1: 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'. Authors: The idea behind our approach was to make clear which samples may have been particularly affected by taphonomic effects, but to show a record covering as many time intervals as possible. For the same reason, samples from Site M0029 were included. The comments from Refs #1 and #2 and other scientists (who read the discussion article and commented personally) show that this approach is not preferable, and that the discussion and conclusions should rather focus on the samples which are not/to a low degree taphonomically altered. We will do so in the revised version. Ref #1: 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 uncertainties in the age models, the methodology for correlating between Sites M0027 and M0029 should be

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described in greater detail. The methods for age dating for all samples should be also be specified. Authors: We agree that the taphonomy aspect can be shortened. We also have decided to completely exclude the samples from Site M0029 from the presented record, since, as Ref #1 points out, there are uncertainties for the age models of both Sites. We agree that it would be better to have a consistent, trustworthy record for certain intervals instead of having a record with more samples, but which forces the reader to check her-/himself which samples may be problematic. Ref #1: 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.

Authors: Here, we cannot completely agree with Ref #1. We admit that calling the NJ shallow shelf an "ideal" research area during the Oligocene and the Miocene may be too euphemistic. In the revised version, we will state instead that the shallow shelf is well-suited for analyses of certain intervals during the Oligocene and Miocene for which mass wasting and reworking effects can be ignored (see above). Further offshore, as Ref #1 states her-/himself, transport effects, particularly over-representation of bisaccate pollen and under-representation of non-saccate pollen, would significantly

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alter the pollen assemblages.

Referee #1: 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)." Authors: This suggestion is certainly an improvement. However, depending on which samples will be removed following the suggestions of Ref #1, we could come up with a different title, e.g.; "Vegetation and climate development on the North American coastal plain (IODP Exp. 313, Site M0027) during the Eocene/Oligocene and Oligocene/Miocene transitions and during the middle Miocene." Referee #1: 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.

Authors, generally concerning abstract: We will incorporate these points into the abstract of the revised version.

Referee #1: 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

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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 unnecessary 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

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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.

Authors, generally concerning introduction: In several cases, we can understand the criticism of Rev #1. We will add information on the Eocene-Oligocene boundary and the Oi events, and on the mechanisms behind the Mi-events. As discussed below in more detail, we admit that the New Jersey shallow shelf is not perfectly-suited for analyzing the complete Oligocene and Miocene. We will rephrase the related sentences. Focusing on certain intervals in this manuscript is a reasonable approach in this context. Concerning the taphonomy issues discussed in the introduction, obviously some statements made by us need clarifying. For example, concerning the non-saccate/bisaccate pollen and the terrestrial/marine palynomorph ratios, we – by no means – wanted to imply that there should be a significant correlation of both ratios for all samples. But both ratios should show similar trends depending on the site-shoreline distance, and discrepancies in these trends could support the interpretation of the bisaccate-pollen data. Since the emphasis on taphonomy is obviously too strong in the manuscript in general, we will try to reduce texts regarding this aspects, but at the same time state more precisely what is meant and why it is important for the interpretation.

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Referee #1: 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'?

Authors: We agree that more information on palaeogeography, source region, wind and currents would be useful. The section will be improved. Recovery was meant in line 26.

Referee #1: 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 ~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

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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).

Authors: The counting sums were not omitted on purpose, we agree that they should be mentioned. Between 150 and 300 non-saccate pollen grains were counted for most of the samples. With bisaccate pollen grains included, the counting sums vary between 200 to 500 grains, surpassing 300 in most cases. We will add this information and shorten the processing protocol. The samples from Site M0029A will be excluded from the dataset.

Referee #1: 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.

Authors: This section will be removed.

Referee #1: 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 taxonomical angle of the current text could also suggest that the entire manuscript may be better suited for a more

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specialised palynological journal. Referee #1: Page 6561, Line 4: 'rich in species' – do the authors mean 'diverse'? Referee #1: 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. Referee #1: 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.

Authors: The suggestion to move the important content of this section and the photographs (plus additional photographs) in Fig 4. to online supporting information is very reasonable. We will do so.

Referee #1: 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. Referee #1: Statistical methods: Please see general comments above. Page 6564, Lines 13-21: Needs to be condensed considerably.

Authors concerning both sections: We will remove the statistical methods part completely, particularly since the results will be partly obsolete after removing samples from certain intervals as suggested by both reviewers.

Referee #1: Quantitative climate reconstructions: This methodological section is the most convincing part of the entire manuscript – it is scientifically sound and well written. Referee #1: 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

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seemingly endless, partially contradictory lecturing on taphonomy in the manuscript).

Authors: It is common practice in the analysis of terrestrial pollen sums in marine sediments to exclude *Pinus* and some other Pinaceae bisaccates from the climatic analysis (e.g., Eldrett et al. 2009, 2014), for 2 reasons: (1) *Pinus* and *Picea* are known to show increasing abundance with distance from shore in shelfal sediments and primarily reflects distant rather than adjacent lowland coastal vegetation due to these grains well known ability to be transported in significant numbers 1000s of km (Mudie, 1982; Hooghiemstra, 1988), and (2) *Pinus* unless identified to subgenera or species groups is not climatically informative as this large genus is found today across North America (and the Northern Hemisphere) in almost every climate capable of supporting woody plant cover (e.g., Thompson et al., 1999).

Referee #1: 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. Referee #1: Page 6567, Lines 3-6: This has been abundantly covered earlier in the manuscript. Please shorten considerably.

Authors: Depending on the general changes to the manuscript, we will either shorten this section or completely remove it, adding aspects relevant to the publication to other sections. While we do not agree with Ref #1 completely, we still follow her/his arguments and think that instead of mentioning the taphonomy aspect several times in the manuscript plus granting it a complete section, it would be sufficient to discuss taphonomical issues at specific points in the results. Concerning the citations, the McCarthy-et-al. publications are from the research area (in case of McCarthy et al., 2013, even from the same Sites!) and thus occur more often, but other authors (Hooghiemstra et al., 1988, Lacourse et al., 2003) have also been cited concerning taphonomy.

Referee #1: General palynology: It is not clear what the thrust of this section is sup-

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posed 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'?

Authors: We will probably remove this section completely. However, we regard some information in this section as necessary, but this can be incorporated into other sec-

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tions. Concerning “mass-waste-influenced” samples, we think a possible approach would be to include such samples in the pollen diagram figure, but to exclude them from the climate dataset and the interpretation.

Referee #1: 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?

Authors: Obviously, this section needs rephrasing to state more precisely what is meant. We will do so.

Referee #1: 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. Referee #1: Page 6570, Line 7: While it reads ‘539 m’ here, it reads ‘540 m’ in Line 4. Referee #1: 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. Referee #1: 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?

Authors: We will remove detailed descriptions and (see below) interpretations of intervals/sequences which show strong signs of mass wasting/reworking. The revised manuscript shall focus on those intervals which yield robust results.

Referee #1: 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.

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Delete this section completely.

Authors: We will do so.

Referee #1: Pollen-based climate reconstructions: Page 6575, Line 1: Based on what is shown in Fig. 7, the values vary between _1000 and _1400 mm, not between _1100 and _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?

Authors: The approach suggested by the referees to focus on certain intervals will surely improve section 5. As written above, we admit that the shelfal setting has disadvantages, but some intervals yield robust results.

Referee #1: 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 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

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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?

Authors: We agree that this sections needs clarification. We will rephrase parts of this section accordingly.

Referee #1: 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?

Authors: Of course, site-shoreline distance and sea-level/water depth are generally coupled, but it is thinkable and possible that only one parameter changes significantly. In case of a very steep slope, even several meters of sea-level fall may leave the site-shoreline distance virtually unchanged, and vice versa, in case of a very shallow slope, only a few meters of sea-level increase may result in a significantly greater site-shoreline distance.

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Referee #1: Section 5.3: Page 6579, Lines 4-6: How would a $\delta^{13}\text{C}$ 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.

Authors: We will rephrase parts of these sections from the discussion. Referee #1 shows clearly that several parts of the text need clarifying. Of course, the idea behind the manuscript is not to reconfirm observations of other authors. One aspect that we perhaps do not emphasize enough is that, even though the record from the shallow shelf does not give a completely hiatus-free Oligocene/Miocene climate record, still can be used as a showcase into different intervals from the Oligocene and Miocene at the same place, while all terrestrial records from Eastern North America, to which we

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compare our results, are much more fragmentary and only reflect the conditions during very short intervals.

Referee #1: Section 5.6: This section, which is exclusively on the interpretation of one single Pleistocene sample of uncertain age, should be deleted in full.

Authors: We will do so.

Referee #1: 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 $\delta^{18}\text{O}$ stack 'should imply particularly warm temperatures' is not correct. The $\delta^{18}\text{O}$ curves used by the authors are BENTHIC $\delta^{18}\text{O}$ 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 $\delta^{18}\text{O}$ 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.

Authors: We admit that Ref #1 points to some aspects to consider for the interpretation of the pollen record. This section obviously needs some clarification and additional citations concerning the Miocene climate optimum. Concerning the counting sums:

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these are always between 195 and >300 non-saccate grains in the respective interval (and significantly higher if bisaccate grains are included, see above).

Referee #1: Section 6 – Conclusions: Page 6585, Lines 9-11: The authors claim that their 'approach of including marinepalynomorph 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 taphonomybased information necessary to discern samples that are compromised by mass wasting/reworking from samples that are not.

Authors: As stated above, we will follow the suggestion to remove the "taphonomy overload" and furthermore clarify the cases where taphonomy-related explanations are difficult to reproduce.

Anonymous Referee #2 Received and published: 9 February 2014

Referee #2: 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 reliable" (6567, line 20) for paleoenvironmental reconstructions

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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.

Authors: There is obviously general agreement between both reviewers that certain samples (Pleistocene, intervals with indication of mast waste) should be removed from the interpretation and palaeoclimate/-ecology reconstructions. We will remove such samples in addition to samples from Site M0029.

Referee #2: Additional comments: Referee #2: Title The title does not include the single Pleistocene sample. Referee #2: 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.

Authors: We will improve the introduction accordingly (see above).

Referee #2: 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.

Authors: We will improve the introduction accordingly (see above).

Referee #2: 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

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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?

Authors: We will clarify these points in the revised version.

Referee #2: Transport validation 6559/6560, line 28 and 1: The authors state that they have excluded 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?

Authors: See related answers to Ref #1.

Referee #2: 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.

Authors: Will be done. See related answers to Ref #1.

Referee #2: 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

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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 understory. 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 understory 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.

Authors: As stated above, the PCA approach will probably be removed from the revised version. With the Pleistocene sample and further samples removed, and the general dominance of certain pollen types (e.g., oak, hickory), the results of a new analyses would probably result in even statistically relevant groups. The vegetation types, with some clarifying, will probably be of more use for the reader.

Referee #2: 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, and b) the uncertainty in pollen identification increases. It has been shown with multiproxy 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

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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.

Authors: It is common in the analysis of terrestrial pollen sums in marine sediments to exclude *Pinus* and some other Pinaceae bisaccates from the climatic analysis (e.g., Eldrett et al. 2009, 2014), for 2 reasons: (1) *Pinus* and *Picea* are known to show increasing abundance with distance from shore in shelfal sediments and primarily reflects distant rather than adjacent lowland coastal vegetation due to these grains well known ability to be transported in significant numbers 1000s of km (Mudie, 1982; Hooghiemstra 1988), and (2) *Pinus* unless identified to subgenera or species groups is not climatically informative as this large genus is found today across North America (and the Northern Hemisphere) in almost every climate capable of supporting woody plant cover (e.g., Thompson et al., 1999). Regarding *Podocarpus*, there is considerable uncertainty as to the botanical affinity of these grains, as noted in our manuscript (p. 10, lines 26-28, p. 11, lines 1-4). Grains assigned to the stratigraphic palynomorph *Podocarpidites* (i.e., ‘*Podocarpus*’) may for some species correspond to members of the *Podocarpaceae* but not necessarily the living genus *Podocarpus* (Greenwood et al., 2013), with several members of *Podocarpidites* considered by US-based palynologists to represent *Pinus* and not *Podocarpus* or related genera in the *Podocarpaceae* (Nichols & Brown, 1992, cited in our ms). Furthermore, Greenwood et al. (2013, also cited in our ms) identified UK Eocene foliage as the related genus *Prumnopitys*, and not *Podocarpus*; both of these extant genera produce pollen that corresponds to the ‘*Podocarpus* / *Podocarpidites*’ type. We therefore took the conservative position of not including ‘*Podocarpus*’ in our climatic analysis, a point we had made in the submitted ms (p. 10, lines 26-28, p. 11, lines 1-4), but we will make this point more explicitly in the revised ms. Regarding our citing of Pross et al. (2012) as sharing with our study the same sources of NLR climate range / profile data, non-arboreal taxa such as the fern families *Gleicheniaceae* and *Schizaceae* (e.g., *Lygodium*) are geographically widespread in Australasia, so the Australian National Herbarium (which has data

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from New Guinea and other Pacific Islands), combined with the New Zealand data set from Reichgelt et al. (2013) provides a valid data set for these primarily Southern Hemisphere taxa. However, the exact statement in our text was : "...the online database of Natural Resources Canada (2012) for non-trees, supplemented by data from sources outlined in Pross et al. (2012) and Reichgelt et al. (2013)", i.e, we used multiple sources for non-arboreal taxa, emphasizing the North American dataset derived from Natural Resources Canada, as we state ms p. 15, lines 7-9, and data generated (as stated in Pross et al. 2012) from the Global Biological Information Facility online database that collates data from all of the world from the national herbaria and similar institutional sources of consortium countries (e.g., Anemia in Schizeaceae to supplement the Australasian data for the family as some grains represent this genus, and not Lygodium). We acknowledge, however, that our 'shorthand' reference to our data sources caused confusion and will therefore re-draft this statement to better clarify and explain the sources of our NLR climate range data, including listing the fossil paly-nomorphs and their NLRs, and the sources of the climate range data for each NLR as a table in our supplementary materials. Furthermore, we are in the process of re-running the climatic analysis for the reduced set of samples in order to focus our data sources primarily on published data sets (e.g., Thompson et al., 1999, 2012; Fang et al., 2011), as used in the recently accepted paper 'A seasonality trigger for carbon injection at the Paleocene-Eocene thermal maximum' for Climate of the Past by Eldrett et al..

Referee #2: 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

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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 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. Authors: Several of the major points above will be obsolete due to the general changes (removing of certain sections and samples).

Referee #2: 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.

Authors: We will remove everything related to this sample from the record.

Referee #2: 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.

Authors: We will see if this hypothesis can be supported by our results and also by sedimentological data.

A. M. Haywood (Editor) Received and published: 14 February 2014

Editor: This paper has received two reviews from scientists with a great deal of exper-

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tise in the subject. Whilst it seems the paper has potential both reviewers have highlighted a considerable number of shortcomings that need to be thoroughly addressed before publication is possible. Given the extensive nature of the changes required a revised paper will need to be sent out for external peer review.

Authors: As discussed above, we would like to follow most of the referees' suggestion in a revised (and significantly shortened!) version of the manuscript. We accept that the revised paper will need to be sent for external peer review.

References (not included in original manuscript) Eldrett J.S., Greenwood D.R., Polling M., Brinkhuis H., Sluijs A.: A seasonality trigger for carbon injection at the Paleocene-Eocene thermal maximum. *Climate of the Past*, x: xxx – xxx. (accepted Feb. 24, 2014; amended from and replaces earlier draft in *Clim. Past. Discussions* in 2013), 2014.

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Hooghiemstra, H.: Palynological records from Northwest African marine sediments: a general outline of the interpretation of the pollen signal. *Phil. Trans. Roy. Soc. Lond. Ser. B, Biol. Scis.* 318 (1191), 431-449, 1988.

Mudie P.J.: Pollen distribution in recent marine sediments, eastern Canada. *Canadian Journal of Earth Sciences*, 19(4): 729-747, doi: 10.1139/e82-062, 1982.

Please also note the supplement to this comment:

<http://www.clim-past-discuss.net/9/C3600/2014/cpd-9-C3600-2014-supplement.pdf>

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

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