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Interactive comment

Interactive comment on "Periodic input of dust over the Eastern Carpathians during the Holocene linked with Saharan desertification and human impact" *by* Jack Longman et al.

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Interactive comment on "Periodic input of dust over the Eastern Carpathians during the Holocene linked with Saharan desertification and human impact" by Jack Longman et al. Anonymous Referee #1 Received and published: 24 February 2017 The manuscript presents profiles of major elements and paleoecological indicators from an ombrotrophic peat bog in Romania. The paleoclimate records are associated to mineral dust and the discussion is focused around possible interpretations that try to disentangle local from distal signals. The authors interpret the history of the site in function of a superposition of changes on diverse spatial scales, from that of local hydrology to the large scale / hemispheric patterns derived from Greenland ice cores or

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North Atlantic marine sediment records. The topic is of interest to the paleoclimate community. The work appears well structured and its presentation in the manuscript is generally clear. I think that a better discussion of uncertainties would improve the manuscript.

R1: We thank the reviewer for their kind words, and constructive review.

General comment My comment about improving the discussion of uncertainties is articulated in two parts, that have to do with (1) the quantification of the dust flux and (2) the attribution of potential sources. (1) For the purpose of estimating the dust flux, only the concentrations of Ti from ICPOES are used, although a semi-quantitative comparison is carried out against three major elements counts from XRF. Other studies trying to estimate dust from peat bog records discussed the uncertainties related to this issue (e.g. Marx et al., 2009; Kylander et al., 2016). Please discuss more in detail these aspects, and if possible provide some estimates of the uncertainties.

R2: This is a good point, and I order to address the reviewer's comments, we have included a summary outlining the uncertainties within the updated manuscript, lines 209-219: "It must be noted here that using Ti alone in dust flux calculations does not allow for reconstruction of all minerals related to dust deposition. Ti, which is lithogenic and conservative, is a major component in soil dust, particularly within clay minerals (Shotyk et al., 2002), but may not be associated with other dust-forming minerals, including phosphates, plagioclase and silicates (Kylander et al., 2016), although our records of K and Si may help indicate changes in deposition rates of these minerals (See Mayewski and Maasch, 2006). As a result, we are unable to infer specific mineral-related changes in the composition of dust. However, Ti alone will record changes in the intensity of deposition of the main dust-forming minerals (Sharifi et al., 2015; Shotyk et al., 2002), and variations in K and Si (particularly with local K- and Si-rich dacites a possible dust source) may further indicate the influx of minerals which are not associated with Ti. Such an approach has been applied successfully to studies of changing dust influx (e.g. Allan et al., 2013; Sapkota et al., 2007; Sharifi et al., 2015),

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with each study able to identify periods of high and low dust deposition from Ti-derived dust flux alone."

(2) The attribution of potential sources is based on a simple analysis of correlations between major elements counts, as discussed by the authors, and is also supported by the interpretation of the evidence from indicators such as testate amoeba and pollen. Nonetheless, it would only be by looking at dust size distributions at the same time, that one could derive more firm conclusions about distal versus local contributions to the dust budget (e.g. Mahowald et al., 2014). If possible, include this kind of data, otherwise please discuss this aspect in the text.

R3: We attempted to address this point via grain size analysis on a Malvern Mastersizer 2000. Unfortunately, due to limited sample material, and the very high organic matter content, obscuration values on the laser particle counter were too low for reliable results to be presented. As Kylander et al. (2016) explain, this is a recurring issue with dust reconstruction within peat cores. The manuscript has been updated to reflect these attempts between lines 164-169: 2.10 Grain Size "In an effort to indicate distal versus local inputs to the bog via the dust particle size, grain size analysis was attempted using a Malvern Mastersizer 2000. Unfortunately, as also observed in previous studies (Kylander et al., 2016) due to the lack of available sample material, and low minerogenic matter (and correspondingly high organic matter) present in the samples, satisfactory obscuration values were not achieved for most analyses."

Specific comments p. 3, 103-106. Please describe how the cores were packed and stored in the phase between recovery and analyses.

R4: Adjustment made to lines 105-107: "Upon recovery, the material was wrapped in clingfilm, transported to the laboratory, described, imaged, and subjected to further analyses. The core was stored at 3°C"

p. 4, 120-121. Later in the text (line 179) you also mention different detection limits for the different elements. Please provide all this kind of information in the same place in the text, and try to mark it visually in the plots.

R5: The value of 50cps is not a detection limit, it is merely guidance for the interpretation of the results. Indeed, studies have been presented displaying variations not related to noise in Mg analysis, where typical cps values are as low as 5-10 (Dulski et al., 2015). With our data it is clear some fluctuations below 50cps are not just noise, as they are echoed in all three elements, and even within the ICP-OES analysis. Regardless, since we have now normalised the data with respect to the scattering effect of organic material, this sentence no longer applies to our data and has been removed.

Figures 3 & 4. I think a slight confusion can arise because of the way some of the records are organized between these two figures. For instance, it would be useful to see in Figure 3 the Ti concentration profile from ICP-OES along with the major elements counts from XRF, rather than directly the dust flux which weights in the sedimentation rate and bulk density profiles. On the other hand it would be nice to have all the factors defining the dust profile in the same figure, i.e. Ti concentration profile from ICP-OES, sedimentation rate and bulk density profiles.

R6: Figure 3 has been updated, with all the factors defining the dust flux; raw ICP-OES values, density and sedimentation rate, displayed on one figure. As such, density has been removed from Figure 4. p. 4, 126. What is the depth span of each sample? i.e. was the full core analyzes, or just portions of it? Lines 127-128 have been updated to indicate each sample was 1cm3 and that the entire core was analysed: "ICP-OES analysis was carried out on 105 samples of 1cm3 of sediment, roughly every 10 cm, through the entire core."

p. 5, 160. Harmonize with what you say at line 118. p. 5, 175. What do you mean by significant? Did you apply some statistical test? Otherwise perhaps change with "visible".

R7: The designation "significant" has now been removed. Additionally, we now clarify the reasoning behind the denotation of dust events in lines 183-187: "Such zones are identified as an increase in two or more of the elements above the background deposition (K > 0.001, Si > 0.001 and Ti > 0.004, see dashed line on figure 3). These intervals are further discussed as reflecting major dust deposition events, and are referenced in the remainder of the text using the denotation D01-D10 (Fig. 3). Two exceptions, at the base of the core, close to the transition from lake to bog, and the last 1000 years, due to high noise, are not highlighted."

p. 6, 190. Please discuss the uncertainties in estimating the dust flux.

R8: These have been outlined earlier in the response, and the discussion has been added to the text (lines 209-219)

p. 6, 208-211. It is not clear at this point what is the contribution of this kind of analysis to the work.

R9: Since we do not use the magnetic susceptibility values within our interpretation, these data have been removed from the manuscript, and from figure 4.

p. 7, 238. The dust "Dn" events are selected based on the occurrences of at least one of the elements form the XRF scan, so it is a bit weird to go through the text until this point with some apparent inconsistency between the discussion of peaks D0 to D3, which are not evident and sometimes in anti-phase the what you call dust record. Please either change your definition of dust event (D) or harmonize the text.

R10: In light of comments from reviewer 2, we have removed D0 from our interpretation of dust events, since it may be simply related to minerotrophy. For events D1-D3, as outlined, we have identified them via one of the elements increasing. We agree some confusion may arise with the interpretation of the 'dust record', which is generally based on the Ti-derived dust flux. As such, we have amended the manuscript on lines 366 & 408, replacing "dust record", with "Ti-derived dust record" When we are considering the results of more than simply the dust flux, we retain the use of the term "dust record".

p. 7, 244-245. As you discuss below, there is not a correlation, so please rephrase with something like “we compare the timing of the identified dust prepositional events with periods ...” Adjustment made to lines 265-266:

“Firstly, it is noteworthy that five of the identified dust depositional events may be compared to periods of Rapid Climate Change (RCC)” p. 7, 260. “Appear to indicate”: maybe it would be better “are consistent with”, in relation to my comment about the missing information on dust particle size distributions. Please include this kind of data if possible, otherwise add a discussion paragraph about this issue.

R11:“Appear to indicate” has been replaced with “are consistent with” on line 284. As outlined earlier in this response, and in the manuscript lines 166-169, particle size variations could not be obtained from this highly organic record.

Figure 5. Check the units of the upper curves (the two cores from McGee et al., 2013): I think you reported the values with the wrong units, which I think are g/cm2/kyr in the original publication, so there is a factor ten difference. In fact, on this scale you have the same dust flux, if not lower, in the North African plume and Belgium or Romania, which would be weird.

R12: This has been checked, and the reviewer is correct- Figure 5 has been revised accordingly

p. 8, 266-268. It is not very clear what is the relation between these two studies, please rephrase.

R13: To clarify these studies, their relationship, and locations, the manuscript has been adapted in lines 289-292: “Saharan dust in Atlantic marine cores strongly increased at this time, with a 140% rise roughly at 5500 cal yr BP observed on the western Saharan margin (Adkins et al., 2006) with another study indicating a rise by a factor of 5 by 4900 cal yr BP in a selection of similarly-located sites (McGee et al., 2013).”

p. 8, 279-289. Again, particle size distributions would help clarify these issues.

R14: We tend to agree particle size data would help the interpretation, but unfortunately we have been unable to attain it.

p. 8, 291-298. Earlier in the text you mentioned the different mobility of these major elements, and how the similarity of their profiles supports the ombrotrophic nature of the peat bog. Please clarify how this is coherent with your analysis here, which is instead based on the difference between the same elements.

R15: These comparisons are not made with regards to any discussion on ombrotrophy. We maintain the similarity of the records, as evidenced by the strong correlations is indicative of the ombrotrophy of the site. However, since the elemental records are not perfectly correlated, and there are periods within which one of the elements is more enriched relative to others, we believe this approach is valid for distinguishing the geochemical signature of the events.

p. 9, 310-313. It would be interesting to compare Type 2 events with the background signature of non-D periods.

R16: This is an interesting idea, and we have implemented it, with results indicating background peat deposition is similar in makeup to Type 2 events- further hinting at the local nature of such deposition. The manuscript has been updated to include these data in lines 339-342: "The local nature of such deposition is emphasised by the similarity the depositional signal to background values; the elemental composition outside of dust events. For all data points not considered to be related to dust (or the minerotrophic lowermost section), the Ti-K regression is low (r2= 0.1513) with a gradient of 0.0863."

p. 9, 314. "Fig. 8" should probably be "Fig. 6", please check.

R17: The reviewer is correct; this has been corrected on line 343

p. 12, 429. Was the data interpolated somehow before performing wavelet analysis? What is the pace / temporal resolution of the time series fed to the wavelet analysis software?

R18: Before performing wavelet analysis, we used Gaussian interpolation with a timestep of 4 years and a window of 12 years. This is included in lines 161-163: "For this analysis, the lithogenic normalised elemental data from ITRAX measurements (Ti, K, and Si) was interpolated to equal time steps of four years using a Gaussian window of 12 years."

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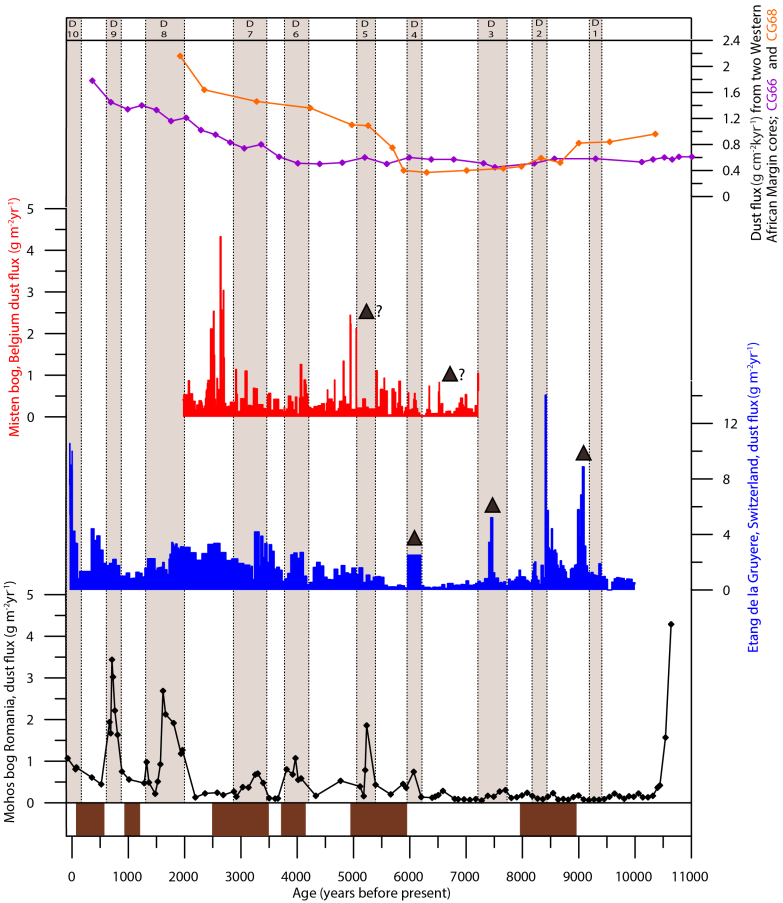

Fig. 1. Updated version of Figure 5

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