**Responses to reviewer #1:**

Please find our responses to the review below. Original review is displayed in black, our responses in blue.

The study is motivated by the question, how much phosphorus is supplied to the Amazon by North African dust sources. The authors examine the source regions for winter time dust storms (as this is the season when dust is transported towards the Amazon) for three consecutive years (2015-2017). Geomorphological characteristics of the source region were determined for the ten strongest dust storms per winter season. The manuscript is well written and I enjoyed reading it. Nevertheless, I have a few questions I would like the authors to address before publication

Response: We would like to thank the reviewer for the assessment of the paper and for the constructive feedback and helpful questions.

General comments: (1) The question I am most curious to know the answer remains somewhat unanswered: Can you estimate how much phosphorus is supplied to the Amazon by the individual dust source regions? E.g., Schepanski et al., Atmos. Chem. Phys. (2009) examined by means of a model simulation the contribution of dust emitted from the Bodélé Depression to the atmospheric dust burden over the Gulf of Guinea and tropical Atlantic showing a contribution of up to 60% over the Equator region. But it remains unclear, how much dust passes through the tropical rain belt. I think it may be worth considering adding a trajectory analysis here showing how many dusty trajectories originating from an emitting dust source actually reach the Amazon without being affected by rainfall or clouds.

Response: We fully agree with the reviewer that this question of 'how much phosphorus is supplied to the Amazon by the individual dust source regions?' is an important one which needs answering. In order to do so, however, two extra key pieces of information are needed. The first is how much phosphorus is in the dust derived from the different types of sources that we identify, the second is how much of this dust reaches the Amazon (i.e. trajectory analysis, as mentioned).

All studies on the (fertilisation) effect of dust transport to the Amazon employ the phosphorus concentration of dust from the Bodélé Depression paleolake sediments; we do not have any information on how much phosphorus is in dust from other sources at present (e.g. other paleolakes or the paleorivers we have identified). The results from this paper show that the Bodélé is not representative of the diversity of surfaces producing dust, and so more data is needed on the phosphorus concentration in the different dust sources before this question can be answered accurately. We have sampled dust from various paleoriver and paleolake systems throughout much of Northern Africa, which we are currently analysing to get a better understanding of the phosphorus concentration in Northern African dust from the individual sources in different regions. This research will be published in a follow up paper that builds on the research presented in this paper.

With regards to the trajectory analysis we again agree with the reviewer – this is a good idea. To achieve this, we are currently developing a new remote sensing trajectory analysis methodology using MODIS AOD to track dust from source to sink. This will form the basis of another follow up paper.

While we are striving to answer the questions raised by the reviewer in due course, by carrying out extensive fieldwork, lab work, and developing and testing this new remote sensing methodology for dust tracking, this involves a considerable amount of research which will be published in papers that follow on from this one. If all this research were to be combined it would form a huge paper, with multiple methods and conclusions that would bury some of the important findings.

(2) Related to question (1), how many of the identified dust sources actually contribute to the dust deposition flux over the Amazon? This can be addressed by a trajectory study as well and would be a worthy contribution to the scientific discussion on the fertilization impact of northern African dust sources

Response: This follows on from the previous question. We agree this would be a very worthy contribution to the scientific discussion, however as we would like to tackle this issue using a new remote sensing methodology in order to answer the question most accurately, we believe this would be better suited for a follow-up manuscript, to eventually be combined with our geochemical analysis of dust samples that we have collected from paleorivers and paleolakes throughout northern Africa.

(3) Was the dust mass per source region calculated for all identified dust storms originating from the corresponding location or for a selected number only? This refers to line 18-20, page 7. Please clarify. Same for the numbers provided in the results section: Are these based on all identified dust storms? Which quantities are put in relation?

Response: The dust mass per source region was calculated for all identified storms. Upon reflection this was not clearly explained (it was also mentioned by reviewer #2), and therefore we have revised the methodology section. It is now clearly stated that the selected number of dust storms (20 largest) only pertains to identifying the location of dust point sources and the classification of their geomorphology (sections 2.2 and 2.3). This has also been better clarified in the results/figures/tables.

(4) Can you please explain in more detail how the dust mass fractions were calculated (refers to line 9-12, page 9)? Providing the fraction implies you know the annual total - or at least did some assumption. Which mass fractions are related? Please clarify.

Response: The dust mass fraction is calculated as the ratio between the dust mass emitted during the 10 largest dust storms per season and the total dust mass emitted during the wintertime seasons. The total Tg emitted during the 2015/2016 and 2016/2017 winter dust seasons (Dec/Jan/Feb) was 82.3 - 127.0, which can be found in Table 2. The ten largest dust storms of both seasons emitted 30.95-47.75 Tg. This is how the 37.6% on line 12, page 9 (original manuscript) was calculated. As it was not clear that the values in Table 2 corresponded to all dust storms in the season, this has been clarified in both methodology and in the Table title. As per suggestion of reviewer #2, we have now split up this figure per season.

p 5, l 15-16 It is absolutely reasonable to limit the calculation to the ten strongest dust storms. Can you add a brief statement on the representativeness of the results despite this selection is made?

Response: Only with regards to the exact location of individual dust point sources and their geomorphology (methodology section 2.2 and 2.3) did we limit our methodology to the ten strongest dust storms. All other analyses were carried out for all dust storms in the seasons. Due to time constraints and labour-intensive methodology of identifying point sources and classifying

geomorphology per point source we decided to limit to the ten largest dust storms per season. The representativeness of this is shown in the accuracy assessment, and the results of this can be found at lines 9-12, page 9 of the original manuscript. Nevertheless, it seems this could do with better explanation; as such we have combined these sections in a new paragraph to make it clearer (last paragraph in section 2.3 of revised manuscript).

p 7, l 9-14 Please consider referring to a map illustrating the location of the listed source regions.

Response: The map presented (in the results section of the original manuscript) illustrates the location of the listed source regions; on reflection this map was indeed better placed in the methodology section. We have moved the map and added a sentence referring to the map.

p 13, l 6 There are studies highlighting the Sudan as active dust source. E.g. Schepanski et a, (Geophy. Res. Lett. 2007, J. Geophys. Res. 2009), and Formenti et al., (Atmos. Chem. Phys., 2011).

Response: We thank the reviewer for bringing this to our attention - this was indeed an error. There are a couple of studies referring to Sudan, however these are nearly all pertaining to northern Sudan/Nubian desert. As you mention, Schepanski (2009) is the exception in naming central Sudan. We have updated this sentence to reflect we are referring to central/southern Sudan and have added Schepanski (2009).