

## ***Interactive comment on “Mineral dust effects on clouds and rainfall in the West African Sahel” by L. Klüser and T. Holzer-Popp***

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Received and published: 26 April 2010

We wish to thank the anonymous referee #1 for the careful reading of the discussion paper and for his good suggestions.

Overall response: Regarding most of the major points, we will restructure the manuscript in the revision step and will also include a third satellite dataset of contemporary cloud and aerosol observations from ENVISAT. As the aerosol retrieval SYNAER from ENVISAT is capable of a real aerosol type separation, the information gained will strongly strengthen the interpretation of the results.

Specific response to major remarks:

1 We fully agree with the reviewer that the word rainfall in the title is misleading and  
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moreover that the suggested warm rain likelihood is not a measure of rainfall intensity or precipitation quantification. Thus we will concentrate on the cloud properties and remove statements about rainfall from the revised manuscript.

2 There are several studies in the literature which describe similar effects as suggested in our manuscript, but only from either case studies or modelling. We therefore do not intent to propose new and not yet published effects of aerosols on clouds. We want to give statistical evidence for the presence of such effects and to present an overall estimate of the bulk indirect mineral dust effects in the Sahel from a large dataset. We will add a more detailed overview of what is already described in the scientific literature and put appropriate citations in the revision of the manuscript.

3 Again the reviewer is right, and thus we will add dust AOD and cloud property observations from ENVISAT to the revised manuscript in order to increase the database of observations. Regarding the remarks on BMDI, we will summarise the method description and evaluation published in Klüser and Schepanski (2009). In this paper also cross-comparisons with MODIS were presented, that is why we did not put them here again. (We will discuss the differences of sensitivity to airborne dust between both metrics in more detail as they are absolutely necessary to understand the presented results, as the reviewer correctly remarked). As a bi-temporal algorithm the BMDI exploits the variation between night and day observations of MSG and thus by definition is obtained only once daily (see Klüser and Schepanski, 2009 for details). Thus it is (at least so far) not possible to exploit the full temporal resolution of SEVIRI with this method and provide daily cycle results.

4 This remark is a point we thought a lot about. Of course the reviewer is right that vertically integrated water vapour does not provide a full characterisation of the air mass. Back-trajectory analysis is not only not feasible for such a large dataset as used here (daily observations over five years in large areas) due to computational limitations, we also doubt if it really improves the results. Neither aerosol observations nor clouds are height resolved. From the structure of the three dimensional circulation in the

Sahel, it follows that height information would be absolutely necessary to trace the right air mass with such an analysis method. Although WVC is not really a physical air mass separator, BMDI is, as all IR dust indices, very sensitive to this property (Klüser and Schepanski, 2009, and references herein). Moreover this paper showed that WVC is quite robust in separating the moist monsoonal air mass from the dry air mass of the Sahara, when the total lower tropospheric column is regarded. We will add some more detailed discussion on this topic and also some explanatory figures to the revised manuscript (see e.g. WVC analyses in Klüser and Schepanski, 2009, which are the basis of the decision to use WVC as air mass separator).

Response to specific remarks:

p 6169, l 17: The reviewer is right, “enhancing” is somewhat misleading in this context. “early initiating” is a better wording.

l 25+: We agree that the description of the data and the dust filtering used in this study should appear in the “data and methods” section.

p 6170, l 18: OK

l 24: OK

p 6171, l 2: Thank you for the hint; it seems that we somehow lost them during the manuscript preparation.

l 11: Thank you very much, we will correct this error.

l 11: BMDI is an infrared dust index like e.g. the IDDI, indicating the presence and partly the infrared optical depth of mineral dust in the atmosphere at the time(s) of observation. We will add an explanation statement to the revised manuscript.

l 28: “heavy” means high dust loads, indicated either by high AOD or low BMDI values. We will add an explanation.

l 28: “observations of dust” means airborne dust indicated by BMDI. We will specify

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this in more detail in the revision of the manuscript. The original retrieval resolution is the SEVIRI pixel resolution ( $3 \times 3 \text{ km}^2$  at nadir). We will also add this information.

p 6172, l 2: OK

l 14: See response to major remark three and the detailed description in Klüser and Schepanski (2009). We will clarify this here in the revised manuscript.

l 15: see response to p 6170, l 24

l 19: We agree with the reviewer and will give a better formulation including the phrase “may lead to”.

l 20: We have tried this but as indicated in Fig. 1b the amount of precipitation during noon time is very small. Thus, although clouds are already present, they do not produce much precipitation, which leads to very small sample sizes of rainfall observations. Thus we see hardly any effect, which we can trust from a statistical point of view, as the bulk of the noon time cloud observations is without precipitation regardless of the aerosol observations. As indicated above we will remove the word “rainfall” from the title as this is not the quantity in the focus of this study. We will reformulate this passage appropriately.

l 22: We will explain the word “seasons” with the meaning of monsoon onset and retreat.

p 6175, l 4: We will change it appropriately.

l 5: We agree with the reviewer and will substitute the liquid phase cloud cover by total cloud cover, as we are also interested in effects on mixed phase and ice phase clouds.

l 10: We have no real indication for deep convection and thus will remove it from the revised manuscript, then talking only about low level, mid level and high level clouds (as indicated by cloud top temperature).

l 13: See response to l 10 above

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I 14: We meant that the histogram becomes bimodal for heavy dust whereas the high contribution of high level clouds for the background scenes do not form a distinct secondary peak in the histogram. But of course the overall contribution to the histogram is higher for the “no dust” (background) case. We will clarify this in the revised manuscript.

I 27: We will substitute “monsoon flow” by “moist flow”. This description also characterises the separator of the air mass (water vapour) better than the wording “monsoon flow”. Given the response to major remark 4, meteorological re-analyses, although being very valuable for determining the three dimensional structure of the atmospheric circulation in the region, are not used here in this study focussing totally on satellite observations.

p 6176, I 6: OK

I 11: OK

I 13: As indicated in the response to major remark 1, we will remove the rainfall and warm rain likelihood. Thus, as the reviewer is totally right, this interpretation also will vanish and the only effect we report and interpret from the satellite observations is the reduction of the effective radius.

I 16: see comment directly above

p 6179, I 16: There are several studies in the literature suggesting such an effect (e.g. Feingold et al., 1999, J. Atm. Sci., amongst others). We will add appropriate citations here to indicate that it is not our theory but that there have been observations of such an effect.

I 21: Again, there are several case studies including aircraft campaigns, satellite observations and numerical modelling suggesting such an effect being present in nature (e.g. Kaufman and Fraser, 1997, amongst many others) We will add more citations to appropriate studies which showed the stabilisation effect in several cases. We do not intend to postulate new effects but we only want to show that the effects described by

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other authors from case studies are also present in a large scale statistical analysis of satellite data. Thus we will add a more detailed description of literature on this effect.

I 22: As we do not show evidence for the drying sometimes reported in the literature (e.g. J. Small et al., GRL, 2009), we will delete this statement from the revised manuscript.

p 6180, I 1: As already indicated, we will add analyses of data from another satellite (ENVISAT) to the revised manuscript, which will help to interpret the results in more detail. We will also add a more detailed discussion on the differences between the results from the (then) three satellite datasets which partly account for the observed differences. Nevertheless we will replace the wording “strong evidence” in this context, agreeing to the reviewers comment.

I 19: see response to Tab. 1 comment below.

I 27: OK

p 6181, I 4: Unfortunately not, as BMDI works best with the chosen observation times and needs the contrast between observations at night and at day. A detailed description of the reasons for the choice of the used observation times (03:00 UTC and 12:00 UTC) is presented in Klüser and Schepanski (2009). We will add a clarification to the revised manuscript.

I 14: We will use the wording “semi-indirect effects” as suggested by the reviewer.

I 21: See comments to rainfall and WRL above, we will remove this from the revised manuscript.

p 6182, I 19: Unfortunately, as almost all satellite aerosol studies, we do not have height resolved aerosol observations. Thus we can only interpret the statistical analysis as showing evidence for this being the fact. We will add an appropriate clarification to the revised manuscript.

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l 27: From this kind of statistical analysis we cannot present error bars of the effects. To overcome this shortage we will include ENVISAT observations into the revised manuscript. With three satellite datasets, two of which use dust AOD as a measure for mineral dust presence, we can present at least an indication of the reliability of the statistical results. As already indicated above, we will provide a more detailed discussion of the reasons for the differences, of the limitations of the respective datasets and also of common effects seen from the different satellites.

p 6183: We will be happy to provide a more detailed discussion including an additional dataset of satellite observations, which will much strengthen the revisions made in response to the remarks of the reviewer.

p 6184, l 27: OK

p 6185, l 4: OK

p 6186f: see response to p 6171, l 2

p 6188: We will add a more detailed discussion including the additional dataset from ENVISAT. As indicated above, we will remove the WRL analysis from the revised manuscript, as it does not reflect rainfall intensity and thus is quite misleading. Regarding the last comment, there are counteracting effects. We have the thermodynamic effect leading to higher cloud tops on the one hand and the stabilisation of the atmospheric layer (see response to p 6179, l 21) suppressing convection on the other hand. Decreasing cloud top height can be regarded as evidence for the latter effect being the stronger one. We will clarify this in more detail in the reformulated discussion after the inclusion of the ENVISAT dataset.

p 6189: See response to p 6188.

p 6190: See several responses above.

p 6192: We will substitute the figure with respect to the reviewer's comments to clarify what the different seasonal fields are. As rainfall is not in the primary focus of the

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study, we will remove (b). We will add clarifications to the figure to the description in the revised manuscript.

p 6193: The reviewer is right; both dust observations are not perfectly correlated. There are several reasons described in detail in Klüser and Schepanski (2009). There also the detailed comparison between both metrics of dust frequency of occurrence is presented. We will add some clarifications on this point to the revised manuscript.

p 6195: We will provide this information in the updated version of the manuscript.

p 6196: We agree with the reviewer that the interpretation of the effective radius is not very simple. In the revised manuscript we will not focus on the effects on droplet sizes (as they are not available from APOLLO) but will use this information to interpret the observations with respect to cloud cover and cloud top temperature. One major goal of this study is not to focus on liquid water clouds as is done very often in the literature but on the total cloud fields (see response to comment p 6175, l 5) and especially the dust effect on cloud cover and cloud top temperature.

p 6197 : W will change it appropriately (see response to comment p 6175, l 5 and to comment p 6196).

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 6167, 2010.

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