Atmos. Chem. Phys. Discuss., 10, C1815–C1822, 2010
www.atmos-chem-phys-discuss.net/10/C1815/2010/

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

Anonymous Referee #1

Received and published: 19 April 2010

Klüser and Holzer-Popp present a study of statistical correlations of aerosol and cloud properties. This method is currently very popular in the literature, but – as the authors rightly acknowledge – interpretation of the results in terms of cause-effect relationships is difficult. This study is in several aspects interesting, in particular because it uses data from various satellite instruments, and because it separates airmasses and – to some extent – aerosol types. Its topic fits well into the scope of Atmos. Chem. Phys. In general, the manuscript is well written. However, at some instances, it seems to be written somewhat hastily (e.g., the missing or unordered references), and at several instances, restructuring would substantially enhance readability.
The study merits some further work. I suggest a few more substantial modifications, and several minor ones.

Major remarks

1. The study refers in its title, abstract and conclusions to "rainfall". But this is unfortunately not investigated. The frequency of occurrence of MODIS-retrieved cloud-top droplet effective radii larger than 14 μm is not a sufficient metric to infer statements about precipitation. Also for microphysical effects, it is hard to identify aerosol effects on droplet number vs. correlation with liquid water path. An analysis of this readily available product should be added.
2. The results are unfortunately rather inconclusive. If indeed the correlations of aerosol concentration with cloud cover and cloud-top temperatures reflect aerosol-cloud effects, then these are in contradiction to previous findings and postulations, and would need to be understood at a more fundamental level. Corroborating these findings would indeed be very interesting and a novel finding.
3. The two dust metrics, namely MODIS "Dust AOD" and the "BMDI" so far stand quite separately in the study. Rather than doing so, and having to explain substantially different findings when using one or the other metric, it would be useful to directly compare the two quantities. It is also a pity that the power of the geostationary satellite to provide data at high time resolution is not exploited.
4. The airmass characterisations should be done in a more robust way than just by sorting by vertically integrated water vapour. Rather, easy-to-use tools such as backtrajectory analysis could be used.

Specific remarks
p6169
l17 What means "enhancing" here?
l25+: The placement of this description is strange. If you decide to keep it, it would be

necessary to clearly explain what is actually used in this study, and how this is done. It would be good to show total AOD and the dust contribution to understand how this algorithm works.

p6170
l18: It would be good to specify that TRMM data are only used in a very minor scope in this study. Otherwise the wrong impression may be given that indeed precipitation retrievals play a substantial role in this study.
l24: This statement about "sufficiently high" convective intensity is contradictory to the remark ion p6172 l15 about the little precipitation.

p6171
l8: The Kriebel references are missing in the literature list.
l11: The reference seems to be Pavolonis and Heidinger (2004).
l11: What about "mineral dust" is obtained? The frequency of occurrence?
l28: Please specify what means "heavy" here.
l28: What exactly means "observations of [. . .] dust"? The dust index? What is the original retrieval resolution which is aggregated to 0.5°?

p6172
l2: explain the acronym WVC here.
l14: It seems surprising here that SEVIRI observations are taken at one time during the day only. A clear explanation is needed why the diurnal cycle is not resolved, and why this particular time is chosen.
l15: see above for page 6170 l24
l19: "Reduced precipitation" is a wrong term here. Maybe the precipitation formation rate is delayed, but at a larger scale, precipitation is constrained only by evaporation and perhaps long-range transport.
l20: What is the problem with using the observation around noon? Isn't this the time of

the day you choose anyway?
l22: The terminology of "seasons" is used here for the first time. It would be good to explain what is meant.

p6175
l4: here and elsewhere: the term "significant" should be used only if indeed the statistical significance has been tested adequately
l5: Is this mid-level cloud fraction liquid? Otherwise, how would this statement agree with the use of liquid-water cloud cover?
l10: how are "deep convective" clouds defined?
l13: What is the difference between "high-level" and "deep convective" clouds?
l14: this statement seems to hold only when comparing "moderate" and "heavy" dust, but "no" dust has yet larger contribution of low cloud top temperatures.
l27: "monsoon flow" and "monsoon season" seem an inconvenient choice of terminology. I suggest choosing for either one another description. Is there a good way to identify monsoon situations, e.g. by analysing meteorological re-analyses?

p6176
l6: the wet seasons
l11: "drastically" seems overly dramatic here
l13: this is a very vague interpretation, and Rosenfeld's interpretations have been widely discussed in the literature. I suggest to either drop the interpretation as for autoconversion/ precipitation formation; or – what would be much better yet – to analyse using the TRMM data available to you to which extent this interpretation is valid. It is important to note that the MODIS Science Team retrievals of cloud-top droplet effective radii tend to be very large compared to other satellite products (e.g., Doutriaux-Boucher and Bréon, IEEE, doi:10.1109/TGRS.2005.852838, 2005). This further complicates such an interpretation as attempted here.
l16: This statement should be confined to the situations where it actually holds.

p6179
l16: this is a very speculative interpretation and would need substantiation to be publishable.
l21: This speculation about potential processes allowing to interpret the results is of fundamental importance to this study. If this interpretation can be shown to be valid and robust, then a potentially important mode of aerosol-cloud interactions would be identified and described. However, some additional effort is needed to show this. What is the reason for this stabilisation? Solely absorption of sunlight by the dust in elevated levels? If so, this effect should be quantified and analysed.
l22: Why would air entrained into the clouds be particularly dry? Usually the air surrounding the cloud should have relatively large relative humidities. Evidence to support this interpretation is needed.

p6180
l1: "strong evidence" is not only an exaggeration, but simply wrong looking at Table 3 (decrease in cloud cover, and little effect on the cloud-top droplet radii).
l19: I don't agree (see discussion of Table 1)
l27: How does this statement agree with what you said earlier (p6179, l 22)?

p6181
l4: There is an urgent need to explain why not the same time of day has been chosen from SEVIRI. It seems obvious that this statement here could easily be tested with the data you have available.
l14: In the literature, "thermodynamic effects" usually refer to the delay in the release of latent heat of freezing for smaller droplet sizes under aerosol influence. Here, I suppose "semi-direct effects" are meant.
l21: As stated earlier, this has not been shown.

p6182
l19: This seems important for the interpretation. Are the dust and cloud layers in your study interacting at all?
l27: Which differences would you expect, i.e., what are the error bars on your findings?

p6183
I think it is too early to in detail criticise the conclusions before the above-mentioned remarks on the study have been answered.

p6184
l27: Ignatov

p6185
l4: and
please order the publications alphabetically!

p6186
the Kriebel and Saunders (1989) and Kriebel et al. (1989; 2003) references are missing

p6187
The Thulet et al. (2007) reference is missing

p6188
It would be interesting to discuss why the effects get stronger with increasing dust index for SEVIRI, while the opposite is true for MODIS Dust AOD.
If δ_{WRL} really reflects precipitation, why is then cloud cover decreasing (i.e., an inverse lifetime effect? How this?)?
Thermodynamic effects would lead to higher cloud tops. Why is the opposite found here?

p6189
Interestingly, here (and also in Tab. 4; for "heavy dust" according to the MODIS product shown here) the effects are consistent with the conceptual model of cloud lifetime and thermodynamic effects, in contrast to Tab. 1. Why?

p6190
The qualitative and quantitative differences between the two satellite products in this case need to be discussed.

p6192
(a): Which time is the field averaged over? It would be important to see the geographical distributions for the individual seasons and flow regimes. It is important to note that there is a very strong gradient in the geographical distribution which hampers the interpretation. Is the field less heterogeneous for the individual cases you selected?
(b): please multiply the y-axis by 8 to get to the commonly used mm/day.

p6193
The MODIS Dust AOD and the SEVIRI BMDI seem in parts of this smoothed time series uncorrelated. It would be very valuable to compare these two metrics of dust frequency of occurrence. I suggest a joint histogram could be done for the three cases no – moderate – high dust for the two methods. If they agree on this relatively coarse metric, which is widely applied in this study, more or less only the diagonal should be populated.

p6195
It would be good to indicate the absolute number of observations going into the analysis of each of the four cases. Is the monsoon season defined as in Fig. 2, and "dry season" the rest of the year?

p6196
The analysis of the effective radius is quite difficult to interpret since it convolves potential correlations with/effects on droplet number concentration and on liquid water path. If an analysis of either of these two quantities (which are readily available) would be added, the interpretation would be much easier.

p6197
The analysis in (a) should be done consistently with Fig. 3, either by choosing here also liquid cloud cover, or by investigating total cloud cover in Fig. 3, too.

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