Answer to reviews of ACP-2014-688 « Positive feedback of dust aerosol via its impact on atmospheric stability during dust storms in the Eastern Mediterranean »

Dear editor, dear reviewers,

Many thanks for the constructive comments and suggestions of the two reviews which led, hopefully, to an improvement and a clarification of this manuscript. Please note that, to answer concerns raised by the reviewers, the title of the paper has been changed to « Feedbacks of dust and boundary layer meteorology during a dust storm in the Eastern Mediterranean ». Please find below the answers to comments by referee #1 and #2. The manuscript has been modified accordingly.

Best regards,
The authors

General comment by Anonymous Referee #1 :

> There are the questions I still have after reading the paper in full two times:
>
> 1/ Is it necessary to simulate dust and its interactions with climate to have an accurate forecast in the tropics and subtopics? Is the influence of dust local (in space) and short in time or is it synoptically of great importance and can last for a season?
>
> 2/ How does the influence of dust for the two short periods and at the two sites (Tamanrasset and Cairo) relate to the broad synoptic weather pattern of the subtropics.
>
> 3/ Given the problems encountered with the optical properties entered in the original manuscript, a good documentation of the ones used in the paper is needed. You should draw the refractive index used for these experiments and compute the radiative efficiency for both SW and LW over the region of interest. Note that Yu et al. 2006 (see Tables 13 through 17) have published values of dust radiative efficiencies for clear sky (SW) that you could put in regards to your results.

To answer your first point : this paper is not about the response of the climate to aerosol radiative forcings, but about short-term radiative impacts of aerosols : the time scale in this study is from a few hours to a few days at most. This study could provide insights to climate impacts of aerosols, but it is not its primary goal.

To address your second point : spring is the period of the year when dust storms are most frequent in the Sahara. As such, this study is representative of the short-term interactions between dust and boundary layer meteorology during an intense dust storm. Again, though, it would be hazardous to extrapolate the results of this study to larger time and space scales (i.e. general synoptic patterns). Other studies, such as the work of Reale et al. (2010) specifically aim to answer this issue. It is however clear from this study (and others such as Perez et al 2006, Heinold et al. 2008 among others), that the important short-term impacts of aerosols on boundary layer meteorology should in turn impact synoptic circulation. This is however just a guess and not supported by evidence brought by our work.

For your third point : the optical properties of the three dust bins have been added (Figure 1), as computed with the refractive indices used in Woodward et al. (2001) and Fouquart et al. (1987). This allows comparison with several studies, notably Perez et al. 2006 and Spyrou et al. 2013.The section dealing with the radiative impacts of aerosols have been substantially expanded, with two more figures showing the radiative impact of aerosols at surface and at TOA, in the long-wave and in the short-wave, and showing the radiative efficiency at surface and TOA, in the long- and in the short-wave. This allowed fruitful comparisons to several earlier studies, and confirmation that the radiative impact of dust computed by the MACC-II system are most of the time consistent with results obtained earlier. In particular, a comparison to the observations of radiative efficiency by Yu et al. (2006) has been made.

Since the paper has many sections, the summaries of results that are included at the end of these sections are very useful. I recommend that the sections 9 and 10 be merged together since they both summarize the main findings from this study.

These two sections have been merged together, thank you for the useful suggestion.

Sections 4 and the first half of section 5 are not well written and need re-writing. Overall most sections would gain by having and English-speaker try to write them in a more concise form and extract the most important messages. Therefore I think that the authors should spend time improving the text from this manuscript.

All the sections have been reviewed and many corrections have been made.

Minor points :

Page 3, lines 138 through 145: when you cite Perez et al. (2006) and Miller et al. (2014) you could have more centered your comments on how these papers relate to your study and summarize their main results that are very relevant to this work

This paragraph have been expanded to describe the results of Miller et al. (2004), Perez et al. (2006) and Heinold et al. (2008), and to relate this work to their results (and specify our objectives).

Page 3, lines 205 to 209: the optical properties were not obtained by E. Highwood and S; Woodward, please trace from which publications these were taken as it is important to document how absorbing dust is.

The correct references were found and added to the text. A paragraph have been added to describe in more detail the two aerosol models that are available in the MACC-II system, and how the optical properties of dust is computed.

Page 4 , line 307 and 308: when you mention wind intensities, for how long were these intensities averaged? I suppose that these are not instantaneous winds. Please provide the information in order for the reader to know if the peak wind intensities have been smooth out by a time average.

The observations are 10mn-averaged wind speeds (as is usual in the SYNOP network) while for the model, since the time step is 15mn, wind speeds can be taken as 15mn-averaged. This information has been added at the beginning of section 5, where observations and simulations of wind speeds are compared.

Page 5 line 424: ‘slightly effected’ is not proper English, did you mean ‘slightly affected’?

Corrected, thank you.

Page 6, section 5, first and second paragraphs need to be re-written, please focus on what we lean about the influence of dust on the forecasts.

This section has been modified in-depth.

Page 6, line 575 and all along the paper: please replace ‘W’ with ‘West’; ‘E’ with ‘East’ when referring to the position relative to a location.

Corrected, thank you.

Page 7, lines 593 to 595: I did not understand how you reached the following conclusion: ‘’ They are less impacted by SW as compared to surface wind speeds, which shows that synoptic factors are less important than the local factors to explain the lower wind speeds of SW.’’

This sentence has been changed since it was not clear. What was meant was that the the impact of the short-wave forcing of aerosols on winds at 925hPa is smaller than on surface winds. Since 925 hPa winds are more representative of general circulation than surface winds, this indicates that the general circulation was not that much affected by the short-wave radiative impacts of aerosols in this case.

Page 7 line 685 : replace ‘translatesinto’ with ‘translates into’.

Page 8 line 706 : replace ‘Most this layer lies…’ with ‘Most of this layer lies…’

Starting page 9 and all along the rest of the text : replace ‘0UTC’ with ’00 UTC’
‘3UTC’ with ‘3 UTC’
‘12UTC’ with ’12 UTC’
‘15YTC’ with ’15 UTC’

Page 10 line 901 : replace ‘TOTAl’ with ‘TOTAL’

Page 10 line 938 : replace ‘rue’ with ‘true’

Page 10 line 959 : replace ‘fist’ with ‘first’

Done, thank you.

Page 10, lines 979-891 : ‘’ This sections aims to assess whether using prognostic aerosols to compute the aerosol radiative effect is beneficial for the forecasts of these two parameters.’’
It is unclear to me which two parameters you are referring to.

It was 2m temperature and AOD. This section has been modified and now only 2m temperature is evaluated against observations ; this sentence and others referring to the evaluation have been corrected accordingly.

General comment by Anonymous Referee #2 :

- With regards to the feedbacks upon dust emission: the main feedback identified and analyzed in the paper is the one that relates the decrease in emission to a reduction of eddy mixing within the boundary layer. This feedback was identified and extensively studied in Miller et al. (2004) and Pérez et al. (2006). Negative radiative forcing at the surface reduces the flux of sensible heat from the ground that powers eddy mixing in the arid regions that are dust sources. The dependence of wind speed upon surface forcing predicts that dust emission should increase in models with positive forcing (Miller et al. 2004). Other studies have shown that dust radiative forcing alters the regional distribution of surface pressure, increasing mobilization where the surface wind is increased (Heinold et al. 2008; Ahn et al. 2007).

What I believe it represents a novelty in the paper is the identification and interpretation of the secondary feedback related to the changes in the horizontal gradient of temperatures.

This should be highlighted in the paper. The original references need to be properly cited and described in the introduction and during the discussion of the results and conclusions. A better job needs to be done to distinguish when the paper is confirming previous results or identifying a new mechanism.

- The first study showing an improvement in short-term weather forecasts by introducing online dust radiative effects is Pérez et al. (2006).

Our work builds on the work of mainly Perez et al. (2006) and also Heinold et al. (2008) and Stannelle et al. (2010). The novelty here are threefold :

- The decomposition of the dust-atmospheric stability feedback (already described in Miller et al. (2004) and Perez et al. (2006)) into the sum of a long-wave and short-wave component. Even if the short-wave component is overall predominant, the long-wave impact is significant (and improves the 2m temperature scores with assimilation runs),
- The dust-thermal wind feedback, as mentioned in your general comment,
- Simulations with data assimilation, which confirm and amplify conclusions reached with cycling forecast runs (as mentioned also in your general comment).

Thanks to your comments, this has been made clearer to the authors, and in turn we have tried to highlight this in the manuscript. The title has been changed, and the relative weight of the sections has also been reviewed, so that mechanisms already described earlier are less prominent in our work. We also mentioned more often and in a more complete way how this work relates to the earlier work, of Perez et al. (2006), Miller et al. (2004) among others.

Specific comments :

> Abstract:
> Please identify more clearly the two mechanisms. The first mechanism confirms previous studies. The second is more of a novelty and should be highlighted. The results concerning the assimilation runs are another novelty and should be highlighted as well.

The abstract was modified to give more emphasis the the novel parts of this study.

> Section 1.1
>
> Lines 73 to 90: I feel this paragraph is both confusing and inaccurate. Are the numbers reported related to surface forcing? Aren't those numbers related to radiative forcing at TOA?
>
> More generally, why do the authors emphasize on the climate effect when the focus is to study short-tem effects. Dust short-term effects and climate effects at the surface substantially differ (and this is why I consider the previous paragraphs as confusing). A dust event interrupts the daily cycle of solar heating by dimming the surface. Over land, the ground cools with the passage of the dust layer overhead, reducing the upward transfer of heat by the longwave and sensible fluxes. But the surface air temperature is quickly restored to its unperturbed value following the passage of the dust layer. On time scales longer than a few days, the atmospheric temperature adjusts to the dust forcing at TOA and the perturbed energy exchange at the lateral margins of the dust layer (please see Miller et al., 2014). In regions of frequent vertical mixing, forcing at TOA is an especially strong constraint upon temperature at the surface.

This paragraph has been removed from the manuscript.
More generally, most mentions to climate models and climate effects have been removed, and the fact that the focus of our study is short-term radiative effects of aerosols. Our time scales is only a few hours to 1-2 days at most (the more so because the meteorological initial conditions are reset to « aerosol free » conditions every 24h), so that the dust forcing at TOA was studied in less detail. We however added more plots about TOA forcing (and net atmospheric forcing).

> Lines 91 to 111: As I explained above, proper references and a specific description of the results of these references on the specific focus of the paper are missing.

This paragraph has also been removed as it was not really consistent with the focus of the paper (which has been reinforced in a later paragraph)

> Lines 138 to 145: The studies cited in this paragraph do not look at mineral dust impact on climate. They rather look at short-term effects (weather time scales). This should be reconciled with my previous comments.

This was corrected, thank you. This paragraph has been expanded to summarize the results of these studies, and how this work relates to them.

Line 183: there is no wet deposition in the model?

There is (and it was added in this sentence). Scavenging was ignored in this sentence because it is a negligible sink for this case study. This was mentioned in a later section.

Line 200: objectives instead of objective

Corrected, thank you.

Last paragraph: Is there any reference for the WGNE model inter-comparison?

There is no peer-reviewed reference yet. I added a reference to the conference paper by Saulo Freitas, describing the intercomparison and its first results.

Line 461 to 466: I do not necessarily agree with this statement. If the decrease in LW downwards between the two periods was due to dust, why does the REF simulation (with a constant monthly dust climatology) show the decrease in LW as well?

This is right, this part has been corrected.

Lines 483 to 485 (and lines 510 to 514 in section 5): You don't need to follow the internal model convention to plot the results. If you do so, you should also describe the convention in the captions of the figures.

We prefered to follow conventions used at ECMWF. They have been added to the captions of the corresponding figures.

Lines 619 to 633: The interpretation of the secondary feedback (through the thermal wind equation) is not vey clear. In what do you base that the gradient of surface temperature is the predominant factor? Have you quantified it? Changes in pressure gradient seem to follow the same pattern in Fig 9.

This paragraph has been partially rewritten so as to try to make it clearer. The impact can be quantified through the thermal wind equation (the result has been added in the paper). The impact of the surface pressure difference was also quantified and found to be locally significant. The impact of thermal stratification is significant, but plays in an opposite direction : decreasing wind speed instead of increasing it.

Line 938: typo ("true")

Corrected, thank you.

Line 955: Please avoid the expression "it seems that". Nevertheless, I believe that the interpretation is correct concerning the differences between TOTAL_ASSIM and TOTAL with respect to surface temperature and winds.

This has been corrected, thank you.

Line 1009: missing end of sentence.

Completed, thank you.

Sections 9 and 10:
Please wrap up by highlighting the new results compared to previous studies.

Sections 9 and 10 have been merged, keeping most of section 9 and highlighting the new results.