Review of "**Remote sensing and modelling analysis of the extreme dust storm hitting Middle East and Eastern Mediterranean in September 2015**"

Specific recommendations have been made by striking through text to be removed and additions are in red.

Scientific significance = 2 (1 if more were made of the importance of land surface changes with time and the quantitative impact of such changes on atmospheric dust loads.)

Scientific quality = 2

Presentation quality = 3 (Mainly based on difficulty of interpretation of figures)

Does the paper address relevant scientific questions within the scope of ACP? Yes

Does the paper present novel concepts, ideas, tools or data? Yes

Are substantial conclusions reached? Yes

Are the scientific methods and assumptions valid and clearly outlined? Mostly yes.

Are the results sufficient to support the interpretations and conclusions? Yes

Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes

Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

Does the title clearly reflect the contents of the paper? Yes

Does the abstract provide a concise and complete summary? Yes

Is the overall presentation well structured and clear? Yes (some changes to figures required)

Is the language fluent and precise? Mostly yes

Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Not all, see below.

Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Yes, see below.

Are the number and quality of references appropriate? Yes

Is the amount and quality of supplementary material appropriate? N/A

General comments

This work is an interesting analysis of a dust storm that was generated over Iraq, Syria and Turkey and then produced a dust plume over the Eastern Mediterranean. The authors approach seems robust and the analysis of the meteorology leading to this dust event is in line with both literature and what I would expect from briefly looking into this particular case. It is generally well written and the figures show what the authors describe.

I think more could be made of the conclusion that changes to land surface over relatively short timeframes can be very important for specific dust events (and presumably the overall dust load). Especially with respect to the known interannual variability of particular dust sources such as ephemeral lakes and fluvial deposits from flooding.

Specific comments

45-51 Description of cold pool/haboob production could be made clearer. e.g.

"~~The responsible mechanism for haboob formation is the generation of a cold pool of ambient air due to evaporative cooling. The rain and ice condensates evaporate (or melt) as they fall through the warmer and unsaturated air and the absorption of latent heat from the phase changes leads in a vigor cooling of the surrounding air.~~ Haboobs are formed by the evaporation (and melting) of hydrometeors as they fall through warm, unsaturated air below the cloud base of convective clouds. The energy required for these phase changes (latent heat) generates cooled downdrafts. When the downdrafts hit the surface they spread out due to their enhanced density compared with the ambient air. ~~When these~~ The convective outflow boundaries are turbulent and gusty and when they travel over bare soil and desert areas ~~they result in the generation of~~ sediment can be lifted, creating a propagating dust wall."

66 Not all cited works relate to the Atlas mountains remove "as an aftermath of Atlas Mountains convective storms."

125-131 I think section 2.1.3 would benefit from a bit more detail. In particular the production and limitations of the SEVIRI RGB dust images. I would like to direct the authors to the work of Banks & Brindley (2013) (http://dx.doi.org/10.1016/j.rse.2012.07.017) as an evaluation and description of the RGB SEVIRI dust images. Particularly the sensitivity to atmospheric moisture and dust height which is relevant to the interpretation of the SEVIRI dust images when dust is raised at different times and by different processes (recently lifted haboob dust less clearly distinguishable due to high atmospheric water vapour and high stability keeping dust close to the surface). This would be useful to add to the discussion in lines 225-227.

143 I am not familiar with the RAMS model but I suspect the levels are terrain following close to the surface but relax to be smooth and parallel in the upper levels. A little more detail would be useful here.

157 and figure 1. I think it would be useful to mark on the locations of the radiosonde launch stations on to Figure 1b. Also the frequency of the launches.

160-162 Did you use any different data for initialisation as part of your sensitivity studies? Roberts et al.(2014) (doi:10.1002/2013JD020667) and Schepanski et al. (2015) doi:10.1002/qj.2453 both show that over West/North Africa the data used for initialisation has a much larger impact on the resultant simulations than other factors such as model resolution, boundary layer set-up or microphysical schemes.

196-197 You mention the combination of cold air aloft and low level warming leading to a thermal low. I think it would be better to discuss and even show the 1000-700 hPa thickness and either mean sea level pressure or 925 hpa geopotential height to identify the formation of a thermal low.

238-243 I don't agree with this conclusion borne from Figure 7b that the temperature depression between the rain drops and the ambient air is the "crucial parameter". This is only a single factor that is likely to lead to high evaporation rates and therefore a strong cold pool. Arguably more important is the sub-saturation and depth of the below-cloud air. If below-cloud air is close to saturation and is shallow then regardless of the raindrop-ambient air temperature depression a strong cold pool will not be formed e.g. the patches over Turkey and Syria with similar values but no cold pool. The quantity of water held as hydrometeors is also important. Please amend to make it clear that the situation is more complex.

251-252 “This latency between satellite and modeled haboob fronts is an indication that the convective downdrafts were in fact stronger.” Or could this also be attributed to a triggering delay due to the imperfect modelling of the boundary layer or the stability/moisture profile making conditions for triggering less favourable than in reality. Regardless of cold pool strength these factors could produce later triggering of convection and a latency in the storm progression compared with satellite imagery.

316-321 It looks like these are thermally driven downslope/upslope winds caused by preferential cooling/heating of the land surface compared to the surrounding sea.

327-329 There are many factors that could (and likely are) responsible. I think you should include a few more of them here e.g. fall speeds, limitations due to dust emission size bins, transport effects due to imperfect modelling etc.

Technical corrections

I feel that that work would have benefitted from being proof read by someone who is a native English speaker. There are occasions when slight errors disrupt the flow of the text. I have highlighted errors that I have seen below.

For example.

42-43 “These systems are well known by local populations ~~at~~ in desert and arid areas worldwide due to their devastating impact ~~in~~ on human health”

64 “A variety of studies on haboobs ~~has~~ have been performed worldwide.”

Make sure acronyms are always defined where they first appear in the text.

For example.

55 “SAMUM 1 & 2” is not defined. SAharan Mineral dUst experiMent.

56 “MODIS”

105 “CALIOP” CALIPSO is defined but not CALIOP

Use “led” not “lead” throughout e.g. 66 and 89.

Wherever UTC time is used it would be useful for interpretation to include local time (LT) in brackets afterwards.

Be consistent with use of AOD or AOT, they are interchangeable.

53 “Moreover, haboobs are usually generated over remote …”

58 “It is also worth ~~to~~ mentioning …”

71 “synergy” I don’t really like this term here. I’m not sure the effect is greater than the sum of the individual parts. If you are talking about specific positive feedbacks then be specific. If not you can just remove “and synergy”

114 “km ~~analysis~~ and vertical resolution…”

144 “dust ~~production~~ emission scheme”

187-188 “the combination ~~between~~ of two distinct meteorological features ~~in the greater area~~:”

202 “extended bare soil areas in Syria (Figure 2).”

213 “~~As seen in~~ Figure 4~~,~~ shows the convergence…”

221 “(plume_2) ~~also~~ was detected…”

223 “the approach ~~approaching~~ of the…”

233-234 “circulation and ~~as~~ is shown in Figure 6b. ~~it~~ It is characterized…”

234 “Somali~~a~~”

259 “Figures ~~8~~ 9 and ~~9~~ 10”

272 “bel~~l~~ow”

281 “observations again suffer~~s~~ ~~again~~ from total…”

296 “~~Libanon~~ Lebanon”

367 “regarding the forecast skill~~s~~ of the atmospheric…”

368 “such extreme episodes are ~~very~~ seldom, they still…”

370 “atmosphere are ~~nowadays~~ now often adequately…”

374 “systems for dust episodes in West Africa.”

375 “the complexity of these events makes ~~their forecast~~ forecasting them very …”

Figures

Figure 1a Change the scale used here. I don’t understand why you would only use the lowest third of the values specified on a colourbar. Label countries (at least Syria, Turkey and Iraq) for ease of interpretation.

Figure1b Include location of radiosondes that are assimilated. Their influence is obviously limited to a certain distance and time from the launch so knowing their position and frequency is important.

Figures 3, 5, 8, 9 & 10. Where possible SEVIRI RGB images should be cropped closer to the model domain they are compared with, either this or show more of the region and draw the domain box on top of the satellite imagery (Figure 5 was especially difficult to interpret) as the different panels are zoomed in different amounts and the model domains are rotated compared with the satellite imagery.

Figure 4. I think that it would be better for interpretation if the style and parameters plotted were changed slightly. Currently topography over 900 m is shaded. In reality we don’t need this detail. You also discuss convergence but do not calculate or show it and interpreting convergence from wind vectors is very difficult. I suggest that you keep the vectors and the red contours for cloud (maybe make the contour lines thicker), but change the topography contours to a single blue or green contour at 1000 m. Then use colour filled contours (or greyscale) to show convergence. This can be as simple as a centred finite difference approach to show where the important convergence zones are.

Figure 6b Mark on location of domain shown in 6a.

Figure 7 As discussed in **Specific comments** I do not agree that the rain droplet to air temperature difference is the crucial parameter in the formation of the cold pool shown. Change 7a to be of colour contours of boundary layer sub-saturation or dew-point depression and have line contours of the rain droplet –air temperature difference overlaid on top. This would show where the sub-saturation was strongest as well as where the temperature difference is greatest and where there are hydrometeors present.

Figures 9 and 10 should have an additional panel added that shows the model dust load marked with the cross section (equivalent to 9a and 10a). This would help with interpretation, especially given the delay in triggering of convection discussed in the paper.