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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interaction of Dust Aerosols with Land/Sea Breezes over the Eastern Coast of the Red Sea from LIDAR Data and High-resolution WRF-Chem Simulations" *by* Sagar P. Parajuli et al.

Anonymous Referee #2

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This paper presents an approach coupling WRF-Chem, vertical profiles of a MPL lidar and photometric measurements (AERONET) to study aerosols on the western coast of the Arabian Peninsula during 2015. The authors also use MODIS, SEVIRI and CALIOP spaceborne observations to help them in their interpretations. The authors aim to better understand the role of coastal breezes on the vertical distribution of dust aerosols and assess the accuracy of the modelling compared to the observations.

This work is of scientific interest in the sense that the role of breezes and their interaction with the general circulation of the atmosphere is not necessarily well evaluated

at key locations on the planet, like in the case of the region considered in this article. Dust aerosols are now recognized as having a significant role on the radiative balance of some regions of the globe, but also on economic life (IPCC). This article is therefore interesting, and the results of this research deserve to be published after major revision.

This article should be seriously revised and better organized before publication. There is a lot of repetitions throughout the text, which makes reading the article considerably more cumbersome and detracts from highlighting the main ideas. There is a need to group together the elements of discussion spread throughout the various sections. It is also necessary to be clearer about the objectives as this article can be seen as a publication on the validation of WRF-Chem on the one hand and on the other hand it claims an annual study on aerosols above the experimental site. The part on the cross-comparison between instruments and model should be well separated from the scientific interpretations. A "Model validation" section should be done more directly.

This study is not conducted over a sufficiently long period of time to be able to speak about climatology. It should therefore be repositioned in a more global context to better highlight its scope. A major event has been observed and is the subject of a "case study", but is this event common in other years? Are the observed dust aerosol contents and their vertical distribution throughout 2015 reportable for other years?

The discussion section is confusing and needs to be better organized by a new structure of the article. It would be preferable to separate it from the conclusion, which will then more clearly highlight the major findings of this study.

Other comments

Introduction

L63. The vertical distribution of aerosols has been studied for decades using lidar measurements from the ground-based, aircrafts, and satellites (LITE, CALIOP, GLAS)

platforms. It is indeed an important parameter for the assessment of the climatic impact of aerosols. Numerous publications exist. For deontological reasons, I prefer to let the authors make their complementary bibliography, without influencing them. They can research what has been done during INDOEX, ACE-2 or AMMA at the international level and elsewhere.

L92. Clouds necessarily influence the lidar inversion which usually requires a reference, usually molecular in the upper troposphere. Can you clarify your statement?

Section 2.

Sub-section 2.1. The scheme on the breeze would be better placed in the revised discussion. L157. Use "annual study" rather than "climatology". L163. The CALIOP instrument? L173. Replace version by data? L190. Define DOD. L210 and following. How do you find the absorption coefficient with a MPL? More needs to be said about the implemental retrieval. L240 and following. Aren't there difficulties in parameterizing turbulence at such scales? Can you justify the choice of the PBL scheme? This is an important element for this type of study. L245. Define MENA. L248. Remind the definitions of u and v. L300-306. This approach assumes that there is no internal mixing. L326. Climatology?

Section 3

L343. Give the equation. L350. The "robust" term is somewhat strong with correlations between 0.6 and 0.7. L393-398. Example of duplication. Figure 6a. Define WS. Climatology? L403-406. Already mentioned. L407-417. Combine with what was already mentioned on the sea breeze. L432-433. No, CALIOP inversions use a lookup table with backscatter, color ratio and depolarization as inputs. L440-441. Be careful because the distance between two ground tracks is large. Fig. 7. Height is the altitude a.g.l.? Fig. 7c (MPL during nighttime) and related discussion. What we see above 5 km looks like contamination by semi-transparent clouds (or an average with cloudy profiles). This may also be why there is such a large discrepancy with the model. L458-

460. The difficulty in retrieve aerosols close to the surface is not the same for CALIOP and the MPL. L468. I do not think it is very good in the spring when the model gives much higher values. Sub-section L461. The model does not mark the PBL top well and it gives much higher aerosol extinction coefficients. It would be interesting to see the temporal evolutions of the PBL height deduced from the MPL and the numerical scheme chosen for WRF. A good representation of the PBL is fundamental to take into account the PBL/free troposphere exchanges. Moreover, to compare WRF and the MPL, it would be more interesting to have an OSSE (observing system simulation experiment) as for example in Wang et al. (ACP, 2014). L491-497. I do not understand what is being demonstrated here. Dust aerosol layers are often above the PBL and in coastal areas the PBL is lower. L504-505. Beware of cloud signatures on lidar profiles. L502-503. Aerosols emitted non-locally are most often transported at higher altitudes, above the PBL. This is therefore not an exceptional case. Sub-section L519. I do not understand what the "clear day"/"dusty day" comparison brings to the understanding of the differences between MPL and model. When there is no dust, it is normal that we do not see anything, it doesn't prove anything. L521-522. It is normal that the vertical profiles look like each other as they are proportional, and if the cross-section is not very variable, we find the same vertical structures. L524. As before, the model gives higher values, such as MERRA. The exception is for winter where the agreement is better. L548-550. That is a well-known feature. Sub-section L557. We return to the diurnal cycles as in section 3.2. Figures 6a and 11b show the same information. Why can't we see the same shift over the winter months? Figure 12. With a logarithmic colour scale the contrasts would stand out better. What are the temporal and vertical resolutions? Sub-section 3.4 L581. The effect of the breeze has already been discussed; it should be grouped together. L621-623. So, we don't replicate what the lidar shows. L630-640. What is described here has already been described for different coastal environments, such as during INDOEX. L644-651. There were also significant differences in the profiles in Figure 8, and these should be discussed together. L658. The altitude range of the land breeze is not sufficient to explain the low layer dust aerosols. L660-666.

A typical vertical wind profile would have been interesting. Sub-section 3.5 L711-715. I thought that a haboob was rather generated following the collapse of thunderstorms and the advection of moist air masses. L732. These AODs are much lower than the one announced in L684.

Section 4.

This part is too long. The discussion should be separated from the conclusion. It can also be associated with the analysis of each key element of the article. The organization of the conclusion relating the work presented is confusing.

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