Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-444-RC3, 2020

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 #3

Received and published: 1 September 2020

General comments on the article.

The overall objective of the paper is to "understand the vertical and diurnal profiles of aerosols over the eastern coast of the Red Sea." This overall aim if the paper is divided into four distinctive questions, the vertical profile, the diurnal and seasonal variation, the ability of WRF-chem to model the aerosols and how the prevailing land sea breezes affect the emissions and distribution of the dust over the study region. I believe this is a valuable scientific study that deserves to be published. The authors have employed appropriate data and analysis to answer the questions. Overall the structure of the

paper need to be re-worked. The authors should consider grouping ideas in the paper in a more consistent manner. The authors need to ensure that the conclusion that they draw are substantiated in the evidence they present. A major short coming of the paper is the attempt to link the dust to the land –sea breeze system – This link is not made successfully. The discussion ignores the fact the there is a massive escarpment in the domain that rises to approximately 1500 m. Acknowledging and accounting for this in itself will not make the link between circulation and dust but cannot be ignored as the Land sea breeze system in this domain is complex and is partly driven by the topography. The link to the dust and the coastal zone completely ignores the fact that the topography will induce its own local and meso-scale wind systems. It is also unclear from the wind data presented when exactly the winds reach sufficiently high speeds to induce these dust storms. The average wind data never exceeds 8 m.s-1. Overall I would focus the paper on the objectives outlined in the paper without trying to link this to land-sea breeze circulations. Detailed comments follow below.

Detailed comments.

Title – I am not sure the title accurately reflects the overall objectives of the paper. The fundamental question posed by the authors is the vertical distribution and the diurnal and seasonal variability of aerosols of the study area. This is as stated by the authors. The land sea breeze is a driver of these two atmospheric aerosol characteristics. The prominence of land sea breezes as expressed in the title is not reflected in the current title of the paper. I suggest the authors re-consider the overall objectives of the paper or modify the title.

Line 36 – "the LIDAR data……remote inland desserts." – The paper provides no evidence that the dust is transported from remote inland dessert sources. In fact the model domain of the dust emissions don't even extend to these areas.

Figure 1 is could be improved by adding a map of the study region. The current figure 1 could be moved to later in the article where the land-sea breeze is discussed which the authors refer to in line 155.

Line 172 – The author's should add details of the KAUST station. It would be very useful to see the actual location on one of the maps. Also what is the altitude of the station and distance from the coast for example as well as the length of the data series?

Line 216-231 is very difficult to follow. The authors could consider rewording this paragraph to capture the method is a clearer manner. This could be improved by adding more details to the method in this section.

Line 227 constraints should be constrains and "do not" should be "does not"

Line 232-236 – this paragraph is not entirely connected to the previous paragraph and does not stand alone where it is. The authors mention quality constraints applied to the LIDAR data but don't mention what these were or refer to a publication that documents this process.

Figure 2. The colored section of the figure representing the dust source is too small to be useful to the reader at all. If the dust source function is important (which it is) then the authors should add an additional map to show this clearly.

The level of detail in the WRF-Chem model methodology section is not consistent with the detail provided for the other data sets. The authors should consider balancing these sections so that all the study methods are well documented for future studies.

Line 326 – two years of data does not constitute a climatology.

Line 327 and 329 need to be expanded. It is not clear what this means exactly.

Line 337-339 – It is not clear why the authors think the mismatch at this stage is due to sampling and measurement frequencies. The most obvious mismatches are the highest peaks of the AOD values seen in the measurements and not in the model AOD. This explanation premature and not convincing given the model temporal resolution or not accurate or both.

Line 387-391 requires a reference.

Line 393-394 should be re-worded.

Line 396-398 – I am not sure that this sentence does justice to the complex transport associated with this process. Land-sea breezes are local scale wind systems that in the case of this study area could become embedded into to meso-scale winds. The link to long-range transport beyond those scales are complex and associated with multiple embedded systems within regional scale transport. The land-sea breeze mechanism is only a small component of that transport process.

Line 403-404 –the authors need to be specific about what the impacts might be of dust and include references here. Do these impacts have any bearing on the land-sea breeze system directly or on the results of this study?

It would be interesting to see the diurnal temperature pattern of shore of the site. The flat temperature cycle is not ideal for the establishment of a strong land sea breeze system. What creates the temperature gradient shift between daytime and night-time between the land and the sea?

Line 421 – 423- in terms of temperature this is not a justified statement. Even in terms of wind speed data the difference between the day and night values id 6 m.s-1 in MAM while in DJF it is at most 4 m.s-1. These differences may be significant in this region but you need to show that. The figure and text earlier points to a weak diurnal temperature cycle in all seasons.

Line 426-428 –describing all the aerosols as limited to the height of troposphere is not very useful and not a finding that is noteworthy. The vertical profiles of aerosol data in the absence of a vertical temperature profile I believe is difficult to interpret.

Line 455-456 – This needs data or a reference to validate this (or a reference). Also I think you need to refine this discussion as I do not see the same trends as you above 2 km for the two data sets.

Line 471 – 472 - The model does not show this layer in the daytime either. In fact the layer is observed in the nigh-time and not in the daytime in the MPL data.

Line 473-474 – The model daytime and night-time profiles are not very different. I think it is a stretch to infer the model reproduces anything with such a result. The model profile is pretty static for each of the categories graphed . This is over interpretation these data. This should be re-worked.

Line 506-511 – I can't agree with this explanation at all. This requires additional work and temperature profile data to substantiate all the assumptions. The PBL does not break at night and the capping inversion is not broken at night as this is driven in the summer by large scale subsidence which is not dependent on day night changes. The PBL is likely to drop in the evening and possibly a alternative inversion layers form that might trap and concentrate aerosols above. But my explanation is also speculation as it would be easy to see this mechanism from vertical temperature profile data at the very least from the model.

Line 541-Line 550 – This has no context. I don't follow where this has come from in the discussion. Figure 10 - Does not provide a new information about the vertical profile of the aerosols. I am not sure why this discussion could not be combined with the previous section.

Line 568-577 – I am not sure why this was not discussed earlier in the paper in conjunction with figure 6. Also the model and the observations have some real differences in terms of the time of the minimum and maximum values for the different months presented. I think this could be r-worded to more accurately describe what is observed. Again – one year of data is not a climatology!

Figure 13 – I am not sure that the land –sea breeze can be described as covering the entire area of your domain given in Figure 13. Especially if one takes into account that mountainous area lies at about 40 deg E. The wind on the eastern side of the mountainous terrain is almost certainly not associated with land-sea breeze mecha-

nisms anymore but rather on topography induced wind cycles. On the coastal side of the mountains the distance to the coast is about 100 km. Again there has to be a topography component to the wind system in this region which is strengthened by the land sea breeze mechanism.

Line 611-651 – in light of the above I believe this all requires some careful consideration and re-working.

Line 652-666 – this discussion completely ignores the fact that there is an enormous 1.5 km high escarpment sitting in the middle of the domain. This needs to be accounted for in this discussion. The last section of the paper is useful in presenting the occurrence of the high dust events observed in figure 3 that are not captured by the model. This could receive more attention taking into account all the comments above.

The discussion and conclusions need to be revised after the changes are made to the paper.

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