We are grateful to the editors and anonymous referees for the insightful comments which highly helped us to improve the quality of our paper. We now turn to addressing concerns specific to reviewer one by one as below.

[1. One major achievement presented in the manuscript seems to be the introduction of the Digital Optical Method (DOM) for the study of dust devils. The authors state that this is the first time this method has been used for this purpose. However, the recent paper by Liu et al. (2016, JGR), on which most of the authors are the same as on the present paper, has already made use of this method. Perhaps the authors originally intended a different timing of the papers, but it would at least be important to mention the existence of a companion paper, which presents part of what the authors discuss as "Prospect" in Section 5. Given that the authors, location, and measurement technique are the same, I suppose that the time period of field observations should be the same, too (given as 7 – 14 July in Liu et al. and as 2 – 14 July in the present manuscript)? Please clarify.]

Reply 1: Thanks for your comment. In fact, Chong Liu(Liu et al. 2016) I and others jointly completed the dust devil observation. We had different time period of field observations for our own independent observation mission. My task was focused on characterizing the internal structure of dust devils by applying the DOM. Following the reviewer's suggestion, we changed "This is the first time to use DOM to observe DDs" with "This study is the first to apply DOM to characterize the internal structure of DDs in the Taklimakan Desert." in the revised manuscript (P4, L14) .

[2. Even though the authors state in Section 5 that "the results documented in this study are far from generalized characteristics of DD opacity", the authors draw very general conclusions throughout the manuscript, e.g. "The distinct horizontal distribution of opacity values proves the existence of the DD's eye" or also regarding the formation conditions and flow structure in dust devils. However, so far as I understand, the authors present results from only one example. How many dust devils have been observed/ recorded during the 12-day observation period? I suppose more than one. Why are no statistics presented? The existence of a dust devil eye detected in one example does not necessarily mean that there has to be one in all dust devils. Understandably, the authors have selected a particularly well-structured dust devil to demonstrate the capabilities of their method. I think it is very important, however, to also show other cases, in which the dust devil structure is more complex, to see how the method performs under more difficult circumstances and to understand strengths and weaknesses of the method.]

Reply 2: Thanks for your suggestion. A total of 15 large dust devils were observed during the observation period. And this case was selected because of a pole as a reference to determine the height and width of DD (P5, L16-L18). And another case has been added in the revised manuscript according to your suggestion (P9, L1-L2).

[3.Several references used in the paper seem outdated (e.g. P. 2, L. 12) and some seem to be used in a misleading context. For example, it is stated that dust devils contribute 30% to global dust aerosol based on results from Koch and Renno (2005). A more recent study (Jemmett-Smith et al., 2015), suggests a much smaller percentage of about 3%. Other recent works support that dust devils likely contribute only 1% on large scale (see review of Klose et al., 2016, doi:10.1007/s11214-016-0261-4). Also, the statement that "30% of the primary particulate mass emitted to the atmosphere in the Sahara Desert", which the authors attribute to Marsham et al. (2008), seems to be wrong. In my understanding, Marsham et al. (2008) conclude that the consideration of boundary-layer convection in their model increased dust uplift by 30% compared to an estimate using mean wind only. This does not mean that all of the 30% can be attributed to dust devils. I recommend a critical examination of the referencing, discussion of the reviewed estimates in more detail, and inclusion of newer references where available. I would like to refer the authors to an exhaustive review of dust devil studies published as a special issue by Space Science Reviews (http://link.springer.com/journal/11214/203/1/ page/1).]

Reply 3: Thanks for the valuable suggestion for improving our manuscript. Following this suggestion, we have rechecked all the corresponding references and deleted the outdated references with misleading discussion. The corresponding discussions were added in introduction of the revised manuscript. (P3, L1-L22).

[4.Overall, the writing/language in the manuscript on hand needs to be improved. I am sure that some misleading statements can be related to difficulties in using English as the language for the paper.]

Reply 4: The revised manuscript has been modified by Mark J. Rood from UIUC.

[5.P. 1, L. 14 and P. 2, L. 10: "vortexes" instead of "vortexes of wind".]

Reply 5: It has been corrected.

[6. P. 1, L. 14: Please provide a reference for the given dust devil height-range of 1 –1000m]

Reply 6: The reference (Lorenz et al., 2016) has been provided in the revised manuscript (P2, L8).

[7. P. 1, L. 19: "swirling vortex" seems to be a tautology.]

Reply 7: It has been deleted.

[8. P. 2, L. 13 – 19: While weak winds and sunny weather might lead to dust devil formation, pressure fluctuations are a consequence, not a cause, of dust devil occurrence. Please clarify. Also, the subsequent discussion of heterogeneous solar radiation is somewhat confusing, and "certain conditions of angular momentum" seems very vague. Please revise.]

Reply 8: Thanks for the suggestion. In the revised paper, "surface pressure fluctuations" has been deleted, and the cause of formation of dust devil has been revised (P2, L15-L19) with the following discussions:

"Occurrences of wind devils are associated with weak wind and sunny weather. As the near surface air temperature rises with the increase of surface sensible heat under heterogeneous solar radiation, the thermal convection is driven by the thermal buoyancy in the convective boundary layer, consequently leading to vortex rotation containing particulate matters with large angular momentums (Kanak, 2005; Klose et al., 2016). And the radius of wind devil is mainly determined by the initial angular momentum of the air mass (Gu et al., 2010)."

[9. P. 3, L.7 – 11: While I know that dust particles can serve as ice nuclei and cloud condensation nuclei, and that dust can supply iron for oceanic phytoplankton growth, I do not know an "umbrella affect". It might be beneficial to briefly outline all three effects. Further, the effects are known to apply for dust aerosol in general, but I am not aware that the relevance of dust devils in the context is known or has been investigated at all.]

Reply 9: Following the suggestions, we have deleted the "umbrella affect" and the general effects of dust aerosols in the revised manuscript, because the relevance of dust devils in the context has not been investigated at all.

[10. P. 3, L. 12: I recommend referring to Neakrase et al. (2010, doi: 10.1007/s11214-016-0296-6) for a discussion of particle lifting processes in dust devils]

Reply 10: Thanks for your recommending the paper of Neakrase et al, which is very useful for our study.

In the revised manuscript, we cited the paper with the following discussion on particle lifting processes in dust devils (P3,L7-L9).

"Dust particles are more easily removed from the surface by DDs through the so-called "ΔP-effect"(Greeley et al., 2003;Neakrase et al., 2016). Dust particle lifting processes depend on structure and morphology of DDs, as well as internal pressure structure and ambient air conditions (Neakrase et al., 2016)."

[11. P. 3, L. 18 – 22: The authors seem to suggest that, compared to their technique, one disadvantage of the use of Doppler radar and pressure loggers for the study of dust devils is that the instruments need to be deployed in the estimated path of a dust devil. While this is certainly the case, I think the same applies to the use of a fixed camera, doesn't it? I also think that the cost of the pressure loggers as described by Lorenz (2012) can with approx. $120 not referred to as high.]

Reply 11: We agree to the reviewer's comments and accordingly deleted the statements to revise the manuscript.

[12. P. 4, L. 2 – 3: So far as I know, Kanak et al. (2000, 2005) conducted simulations with zero wind, so can probably not confirm neither deny that weaker winds and stronger surface heat fluxes are favorable for dust devil formation. See also Klose and Shao (2016, doi: 10.1016/j.aeolia.2016.05.003) for a study of the dependence of dust devil formation on atmospheric conditions using LES, Spiga et al. (2016) for a review on LES used for dust devil studies, and Rafkin et al. (2016) for a review on dust devil formation conditions (the latter two references can be found in link within comment 3).]

Reply 12: Thanks for the constructive comments. In the revised manuscript, we cited the studies of Toigo (2003) and Ito et al.(2010) , Klose and Shao (2016, doi: 10.1016/j.aeolia.2016.05.003) and Rafkin et al. (2016) in the discussion on dust devil formation conditions (P3,L18-L22;P4,L1-L2).

[13. P. 5, L. 21: How is dust devil height determined? Is the "upper end" of a dust devil defined by a particular opacity threshold?]

Reply 13: We estimated the dust devil height with a reference of the upright electric pole right next to the DD (Fig. 1). We defined the "upper end" of a dust devil with the opacity of near to zero.

[14. P. 6, L. 1 – 2: Why are the lines chosen to follow a conical pattern rather than a regular grid? I could imagine that in particular for dust devils that are not well-structured and might deviate substantially from an ideal conical shape, the choice of a dust-devil shaped grid is problematic. A regular grid might also potentially enable a better comparison between differently shaped dust devils.]

Reply 14: Thank you for your suggestion. We agree to the reviewer's comments. our choice of a dust-devil shaped grid is problematic. In this study, the lines are chosen to follow a conical pattern rather than a regular grid, just for the simple characterization of the optical structure of DDs. The method to quantify spatial distribution of opacity is able to present the similar spatial variability like grid boxes defined in DD numerical models to simulate spatial distribution of physical properties (Mason et al., 2013;Gu et al., 2006). In the further study, we will use the regular grids for a better comparison between differently shaped dust devils. The corresponding uncertainties have been addressed in the revised section 4. Conclusions and discussions.

[15. P. 6, L. 16 – 22: Is this derivation needed if the result is N = N0 for a plume-free atmosphere?]

Reply 15: Yes, with the transmission model quantifies the derivation of opacity of a plume with $N=N_0$ for a plume-free atmosphere.

[16. Figure 3: I think it would be easier to compare the different lines if they were all in the same figure. Also, Figure 3c does not seem to contain much additional information, so I suggest removing the figure.]

Reply 16: We agree to the suggestion. Figure 3c has been removed in the revised manuscript.

[17. Figures 5 and 7: I suggest using colors rather than numbers to visualize the opacity patterns.]

Reply 17: Thanks for your suggestion. Fig. 5 has been replaced by another case with colors visualizing the opacity patterns, and but Figure 7 is kept with numbers to precisely present the opacity values.

[18. P. 9, L. 8 – 10: Another reason for an expected opacity decrease with height is probably that the dust source is at the bottom and a higher particle concentration is expected close to the source. While gravitational settling certainly leads to a strong decrease of larger-sized particles with height, smaller particles might actually create higher opacity than larger particles (at comparable concentrations). What do the authors think how this would affect the vertical opacity profile?]

Reply 18: We totally agree to the comments. Accordingly we have modified the discussions in the revised manuscript (P9, L9-L13) as follows.

"The large particles decrease due to the gravitational settling and the fine particles continue to rise with height. However, the change of vertical air flow could lead to the vertical decreases in concentrations of fine dust for the declining opacity of DDs with height. These results are consistent with those from numerical simulations by Gu (2007) and Gierasch (1973; 1974)."

[19. P. 12, L.22 – P. 13, L. 2: The fact that the opacity in a dust devil depends on its formation

conditions and intensity seems clear. The investigation of more examples may already provide more insights in the variability of dust devil opacity. This might also be a good opportunity to reference to the companion paper of Liu et al. (2016), in which dust devil formation conditions are investigated.]

Reply 19: Following the reviewer's suggestion, the companion paper of Liu et al. (2016) has been cited for the discussion on dust devil formation conditions in the revised manuscript (P12,L17).