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Interactive comment on "*A Tale of Two Dust Storms*: Analysis of a Complex Dust Event in the Middle East" *by* Steven D. Miller et al.

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This paper investigates two dust storms that occurred on 3-4 August 2016 and the effect that the environment had on the ability of a particular type of remote sensing algorithm to detect them. The focus is on the infrared split window brightness temperature difference (SWBTD) technique, which relies on differences in the absorption features of silica containing minerals at 10 and 12 μm, which leads to greater extinction at 10 μm compared to 12 μm. Water vapor has the opposite behavior, which greater extinction at 12 μm compared to 10 μm, meaning that the presence of significant water vapor could potentially obscure the signal used in the SWBTD technique. The case considered here includes two distinct dust plumes that are readily apparent in visible satellite imagery of the scene. However, two different infrared dust detection algorithms are

both only able to detect the westernmost plume, while missing the easternmost plume nearly completely. Through careful analysis of the meteorological situation and use of additional satellite data sets, the authors convincingly show that the underlying cause of the missed detection is the presence of climatologically elevated amounts of water vapor in the eastern plume. In order to better understand the reasons for the failure of the SWBTD in this situation, the authors conduct a thorough investigation of the physical mechanisms that underpin the SWBTD approach, beginning with the assumed extinction and scattering properties of atmospheric dust. Their results again point to water vapor as the primary culprit in preventing successful retrievals from the SWBTD technique. By including a water vapor correction factor, they demonstrate a path to improving the retrieval in situations with elevated water vapor and make suggestions as to how this can be accomplished in at least a semi-operational manner.

The paper is interesting and extremely well written. For the most part, the analysis is careful and the results are convincing. I find that is paper is appropriate for publication in the journal, after some issues, described below, are addressed. These are primarily of a technical nature and either involve some details that need to be corrected or topics that should be discussed in greater depth.

Major Comments:

The major comments are organized by section of the text and include line numbers where appropriate.

Figure 1: It would help to have the two plumes of interested labeled as "1" and "2", not just the missed ("M") portion of one of the plumes.

Figure 4: This would seem to be a prime place to show an image of the SWBTD itself used in the DEBRA retrieval, but this is not done. This would surely be more convincing than the later figure (Figure 5c) that shows the SWBTD, but on the edge of the AIRS swath. As a reader, I find this frustrating because the authors go ahead and check the cloud screening, which to my eye clearly is not the issue, but fail to show what is likely

the root cause of the lack of detection.

Figure 5: In Figure 5c, the colors used to label the locations of “1” and “2” are valid colors in the color scale. Given the need to include these locations in Figure 5a and 5b for reference, I would suggest changing to a different color (like black) since they are labelled anyway, so the color information is redundant. In Figure 5d the color helps, but it does not need to map back to the color of the points because, again, things are labelled. Additionally, the AOD from Deep Blue is shown in Figure 5b, but there is no discussion in the text regarding AOD.

Page 12: When discussing Figure 5 both the AIRS spectra and SWBTD values are shown. It is not clear how the SWBTD is calculated. The header in Figure 5c says “BT difference between 10.35 and 12.3 μm [K].” Is a “single” wavelength used or some sort of spectral range? It would also help to indicate on Figure 5d where these values were extracted.

Section 3.4: I am a little surprised not to see the AIRS TPW make an appearance in this section. The NUCAPS results are convincing, so maybe the analysis would be redundant. With regard to discussion of the NUCAPS results, the authors write, “the NUCAPS profiles show significant differences in low/mid-tropospheric moisture. . .” It would be useful to quantify these differences. For example, you can read off the water vapor mixing ratio on the skew-T at 500 hPa and use that information to quantify the difference (it is a little hard to do with the resolution of the figure in the manuscript, however). A point of comparison for a “significant” difference would also be helpful.

Table 1: I happen to have the OPAC numbers easily available, and I see an issue with the way the values are presented in this table. Taking the imaginary part of the index of refraction, it turns out to be -0.5 at both 10 μm and 12.5 μm in the OPAC table. However, the 10 μm value is on a slope, while the 12.5 μm is at an inflection point in a plot of the imaginary part of the refractive index as a function of wavelength. Given the sensitivity of the calculation of SSA, for example, to this parameter, it is probably

not acceptable just to take the nearest tabulated value. Moreover, the OPAC database provides refractive indices at 10.6 μm, which is closer to 10.35 μm than 10 μm. Here the value for the imaginary part is -0.25, which is a factor of 2 different than the -0.5 value that appears in Table 1. Interpolating using a linear fit to the actual wavelength yields values for the imaginary part of the refractive index of -0.35 for 10.35 μm and -0.47 for 12.30 μm. This means that the value is 30% lower at 10.35 μm and only 6% lower at 12.30 μm, which may affect the conclusions the authors make based on the dust optical properties alone. It is unclear how carefully the other numbers derived from the ARIA and DB17 databases were calculated, but similar errors might be expected.

Page 20: It is fine to mention that “the spherical particle approximation of Mie theory was assumed in computing the dust optical properties,” but the question is how much error (approximately) is made with this assumption given that dust is non-spherical. This is treated in some detail in the infrared by Klüser et al. (2016).

Table 2: There seems to be an issue with the AOD values at 12.30 μm reported for DB17. The AOD should be proportional to the dust loading, so a plot of the AOD against the dust loading should be a line. For all other dust types, including the ones at 10.35 μm, this relationship holds, but DB17 has a “kink.” My suspicion is that the AOD entry for a dust loading of 186 μg/kg should really be 0.591, not 0.291 as reported in the table.

Page 21: The authors comment, “Despite these differences, the proportional relationship of extinction between the two wavelengths does not change among the various composition assumptions.” Is it the proportion or the difference that matters for SWBTD? Taking the middle row from Table 2, the ratio (proportion) of 10.35/12.30 μm is 3.8, 1.58, 1.25 for ARIA, OPAC, and DB17, respectively. This is a difference of up to a factor of three. Based on the note above regarding Table 1, these differences might be even larger if more appropriate refractive indices were used. The authors then comment, “hence, the sign of the SWBTD for mineral dust remains negative for all three databases.” This statement is not supported in the analysis shown to this point in the paper as both the AOD and the "blackbody" curve of the surface temperature both the SWBTD, at least to first order. In fact, Figures 11 and 12 (on the next two pages) demonstrate this exact point. I think it would more correct to state at this point in the paper, "hence, the sign of the SWBTD for mineral dust for extinction alone remains negative for all three databases."

Page 26: With regard to the DEBRA results presented in Figure 13, there is no discussion as to why the "dry" case reports so much dust compared to the "moist" case. While it is clear that the increase in TPW results in fewer plumes being identified as dust in the "moist" case, it appears that nearly everything, with the exception of what I assume are cloud fields, is identified as dust in the "dry" case. The explanation is obviously related to the thresholds used in DEBRA, but some discussion of this would be appropriate at the end of this section.

Page 28: At the end of this section the authors argue that models could provide some of the missing information needed to refine the vapor-indexed dust detection method. For example, models can provide vertical information on the location of the dust or a best guess for the atmospheric moisture profile. However, on page 24, Lines 12-14, the authors write, "whereas the [WRF-Chem forecast] model does not capture the exact details of the dust and moisture distributions as observed, it does represent the dry- and moist-embedded dust plumes to an extent that is sufficient for the analysis of water vapor impacts." Given that WRF-Chem runs are already computationally expensive, and do not provide sufficient detail on the observed dust and moisture distributions, is it really likely that other model runs, even with assimilation, would be able to provide the information with the necessary level of fidelity to really impact the results? Proving this point one way or another is clearly beyond the scope of this paper, but I feel the statements made at the end of this section should be somewhat more qualified.

Page 29. The authors write, "this study demonstrates that with a priori information on the moisture profile and dust altitude. . ." However, I was under the impression that section 5 only investigated the effect of the moisture profile. In fact, in the introduc-

tion to this section the authors state specifically, ". . . we examined to what extent the detection might be improved by incorporating atmospheric column moisture as *a priori* information into SWBTD-based detection algorithms."

Minor Comments:

Minor comments are provided mainly as suggestions to the author. Line numbers are provided where appropriate.

Page 1, Line 26: Missing an article. ". . . indexed to an independent-sensor. . ."

Page 2, Lines 10-11: On page 1, "littoral zone" is presented as a synonym for "coastal zone," this sentence asks us to consider "littoral zone aerosol properties. . . in coastal zones. . ." The terms are used somewhat interchangeably throughout the paper, but it does not make sense here. It would be cleaner to write ". . .characterization of aerosol properties for short-term forecasting applications in coastal zones. . ."

Page 3, Line 18: O2 should be subscripted O_2

Page 3, Line 20: The citation should probably be just "(Xu et al., 2017; 2018)"

Page 3, Line 22: The literature is unclear if "Reststrahlen" should be capitalized. I would argue that since it is just a German word (and not the name of someone), it should not be capitalized. In fact, on the next page it is not capitalized. Also, I think the parentheses in this sentence are not quite what is intended. It should read something like: ". . .involve the restrahlen band of silica (or quartz), a common and often significant constituent of mineral dusts found worldwide (Di Biagio et al., 2017), caused by. . ."

Page 4, Lines 3-4: I do not think this sentence refers to the "spectral band." Instead, it would more clearly be the "atmospheric window" that is being discussed.

Page 4, Lines 11-12: I think you need to lead the reader through the "working hypothesis" that you are proposing. Because the signal for water vapor has the opposite sign for mineral dust in the SWBTD, in the presence of both significant water vapor and atmospheric dust, the signals might cancel one another, leading to a violation of an expected BTD threshold and, consequently, the lack of detection for the case in question.

Page 4, Line 18: Strictly speaking, I would say that the lapse rate of the lower atmosphere over the desert in the daytime is "close to dry adiabatic."

Page 5, Line 4: Is the reference "Tramutoli, 2005, 2007" or "Tramutoli et al., 2005, 2007"? The reference list suggests it should be the former.

Page 7, Line 5: Should be "Cotton et al., 2003"

Page 9: Lines 3-4: probably ". . . a major challenge for numerical modeling. . ."

Page 10, Line 13: There is no reference for the Miller et al. (2009) citation in the reference list.

Page 11, Line 9: Seems like there should be a reference to the MODIS Deep Blue algorithm.

Page 13, Line 7: It is always confusing, but I still call it the "CALIPSO satellite" so it should probably be ". . . on the NASA Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observation (CALIPSO) satellite (Winker et al., 2009). . ."

Page 14, Line 2: Up until now, the CALIOP lidar and the CALIPSO satellite were referred to correctly. The backscatter shown in the figure is properly from "CALIOP" and not "CALIPSO."

Page 14, Line 8: ". . .CATS and CALIOP profiles. . ."

Page 16, Line 6: ". . .CATS and CALIOP observations. . ."

Page 16, Line 9: "...seen in the CALIOP data. . ."

Page 16, Line 15: "...CATS and CALIOP would. . ."

Page 18, Line 5: Should be "Nalli et al. (2016)"

Page 21, Line 11: I assume this is a "dry adiabatic temperature profile. . ."

Page 21, Line 21: "Kelvin" should be capitalized.

Page 23, Line 2: The phrase is typically "unmasked by" rather than "unmasked from" as it appears here. The meaning of the sentence is that this effect is ". . .a signal that becomes increasingly apparent as the influence of the overlying water vapor decreases as the dust layer's altitude is increased."

Page 24, Lines 3-4: ". . . it alone cannot explain. . ."

Page 24, Line 10: ". . .observed by a satellite for. . ."

Page 25, Line 9: ". . .observations of the dust distribution. . ."

Page 26, Line 1: Should check with the journal how this small number should be represented.

Page 26, Line 30: "7 Kelvin."

Page 27, Line 19: To clear up confusion, the sentence could conclude, ". . . is not simply imparting an image of itself upon the newly enhanced dust over land."

Page 28, Line 6: Maybe what is meant is, ". . . it could be used to provide an extrapolated first-guess. . ."

Page 28, Line 12: I think "Summary and Conclusion" should be Section 6.

Page 28, Line 25: I think it should be either "associated with" or "located within" "a maritime (moist) air mass. . ."

Page 29, Lines 7-8: "a priori" should be italicized.

Page 29, Lines 21-22: The Albers et al. reference is listed as "submitted," but here it appears as 2018, and the same is true of the Bukowski et al. reference. The Saleeby et al. reference says it was submitted to ACP in 2019, but here it appears as a 2018 reference. The Zupanski et al. and Wu et al. papers at least appear as submitted in

2018 in the reference list, but their actual publication date may be different, of course.

Page 30, Line 13: The date for the Cotton et al. reference appears in the wrong place for this reference style.

Page 30, Line 24: The Ginoux et al. (2011) reference does not appear in the body of the paper.

Reference

Klüser, L., Di Biagio, C., Kleiber, P. D., Formenti, P., and Grassian, V. H.: Optical properties of non-spherical desert dust particles in the terrestrial infrared – An asymptotic approximation approach, J. Quant. Spectrosc. Radiat. Transf., 178, 209 – 223, https://doi.org/10.1016/j.jqsrt.2015.11.020, 2016.

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