

Interactive comment on “Satellite Imagery and Products of the 16–17 February 2020 Saharan Air Layer Dust Event over the Eastern Atlantic: Impacts of Water Vapor on Dust Detection and Morphology” by Lewis Grasso et al.

Anonymous Referee #2

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Evaluation of overall quality The authors present a case study of a Saharan-like, dry dust air mass on 16-17 February 2020 to investigate whether higher water vapor concentrations can mask dust detections derived from satellite-based differential brightness temperature data as shown in numerical model-based studies of Miller et al. (2019) and Banks et al. (2019). For their analysis, the dry, dusty air mass off the west African coast is segregated into two partitions based upon the “dust” and “polluted dust” CALISPO vertical feature mask categorization. However, I feel that this segregation brings up the weakest element of this manuscript: the role of the polluted

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air. In short, the authors draw much attention to polluted air early in the manuscript, but then drop all mention (or relevance) of the polluted airmass and any role it may play later in the manuscript to focus instead entirely on water vapor. After this initial stumble, the investigation gets back on track to its hypothesis that higher water vapor content can mask dust detections. The material presented afterwards, definitively proves, with the combination of GOES and VIRS visible and infrared imagery, that elevated dust present in visible satellite imagery south of 10N was clearly missed by differential brightness temperature bands often used for dust detection where GFS total precipitable water values exceeded 20 mm. They also demonstrate that the presence and amount of water vapor can be derived from satellite-based estimates of total precipitable water and the more recently developed advected total precipitable water data products. The other problematic part of the manuscript lies in sections 4 and 5, which I feel distract from the overall narrative. These sections highlight current aerosol data assimilation and importance of aerosols to weather forecasting in South Florida, but the discussion on these topics is severely limited by the lack of any concrete examples or examples of what and how differential brightness temperatures could be used to initialize aerosol data assimilation. Finally, once the authors show that dust is masked by higher water vapor content, the existence of this missed dust is largely forgotten and I am left wondering as to how significant this problem is or if any methods may exist to detect this masked dust. Despite, these reservations about some elements of the manuscript, I do feel that the investigation presented does meet its investigative goals and provides a clear and relevant example of how water vapor can mask dust detections from commonly used satellite-based datasets and that this work should be published following some major revisions.

Specific Comments/Questions

Major:

1) In section 3, much emphasis is given the presence of polluted air within the ubiquitous presence of Saharan dust in the SDR as compared to the more pristine NDR.

After this section, the presence of polluted air is not mentioned again because of the paper's emphasis on masked dust detections in regions of higher water vapor content. Do you believe that the presence of polluted air has any discernable impact upon the ability differential Tb methods to detect dust layers or the on the values of differential Tb (i.e., more negative, less negative)?

2) I feel that sections 4 and 5 are almost an entirely different paper because material from these sections feel disconnected from the narrative. Material from these sections is not referenced in the abstract or as a key goal of this investigation. I do not dispute the importance of dust data assimilation for weather forecasting and health, but neither section shows or cites a concrete example of an application of differential brightness temperature dust detection being applied to improve data assimilation or weather forecasting. In short, something to help illustrate how you are improving over existing techniques would be quite helpful to tie everything together.

3) How significant is the problem is missed dust detections due to water vapor? The manuscript successfully shows dust is indeed masked by higher vapor concentrations, but what is the impact from this result? Should we be concerned that so much dust is missed? Are there any potential methods or suggested methods to address this issue?

Minor:

4) I would strongly advise against using the terms "SAL dust or SAL transport". Although dust is often associated with the SAL, dust is neither exclusive to it, nor is it always present within it. Figures 2 and 17, help illustrate this point where your sounding profiles (Figure 17) show the SAL to be elevated above the low-level marine layer and below the free troposphere, yet in Figure 2, the CALIPSO VFM shows dust being present from 3.0 kms to the surface. Furthermore, your paper does not limit itself to just results from just within the SAL.

5) Page 16, TPW and TB differencing. You show that you can successfully discriminating between the NDR and SDR regions based upon TPW and differential Tbs for

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your case study. Aside from the critical TPW value (i.e., Miller et al.) are there any other limitations to this method? Would you expect the nature of the aerosol mixture to have any impact should polluted air mix with the more pristine dust environment of the NDR?

6) Page 17, Figs. 12 and 13, ALPW: In Gitro et al. 2018, ALPW data products are noted to have 3-hourly output (including for 1800 UTC) because it leverages GFS analysis winds to advect LPW fields to a common analysis time despite its dependence upon LEO satellite data. In your manuscript, you show data for 0300 UTC 17 Feb. 2020, which you rationalize while stating, “Due to the use of LEO sensors, imagery for the ALPW was not necessarily available as often as ABI data from GOES-16. Subsequently, retrieved values of ALPW, in the layer from the surface to 850 hPa, valid at 0300 UTC 17 February 4 2020 are displayed in Fig. 12”. In light of Gitro et al. 2018 and your own explanation not being commensurate, is there another reason why ALPW data at 1800 UTC itself was not used and then compared to Figures 8-11, especially because exact ALPW values are cited. I do agree with the author’s assertion that dust layers are slowly evolving features (i.e., Fig. 18). The time difference seems unnecessary, even if your rationalization is use it as reference for the NUCAPS data, which are dependent upon the later overpass times (0333 UTC) of CrIS and ATMS instruments, and your dust airmasses of NDR and SDR are still distinct and slowly evolving.

7) Did you find that the critical vertical integrated water vapor values presented by Miller et al. (2019) and Banks et al. (2019) for dust masking by water vapor were consistent with your results? I would imagine that NWS forecasters might have a strong interest in knowing how well constrained the potential critical values are when applying the differential Tbs technique to find pockets of elevated, dusty air.

8) Would you expect the efficacy of differential Tb techniques to have any seasonal dependence?

Technical Corrections

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General:

- 1) Line numbering. For review, line numbers should be continuous for the entire manuscript and not reset on each page
- 2) Spacing following periods. I see examples of both one and two spaces in the manuscript. Please choose one method and then make the manuscript consistent.
- 3) Numerous typos. While it did not prevent me from understanding your paper, they were enough to be distracting. Please check for these typos with fresh eyes.
- 4) Incorrect usage of “Since” and “Because”. Since implies the passage of time (i.e., Since 1972) where because implies a reason for something.
- 5) Figure colorbars. For figures using GOES-16 data the color bar label text values tend to be rather small and hard to read. Can the font be modified or made larger?
- 6) Tendency for over usage of prepositions. Throughout the manuscript, there were numerous instances of phrases such as “. . .values of the difference between the temperature and dewpoint temperature. . .” or “. . .of values of TPW. . .”. While not grammatically incorrect, this type of phrasing comes off as excessively wordy and harder to read. Instead these phrases could be re-written as, “. . .difference in dewpoint and temperature values. . .” or “of TPW values”.
- 7) By definition the SAL itself is strictly defined a region of dry, well-mixed air that is bounded by the maritime layer below and free troposphere above where temperatures and warm and the air is dry due to dry, convective mixing upstream in the Sahara. Therefore it is somewhat taboo to say the “SAL is transported” or the “SAL is fractured”. Furthermore, a SAL is not required to contain dust (even though it is often associated with it). Instead for this section I might rephrase to say “transport of dust associated with a Saharan-like, elevated mixed layer is” because what you are tracking from satellite are the region of dusty, dry, well-mixed air mass westward across the Atlantic, which are eroded by air mass intrusions as it progresses across the Atlantic. This comment is

mainly associated with section 5.

8) Given 7, a SAL present over the Atlantic is by definition an EML. I would advise picking and using only one term or using a term such as “Saharan-like EML”. Switch between these terms is most common in section 5 and the end of page 25.

9) Typo on Page 26, line 1: should read as “into” rather than “in to”

Specific:

1) Typo on Page 2, line 33: should read as “. . .from two sources. . .”

2) Grammar error on Page 2, line 34: should read as “. . .data were. . .” rather than “. . .data was. . .”

3) Typo on Page 3, line 15: remove the extra space between ‘which’ and ‘are’

4) Typo on Page 3, line 20: should read “. . . was launched on. . .”

5) Typo on Page 3, line 21: remove the comma after operational

6) Typo on Page 3, line 24: “one-half orbit” should read as “half an orbit”

7) Define term on Page 3, line 30: Explicitly define low-earth orbiting as LEO. The acronym is used later without having be defined first here.

8) Typo on Page 3, line 31: remove the comma after “. . .2009)”

9) Clarification on Page 3, line 31: not sure of your intent in the phrase “A component of CALIOP is a lidar” because CALIOP itself is the lidar aboard CALIPSO. Please clarify your meaning.

10) Typo on Page 4, line 14: remove “tropical” before ‘Saharan Air Layer’ for consistency.

11) Typo on Page 4, line 17: should read as “. . .end-user feedback. . .”

12) Typo on Page 4, line 19: add “its” between “enable” and “use”

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- 13) Typo on Page 4, line 20: Add a hyphen after “0.5”
- 14) Typo on Page 4, line 34: should be “16-km footprint”
- 15) Define term on Page 4, linear 27: CIRA used 3 times in paper, but is not defined.
- 16) Typo on Page 3, line 36: Replace “Since” with “Because”
- 17) Type on Page 5, lines 8 and 9: add hyphen before “km”
- 18) Type on Page 5, line 15: replace “Since” with “Because”
- 19) Typo on Page 5, line 16: should read as zero-hour forecasts
- 20) Typo on Page 5, line 18: remove command after “(Fig. 1)”
- 21) Suggested edit, Page 5, line 19: Latter half of sentence reads oddly to me. I would suggest revising to “. . .contour, located over the eastern Atlantic.”
- 22) Minor correction, Page 6, Figure 2: the text of manuscript makes direct reference to Figure 2a and Figure 2b on page 7. It is understood from context, but I suggest either modifying the figure caption to include 2a and 2b or add an ‘a’ and ‘b’ on your figure panels.
- 23) Typo on Page 7, line 1: should read “. . .an ascending CALIPSO overpass. . .” because there is only one overpass in reference to Fig. 2.
- 24) Typo on Page 7, line 9: add “the” before first “VFM”
- 25) Suggestion on Page 7, lines 8-11: I believe your statement is not quite correct. All the VFM ‘sees’ is that the polarization signal happens to cross the threshold criteria between ‘dust’ and ‘polluted dust’. While it does separate regions of more significant pollutant concentrations, it does not necessarily mean that notable pollutant concentrations are not present in regions noted as being ‘dust’. It is just below the threshold to flag it as polluted dust. I would suggest revising to emphasize that dust was ubiquitous for the entire transect, but the greatest pollution concentrations are found south of 15N.

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26) Suggested revision on Page 7, lines 13-18: I would revise this paragraph. A hypothesis assumes that you will either validate or refute it in your study, but you are only stating your assumptions for what will be characterized for the NRD and SRD. My suggestion would be to remove the paragraph and just add a one sentence to the end of the last one saying “For this study, we assume regions along the CALIPSO transect northward of 15N were only dust and regions south of 10N. . . .”

27) Suggestion on Page 8, line 2: I would sell that this thickness range is consistent with Figure 2.

28) Suggested a revision on Page 8, line 2-4: Reads awkwardly, please revise. I would suggest combining the statement (line 2-3) with the question (lines 2-4).

29) Typo on Page 8, line 19: replace “diagnosed” with “derived”

30) Suggestion on Pages 8 and 9: Your caption in Figure 4 point to Figure 5, which comes after it, which is not ideal. Because Figures 4 and 5 are so closely related, you may want to consider merging the two figures together as Figure 4a and 4b. This your annotations make more sense because you can directly see what is being emphasized without the need for additional explanation.

31) Suggested revision on Page 9, line 4: replace start with “Specifically, difference plots of brightness temperature values (Tbs) at 12.3. . . .”. The original reads oddly with all the “of’s”.

32) Missing information on Page 9 lines 6 and 7: Although you are pointing to Miller et al. (2019), it would be more helpful if you stated what the critical value of vertical integrated water vapor is, otherwise your meaning comes off as rather vague. Is the content high? Low?

33) Removal on Page 8, lines 8 – 12: After “Although”, the information is redundant and does not add anything to your manuscript.

34) Removal on Page 8, Line 21-22: I would suggest removing sentence starting “With

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that” because it just repeats the same statement as the sentence coming before it. Instead skip right to the example.

35) Add detail on Page 8, Line 4-5: As this is a new paragraph, I suggest adding the references to Figures 4 and 5 in parentheses after “channel difference image” and “GeoColor image”.

36) Typo on Page 10, line 6: should read as “. . . upper-left portion. . .”

37) Suggested revision on Page 10, line 6: The white letter “A” is on Figure 4, yet you are talking about Figure 5. I would either mention that it is in Figure 4 or just say “denoted by the letter “A”.

38) Suggested merger on Page 11, line 12 and 13: Reads a little choppy, I would suggest merging the “Note” and that “That is” sentences.

39) Revision or removal of sentence on Page 11, line 14-15. You clear illustrate that the TBs difference method for dust detection is not foolproof, but the latter part of the sentence reads a bit too vague because it is not clear as to what the “other components” refers to. Will you talk about them? Are you referencing other work? Some clarity here would help.

40) Page 12, line 15: Would be helpful to know what this critical integrated water vapor value is. 41) Typo on Page 12, line 20: “Since” should be “Because”

42) Suggested modification on Page 13, line 13: Larger value can imply either larger positive or negative values. I would replacing “larger” with “positive” to remove any ambiguity.

43) Suggestion on Page 13, line 23: Remove the “stated differently” phrase. You say the Tbs difference is 15C, yet -18C and -2C are 16C apart. Is this a typo or a rounding problem? When combined with the additional directions, it makes this statement more muddled.

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- 44) Suggested correction on Page 13, line 24: replace “. . .in values of Tbs of the . . .” with “in Tbs values of the . . .”. To many prepositions here reads oddly.
- 45) Typo on Page 14, line 6: “Since” should be “Because”
- 46) Grammar error on Page 14, line 17: The colon use within the sentence makes no grammatical sense. Did you intend for a semi-colon? Please revise.
- 47) Suggested correction on Page 14, line 19: Need a better transition. You have the what “Values of TPW are shown in relation to various satellite fields”. This sentence is grammatically correct, but it is a statement without the context. You give me a statement, but not a motivation on what you will do. “I am doing X to investigate Y.”
- 48) Minor correction on Page 16, line 5: “Largest” can be both positive or negative, it would be unambiguous if either “warmest” or “highest” were used instead.
- 49) Suggested edit on Page 16, line 15: Although you mention it in your caption, it would be worthwhile to also mention the channels being differenced are 10.35 and 12.3 microns.
- 50) Suggested revision on Page 16, line 15-18: Sentence starting with “Not only” is quite wordy and hard to follow. I would suggest revising it into a more concise form such as “These figures show the NDR to be co-located with a, b, and c.”
- 51) Sentence too vague or out of place on Page 16, lines 19-20: The last sentence is terribly vague and just seems out of place. Which sensors? What is the significance?
- 52) Suggestion on Page 18, line 7: You mention that ALPW decreases from south to north and I can see this in your data. Perhaps it might be worth adding an annotation to Figures 12 to show exact where the 27.9 mm and 15.4 mm ALPW values are being estimated.
- 53) Suggested replacement on Page 19, line 2. Replace “retrieval of values of ALPW” with “retrieval fo ALPW values” to remove excessive prepositions.

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- 54) Possible logic error on Page 19, lines 2-3: The sentence has circular logic because “values of TPW are show in Fig. 15”, but then later in the same sentence say it “was the time of the granule show in Fig. 15”. Do you mean to reference a different figure?
- 55) Suggestion on Figs 12, 13, and 14: You often cite 15N because it divides your pristine NDR and polluted SDR environments. I think it would be useful to consider annotating a line to each of these figures to show the 15N parallel for reference, especially for figure 14 where the coastline is muddied by the show Tb difference data.
- 56) Overly wordy phrase on Page 20, line 6: “values of the difference between the temperature and dewpoint temperature”. I would consider revising to be more concise.
- 57) Suggestion on Page 20, line 19: Instead of using the phrase “appeared earlier in the manuscript”, I would consider adding “(Fig. 10)” after “images” on line 18 to make your wording more exact.
- 58) Typo on Page 23, line 7: should read as “. . .assimilation of aerosols. . .”
- 59) Awkward working on Page 23, line 7, “. . .assimilation of the coupled atmospheric component. . .”. It is no clear as to what the atmosphere is coupled, I would suggest removing “coupled” to make the distinction between WCDA and SCDA more clear.
- 60) Typo on Page 23, line 14: should read as “. . .data of aerosols. . .”
- 61) Typo on Page 23, line 19: should read as “. . .a NWP model.”
- 62) Logic error on Page 24, line 17: Once over the Atlantic, the SAL is by definition an EML.
- 63) Typo on Page 24, line 36: should read as “. . .split-window difference. . .”
- 64) Additional detail needed Page 25, line 8: It would be useful to know what this “some value” is.
- 65) Typo on Page 25, line 20 and line: remove the space before “

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66) Typo on Page 27, line 17: Remove the hyperlink associated with the doi

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