Comment on acp-2021-418 - DOD from MIDAS
Anonymous Referee #1

Referee comment on "15-year variability of desert dust optical depth on global and regional scales" by Stavros-Andreas Logothetis et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-418-RC1, 2021

Review for Atmospheric Chemistry and Physics
Title: 15-year variability of desert dust optical depth on global and regional scales
Authors: Stavros-Andreas Logothetis, Vasileios Salamalikis, Antonis Gkikas, Stelios Kazadzis, Vassilis Amiridis and Andreas Kazantzidis

General Comments: The authors have analyzed dust optical depth (DOD) trends from the MIDAS dataset for all major global dust source regions in some detail in this paper. I think that this paper makes a useful contribution to the literature and provides a generally comprehensive global view of trends in dust optical depth. However, I have a few issues with the manuscript that the authors should explore in order to make the analysis more convincing and also easier to read and comprehend. First, how are we to gauge the reliability of the DOD trends without showing some verification versus AERONET sites for example? For example the Gkikas et al. (2021) paper on this dataset shows significant regional biases in DOD for MIDAS versus several AERONET sites in their Figure 4d. You should include at least two trend analyses from AERONET data to compare with your MIDAS inferred trends of DOD. The SDA retrieval from AERONET (O’Neill et al, 2001, 2003) separates the AOD into fine and coarse modes and trends of coarse mode AOD from AERONET could be compared to the MIDAS trends of DOD. Second, throughout the manuscript I found it difficult and confusing to have to look at the supplemental document file in order to confirm principal conclusions made in the primary manuscript. The way this paper is currently written it requires quite frequent switching between reading the 2 document files in order to understand the author's arguments. I strongly urge the authors to move some-many of the figures that are in the supplemental section into the main paper. Third: The very localized (relatively small area) of the large negative trends in DOD shown for the Bodele Depression (Figure 9) suggest the possibility of a surface reflectance artifact in the satellite retrievals and therefore possibly a trend in surface reflectance and not of dust concentrations. Since this is the largest single dust source on the planet then why is the strong negative trend in DOD over such a small area? It is well known that the dust plumes from the Bodele are advected by winds over very long distances. You need to explain in the text why the strong decrease in DOD is so limited to the very site of this dust source itself. Also the white areas around the blue negative trends are assumed to be near zero in Figure 9, although the color bar at the bottom shows yellow color for near zero trends, not white. Can you explain the discrepancy in the color bar and the map?
Specific Comments:

Lines 141-142: Some quantification is needed here rather than calling the MIDAS dataset 'trustworthy' and rather than just saying '...a quite high level of agreement' exists with other datasets.

Lines 206-207: It seems that this sensitivity analysis is important. Therefore I think the authors need to include some discussion and figures in this paper in order to support their conclusions.

Lines 210-213: This is not obvious in Fig 1 below. As a reader it is difficult to have to look at a different supplemental document in order to confirm conclusions made in the primary manuscript.

Line 221: Please define the blank or white areas on the Fig 1 maps since white is not shown in the color bar scale.

Line 228-230: It seems strange and unlikely that for such a large dust source as the Bodele that the decreasing trend is confined only to the exact location of the depression itself since it is well known that this dust is advected in various directions out of the basin.

Line 236-238: Please explain here what spatial resolutions were examined by de Meij et al (2012). Again I find it extremely difficult to have to switch back and forth between the main and supplemental documents in order to follow some of your main points in this paper.

Lines 253-261: I think that a table comparing the two data sets (Voss and Evan (2020) and the current study) is warranted here.

Lines 265-266: Please be clear here what the filtering criteria are. Again here, the readability of this paper is reduced significantly by frequently referring to supplemental figures in a different file.

Line 282-283: Yes this is obviously true, however you did not really present any evidence for erroneous trends. Why conduct this exercise if you cannot show clearly why a lack of temporal filtering is problematic? Or maybe I have missed your point, in which case it would be worth trying to clarify this.

Line 288: Please give a short summary of this methodology. As this paper currently is written a reader needs to jump from one document to another (either previously published papers or your supplemental document) to understand what is being done or discussed.

Lines 322-323: Every satellite data set easily detects trans-Atlantic dust transport. Why reference only your current database?

Lines 326-327: OK I applaud the honesty here but this decreases my confidence somewhat in the trend analysis maps. Comparison of these trends with some verification versus AERONET sites would be useful.

Line 339: I think you might mean “tends to be getting dustier” since Table S1 is a table of trends.

Lines 404-405: It seems that some mention should be given to the statistical significance and uncertainties of these measured/computed global AOD and DOD trends.

Line 468: Please provide some explanation of the strong land-sea gradient in the DOD
over the southern portion of the Red Sea shown in Figure 11b.