In this study, the authors attempted to identify HLD sources across the Canadian Arctic Archipelago and adjacent areas by using FoO of DOD from 20-yr high-resolution MODIS retrievals as well as VIIRS aerosol type product, CALIOP products, and observations from two AERONET sites. The authors also compared the spatial distribution of FoO with G-SDS-SBM dataset and showed an overall agreement. There are also significant areas of disagreement between FoO from MODIS and VIIRS and between satellite retrievals and G-SDS-SBM dataset. Overall, I think this study provides insights for identifying HLD sources and can benefit future modeling and observational studies. I have a few comments for the authors to consider.

We thank the Anonymous Reviewer for their work on our paper.

General Comments:

1. I wonder if MODIS products using the Dark Target algorithm (DT alone or combined DT/DB) can change the FoO of DOD and contribute to the uncertainties in identifying potential HLD sources, especially for coastal regions having both land and ocean.

The FoO of DOD will likely change a bit when using two satellite retrievals based on the same data, but optimized for land vs. water. How much change can be expected in FoO of DOD (or the always larger AOD) is a complex question, although we have some idea from the work of Sayer et al. (2014, JGR), which generally shows high correlations in JJA between DB and DT AOD (Figure 5), as well as small AOD differences (Figure 4), with the major caveat that Arctic (> 60 N) satellite coverage is sparse, even in JJA. The abstract of this paper states: "*Neither algorithm consistently outperforms the other, although in many cases the retrieved AOD and the level of its agreement with AERONET are very similar. In many regions the DB, DT, and merged data sets are all suitable for quantitative applications, bearing in mind that they cannot be considered independent, while in other cases one algorithm does consistently outperform the other. Usage recommendations and caveats are thus somewhat complicated and regionally dependent.*" No recommendations are made for the Arctic because none of the 111 AERONET stations used in the validation of the DB/DT/merged products were in the Arctic (Figure 12). As far as using the merged DB/DT product over land, Figure 1 of Sayer et al. (2014) shows that some regions of the Arctic (over land) use only DB, some use only DT, and some use a merging of the two. Coastal regions having both land and ocean in data pixels are indeed tricky and would require very special treatment at the level of L2 granules. Our MODIS data has a 10-km resolution, making products less certain roughly 5-10 km inland. We have added this limitation to our discussion. We also added statements in our Conclusions about the need for future work related to the use of different satellite retrieval products for dust identification in the high Arctic.

2. I would suggest providing more details or high-level formulas to better explain how SI from G-SDS-SBM is calculated which may be helpful to explain the differences between the datasets and satellite retrievals. It seems to be mostly dependent on soil properties. It is not clear to me if SI depends on surface wind.

   We agree that further explanation of how SI from G-SDS-SBM is calculated would be helpful. We added clarifications to the Introduction (1) and Other Datasets (2.2) of the manuscript. Briefly:

   - friction velocity of 1 m/s assumed globally

   - 5-year climatology (2014-2018) of month-specific assimilated meteorological fields at 0.25 deg resolution (ECMWF) were used to estimate minimum soil moisture values and maximum soil temperature values, gridded globally. This represents a region's maximum capacity to emit under favorable meteorological conditions (i.e., dry warm soil and high wind).

   SI being 1 indicates maximum capacity in cases of strong surface winds. However, the authors state that the derivation of SI does not need surface wind. What about the surface wind/friction velocity in the large green domain in Figure 6c compared to that in the red domain in Figure 6b and 6c.

   - An SI of 1 indicates a soil's potential to emit at or above the 99th percentile level of erodible soils, globally (Vukovic (2021)). The derivation of SI did not include surface wind input fields, rather, a globally constant high friction velocity of 1 m/s was assumed at all times in the emission parameterization. As such, SI represents the potential of a soil to emit under high wind conditions.

   - Geographical wind speed differences do not appear to explain discrepancies in Fig6b or 6c; we included a wind climatology in the Supplement.

   I also wonder how native (~0.008°) SI data were regridded to 0.1°.

   - Regridding was performed using bilinear interpolation in the CDO package.

Specific comments:

Line 9, please check if multiplication sign is used instead of letter x. Please check other places as well.

Fixed throughout.

Lines 32-33, there are some more recent studies addressing this issue. I would suggest adding them. For example, Shi et al. (2022). Relative importance of high-latitude local and long-range-transported dust for Arctic ice-nucleating particles and impacts on Arctic mixed-phase clouds. Atmospheric Chemistry and Physics, 22, 2909-2935.

Yes, this is a great paper that we have also found since our submission to ACPD. We have referenced it and expanded the discussion around lines 32-33 to include the contributions from Shi et al., including the primarily lower tropospheric contribution of HLD. This is also relevant to the discussion of CALIOP-derived vertical dust distributions and is now mentioned in Section 3.3.

Line 235, why FoO DOD_{B16} > 0.5 is much lower than FoO DOD_{PG16} > 0.5 in the east of 95°W high-latitudes?

In the paper we stated that FoO DOD_{B16} > 0.5 is in general 50% lower than FOO DOD_{PG16} > 0.5 and we explained that this was due to the threshold nature of DOD_{B16}, which requires α < 0.3 (or no DOD is produced). Second, we stated that DOD_{B16} hot spots are focused east of the 95°W longitude, and did not state reasons why. We clarified these points and uncertainty in the manuscript.

Line 238, for DOD_{PG16}, is it calculated when w < 1? Does that mean it is filtered as well?

Yes, but in practice this filtering eliminates small numbers of points because ω is > 0.99 only 0.5% of the time.

Figure 3, I would suggest changing the color bar for the three bottom figures. It is just showing black over most places and gives less info.

The color bars were adjusted in the final manuscript version.

Figure 5. I feel it gives limited information compared to lat-lon plot. How about showing similar lat-lon spatial distribution of FoO of CALIOP DOD using the algorithms from literature (e.g., Yu et al., 2015)?

It is true that Figure 5 gives more limited horizontal information compared to the lat-lon plots, however, it gives some vertical information about the location of the dust in active satellite observations. We chose this representation also because a lat-lon plot of CALIOP FoO of aerosol type “dust” was very sparsely populated, even in a 20-year mean, at the horizontal resolution of the lat-lon figures (DOD would also be sparse, if it was calculated). As discussed in Section 2.1.3, this is because of the extremely narrow footprint of the CALIOP laser.

Thank you for the reference to Yu et al. (2015). These authors carried out a quantitative analysis of transported dust flux in the Saharan outflow region, which has the most abundant and continuous plumes of dust in the world, making their quantitative work more justified than in our case of using occasional DOD extremes to point to potential dust source regions.

Lines 362-363, this sentence is not clear to me. Could you explain a bit more and rephrase it?

"This 10% MODIS threshold , which does not match the SI threshold of 0.5 (50%), was chosen because we are testing for the FoO of strong dust emission events (DOD > 0.5), and it is reasonable to assume that such conditions will be met in far less than ~~0.5 (~~50%~~)~~ of observational cases, i.e., 10%."

Figure 6, I agree with the key point the authors tried to make. However, I would suggest at least trying to use different thresholds for FoO MODIS DOD and FoO VIIRS AT_{DUST}. There are significant differences between FoO MODIS DOD and FoO VIIRS.

We experimented with different thresholds before submission and there was not one that made all three datasets agree, so we decided to use the same thresholds for all datasets, for consistency, as stated in the discussion. We revised the wording to make it clear that we had experimented with thresholds.

Figure 7, I'm a bit confused about this figure. What's the unit for FoO shown here. Is it the number of occurrence or the percentage as to total observations? It seems that blue and green lines correspond to the left y-axis while the brown line corresponds to the right y-axis. For right y-axis, is it for number of occurrence or the percentage?

Indeed, this figure was confusing (also to CC 1 and CC 2) and we have revised it in several ways. It used to show the spatial mean number of occurrence, but the legend labels did not match the axis labels (there was a descriptive note in the text, but it was hard to spot). We have matched the axis and legend labels and normalized the FoO values, eliminating the need for two y axes. The normalizing factors are included in the caption.

Line 448, I would suggest showing the locations, as well as other selections of region such as the region in Figure 7, in Figure 8 or in the section 2.

Line 448 refers to Figure 11, which was created as we struggled with making the fewest number of figures while not showing Results (new dust source locations) in Methods (AERONET station locations). We have added the spatial averaging domain of Figure 7 to Figure 11.

For the conclusion part, I would suggest the authors give a high-level summary of some potential HLD sources in the order of confidence (i.e., agreements between MODIS, VIIRS, and G-SDS-SBM).

Agreed. We have expanded the summarizing statements about potential HLD sources in our Conclusions, referring to various dataset agreements.

References

Yu, H., Chin, M., Bian, H., Yuan, T., Prospero, J. M., Omar, A. H., Remer, L. A., Winker, D. M., Yang, Y., Zhang, Y., and Zhang, Z.: Quantification of trans-Atlantic dust transport from seven-year (2007–2013) record of CALIPSO lidar measurements, Remote Sens. Environ., 159, 232–249, https://doi.org/10.1016/j.rse.2014.12.010, 2015.