

Reactions to referee #1

We thank the referee for the thorough review along with the suggested modifications / corrections. Our detailed reactions (red font) are below.

This paper applies a collection of in situ and remote sensing observations to characterize the properties of seven dust events in the Canadian Arctic Archipelago. Surface data focuses on instruments at Eureka: the CIMEL, from which AERONET retrieval products were obtained, the Arctic HSRL, and an APS. Satellite remote sensing data include MODIS, MISR, and Sentinel-2 imagery, aerosol optical depth mapping, and from MISR, also plume heights. Wind data are obtained from MISR and from Reanalysis. The authors have built a cottage industry in studying dust in the Arctic, which is an important topic, as summarized in the paper's introduction section; this paper aims to provide a useful addition to that literature.

The reviewer will recall that MISR plume speed estimates were also provided (the capabilities of MISR vs CALIOP were a kind of epiphany for us in terms of their practical application).

A lot of careful work has gone into analyzing the seven cases presented. The value in this work is that it demonstrates techniques that could to some extent be applied more widely around the Arctic based on remote sensing data alone. Such wider application would be part of follow-on work, which is why the current paper is appropriate for AMT, as the authors indicate. The authors make a considerable effort to justify the interpretation of satellite remote-sensing signals in the seven cases as due to local dust events, and they are each quite convincing, despite very low AOD values. I note that most passive satellite remote-sensing observations in the Arctic remain extremely difficult to interpret. My primary recommendation is that in the Abstract and especially in the Conclusions, caveats might be provided that make clear the seven cases presented here are all distinct, narrow plumes, at least partly over dark water, downwind of likely dust sources, under contemporaneous high-wind conditions, and that interpreting MODIS and MISR data as indicating local dust plumes more generally must be done with similar care. Some further notes are included below.

The referee's primary recommendation was, we believe, relevant: the paper now ends with a paragraph along the lines of that recommendation (to the point of borrowing some of the referee's expressions)

Notes

Lines 182-184. As this study uses primarily MODIS DT over water, in interpreting the FMF product, one needs to consider that sea spray can also be a "coarse mode" aerosol, especially in the vicinity of the high-wind events that can also be effective in mobilizing local dust. This is not a concern in the seven cases highlighted in the current paper, but caution would be needed when applying the approach more generally.

We agree with the idea of inserting a cautionary note. A sentence indicating that dust plume aerosols are easily differentiated from any possible sea-salt aerosols was added to the end of that section.

Figure 2. In itself, this is a tough measurement. All the AODs are below 0.01. I understand that the authors have done a convincing job assembling other indications that this is a dust event.

However, I have to ask whether variability in the background AOD produces similar fluctuations in general, especially if the intent is to subsequently apply this technique more widely, specifically in cases lacking the distinct, narrow plumes over dark water surfaces, downwind of regions with the characteristics of likely dust sources represented by the seven cases studied in detail here.

Those small AOD values should indeed be viewed with a degree of healthy skepticism. But, the temporal correlation of the lidar CM AOD (τ_c^l) and CIMEL CM AOD at OPAL ($\tau_{c,1.5}^o$) and lack of correlation with the CIMEL CM AOD at the Ridge Lab ($\tau_{c,1.5}^p$) provide a significant measure of support for an argument of low altitude (local) dust events at the OPAL station. This is further supported by the CIMEL standard deviations (from the figure caption: “The std of $\tau_{c,1.5}^o$ is generally significantly larger than the std of $\tau_{c,1.5}^p$ (this disparity amounts to a quantitative check on the relative unresponsiveness of $\tau_{c,1.5}^p$ ”).

In order to respond to the reviewer’s comment about “variability in the background” we carried out a day-to-day investigation of τ_c^l , $\tau_{c,1.5}^o$, $\tau_{c,1.5}^p$ and $\Delta\tau_c$ variation across the months of July and August 2007 (the temporal extent of our 7 dust events). The 1st most obvious effect¹ was the generally common temporal variation of the PEARL and OPAL CM AODs (what one might expect for two quantities that share the same line of sight in the absence of any significant CM AOD variation between them). In short, the OPAL and Ridge Lab background CM AOD variation was consistent with what one would expect in the absence of a dust layer between the two instruments².

Lines 285-288. I know it is given in the publication cited in the footnote, but this estimate of MOIDS sensitivity to AOD seems *extremely* optimistic, especially when applied in the Arctic. Consider the further, extenuating conditions at high latitudes – low sun angle, very bright snow or ice-covered surfaces that can affect the recorded signal in nearby dark-water areas due to latency, internal reflections, or other instrumental effects, and possible thin cirrus contamination. I guess, for wider application of the approach, it would strengthen the case to show that, in non-dust circumstances (based on verifiable surface measurements where available), such remote-sensing signals are unlikely to occur. I.e., characterize as much as possible the likelihood of false positives.

The referee’s characterization of “extremely optimistic” had, in fact, been explicitly brought up in our text: “However, MODIS AOD precision is clearly an excessively optimistic (out of context) statement ...”. We don’t pretend to believe that MODIS is sensitive to the 0.0004 precision of the footnote. The suggestion of a complementary study (focused on, what would amount to very weak (CM) AODs competing with dust plumes of very weak CM AODs) is, we believe, beyond the scope of the present study. In the end, for the purposes of the present paper, we have to rely on circumstantial evidence (sufficiently large CM AODs, plume visibility in color images, the presence of nearby dust drainage basins, etc.) to confirm the identification of a dust plume. The referee will recall that we placed an approximate lower

¹ Partly masked by the fact that the OPAL CM AOD could be very noisy on ~ 30% of the days in those two months (but not so noisy as to be rejected by the AERONET cloud-screening protocol). In effect, the OPAL AOD rate of point-to-point change (where the reference point is itself a weak AOD) is less than the AERONET AOD cloud rejection threshold of 0.01 per minute (the AERONET AOD threshold does not attempt to account for strong relative changes due to very small AODs). We do not know the source of this noise but can state that it did not seem to be a factor during our 7 events.

² We also found very clear (total column) CM background situations where the variation of the lidar and OPAL CM AODs was significantly smaller than the illustrative July 23 (Event 5) dust event of Figure 2 (and approximately constant)

limit of ~ 0.02 (in the conclusions section) as the dust CM AOD that one could hope to detect using current satellite measurements.

Figure 4. Another thought, given the challenges involved in applying this technique more widely. Conditions for mobilizing dust would include sufficiently high wind (which is considered for the cases included in the paper), the availability upwind of loose surface dust that is not snow-, ice-, or vegetation-covered (also considered in the paper for the case studies), and usually some surface roughness to mix momentum downward. At least some locations of surface dust deposits have already been mapped across the Arctic, based on the identification of dry river deposits, mountain talus accumulations, etc. in seasons when those surfaces are snow- and ice-free. The exposure of such upwind surfaces at the specific time of dust-plume observation could be verified with contemporaneous satellite imagery. These considerations would be especially important when interpreting possible remote-sensing local dust-plume detections in general, in the absence of ground-truth data. (I agree that the seven specific cases presented in the current paper are convincing.)

We agree that dust emission requires not only sufficiently strong winds but also the presence of exposed, erodible surface material and favorable surface conditions (e.g., limited snow/ice cover and sufficient surface roughness). In the case studies presented here, we verified the availability of potential source regions using contemporaneous satellite imagery to ensure that upwind surfaces were exposed at the time of observation. We agree that clarifying these conditions would improve the interpretation of local dust detections in the absence of ground-based confirmation. The available “North American Environmental ATLAS” was employed to classify most of our dust sources as “Barren land” and water shed areas (i.e., areas that can be considered as potential dust sources).

Page 18, footnote 34. I’m not sure what “...neither the plume height nor the plume speed sampling trajectories are subject to any objective sampling protocol...” means. As I understand, the MISR MINX retrievals are done on all 1 km pixels within a user-defined aerosol plume region where the spatial contrast relative to surrounding pixels in the multi-angle images is sufficiently above the noise to produce a geometric height retrieval.

Our statement refers not to the MINX retrieval algorithm itself, but to the selection of plume structures and sampling trajectories analyzed in this study. Specifically, plume regions and sampling paths were identified visually from color imagery rather than through an automated or statistically defined detection protocol. Therefore, the reported plume height and speed statistics should be interpreted as indicators derived from visually selected plume features rather than results from a systematically sampled population of plume pixels.

Generally, it would be helpful to have a more complete list of Symbols and Abbreviations. I know there is a list in Appendix B, but many are missing, such as, τ_c^o , τ_c^p , τ_c^l , VDR, etc. It is a long article, and there are many non-standard abbreviations; searching for the meaning of some abbreviation becomes tedious.

We reviewed the list and added the appropriate symbols and abbreviations.

Reactions to referee #2

In the first instance we would like to thank the reviewer for his or her insightful comments and the significant amount of time that it must have taken to prepare this review. Our detailed reactions (red font) are below.

The topic of remote sensing of high latitude dust (HLD) is of particular interest because dust has direct and indirect radiative effects on the radiation budget and also on surface albedo. As the authors clearly highlight, despite the wide spatial distribution of HLD in the Arctic, its optical properties and abundance do not allow for a clear signature on remotely sensed signals. The use of space-borne observations together with ground-based in situ and remote sensing measurements, which are scarce in the polar regions, can help characterise the properties and effects of HLD.

The manuscript by S.A. Sayedain discusses local HLD in the Canadian Arctic using both remote sensing and in situ ground-based measurements, as well as satellite observations and products derived from MODIS and MISR. However, the presentation of the work is not very clear, both in the text and due to the lack of significant figures. Although important for following the analysis and describing the results, the figures are included in the supplementary material in PowerPoint format (which was not made available to the reviewer).

The supplements were not available because they exceeded the journal limit. They were instead made accessible via the data repository link provided in the “Supplement” section of the paper. We believe that use of these supplements files makes the essential narrative of the paper easier to understand (please see our footnotes comment immediately below)

More specific comments follow.

The rather frequent use of footnotes and parenthetical text, combined with reference to supplementary material, does not make the manuscript a completely smooth read. A parenthesis within another parenthesis should be avoided.

We tried our best to minimize the use of parenthetical text. However, with respect to footnotes, we believe that they actually help to achieve a smoother read (the essence of an idea is in the main text: the reader is free to dig into the footnote details if he or she wants more than a higher-level narrative).

Additionally, in my opinion some information in the notes may be omitted (such as 18 and 21). We eliminated footnote 18 which is not essential to the fundamental CM AOD narrative. We disagree with getting rid of footnote 21: there is room for confusion between the indigenous name of the lake (Lhù'ààn Mân'), the (historical) non-indigenous name of the lake (Kluane Lake), the AERONET name of the Kluane Lake research station (Kluane Lake), and the acronym (KLRS) of the research station. These nuances need to be clear if one is to understand the links with earlier literature. That said, and in the spirit of the reviewer's comment about reducing the use of footnotes, we decided that footnote 22 could be eliminated).

It is clear that the complexity of describing what is happening over a large area around Eureka, with a weak footprint especially in remote sensing measurements, requires the use of extensive material, including satellite imagery, reanalysis, etc., hence the use of supplementary material. The result, however, is that the text and figures the reader expects in a paragraph are different

from what they find. An example is paragraph 4.1.3 Satellite imagery versus ground-based measurements at Eureka: the entire discussion of the evidence for the dust plume over Eureka Sound is based on Sentinel-2 imagery and the CARRA wind field, which however are not found in the text, while the text only contains the wind pattern at Eureka. Furthermore, this paragraph would play a key role in the manuscript because it would link the in situ measurements to the remote sensing ones, on which the case analysis in Section 4.2 is based. However, Section 4.2 seems to me to be practically independent of the previous one. The example description of the September 8, 2020, event is representative. Figure 5 is very interesting and clear. The reference to Figure S14a doesn't add much, but it makes the reading more difficult. On the other side, Figure S14b with the CARRA wind field could be included in the text to provide a complete description of the case study. To make reading easier, I suggest selecting the minimum number of images/maps essential to support the methodology and results and reporting them in the text, while leaving non-essential figures and animations in the supplementary material.

We agree that Section 4.1.3 is intended to link the in-situ observations at Eureka with the RS evidence, and that the current placement of key figures takes some effort on the part of the reader. The opening paragraph summarizes a weak dust event using multiple sources, including Sentinel-2 imagery and CARRA wind fields, which are currently distributed between the main text (Figure 3 and Figure 4) and the Supplement (Figures S12 and S13).

We have to respectfully disagree with the reviewer concerning the transfer of any part of the supplementary material to the main text. In all cases, the supplementary material provides high-resolution animation features that we think are quite important to the dust plume identification narrative we are trying to make. These animations should not be transferred to the types of fixed and lower resolution images found in the main text of a paper. For example, Figure S12 shows a full resolution Sentinel-2 color image blinking with wind vectors: we think that it is critical to the dust-plume identification narrative to see the full-resolution dust plumes blinking with full resolution wind vectors (we don't wish to weaken these arguments with lower resolution imagery or vectors).

Line 342: if the largest AOD is 0.42, why the legend of figure 5b has a lower value, 0.40 as maximum.

True, but we set the upper limit of the color scale to 0.40 for visualization consistency and for comparisons with the CM AOD scale in Figure 5a. (and, in the end the change to the graphic is practically unobservable since only a few pixels exceed this value).

Line 356: same comment as that for line 342.

Our response is basically the same