

General comment to the reviewer: *the answers to your specific comments are immediately below. We would also point out that we made other changes that we thought would help to clarify the text (as well as the correction of few technical errors and/or typos that we missed in our proofreading before the original submission)*

Review of: “Remote Sensing detectability of airborne Arctic dust” by Norman O’Neill et al.

This paper basically describes the effect that based on brightness temperature dust in the Arctic may have been overestimated and misclassified in the past. It is basically a response to the Bowen & Vicent (2021) nature paper and in this work O’Neill et al. argue that water (clouds) may lead to a systematic overestimation of dust in the Arctic. This is of course a relevant finding. However, I am not very happy with the presentation. Due to the lack of a dedicated result section and a long supplement, which is not ordered I found the paper a bit difficult to read. I recommend to re-structure the paper for better readability.

We tried our best to respond to the “lack of a dedicated result section and a long supplement” comment. But before getting into details of how we responded to that comment we would remind the referee that our article is a general critique of overestimating DOD in the Arctic (with, as the reviewer indicated, a focus on one particular example). The type of results we use in our examples¹ can largely be found in the literature.

We admit however that the $BT_{D_{11-12}}$ vs BT_{11} families of curves in the supplementary material should have been in an appendix of the paper (as per ACP protocols): we accordingly moved former Figure A8 and its discussion to a new sub-appendix of Appendix B (please see the new TOC at the end of this document). This provides the necessary detailed context for Table 1. What remains in the supplementary material are event details in support of various discussions in the main text (and notably animations that we think are important for the discussion of the Mar. 22, 2015 Barrow event).

At some points I ask to provide more information, such that the reader can judge all statements, see my detailed remarks below.

- Line 42: (and later line 74) what do you mean by $AOD\ 0.016 \times 1.5 \pm 1$ and $\sim 0.0023 \times 1.2 \pm 1$? Is the exponent the Angstrom? If so: what does the +/- sign means? To what wavelength you are referring to? How did you derive these values?

The AOD statistics employed in AboEl-Fetouh et al. (2020) are the monthly binned geometric mean and geometric standard deviation (“ $\tau_{c,g}$ ” and “ μ_c ”) as applied to their coarse mode AOD databases). The geometric statistics at Resolute Bay² during the springtime (April & May) Asian-dust event are represented by $\tau_{c,g} \times \mu_c^{\pm 1} = 0.016 \times 1.5^{\pm 1}$. Similarly, the τ_c range during the June, July, and August summertime is $0.0023 \times 1.2^{\pm 1}$. The basics of geometric statistics as applied to CM AODs (as well as FM AODs) are outlined in the AeF citation (see the “Statistical Approach” of Section 3.4 in that paper).

- Line 47: reference to S8: Tell here right away that MORTRAN has been used. Does this refer to an AOD of 1? Is the aerosol distribution log-normal (with which parameters?)

¹ Including the case of negative $BT_{D_{11-12}}$ for a water cloud in the inversion layer (which can be found in Key, 2002)

² in perfect analogy with the ubiquitous arithmetic mean / arithmetic standard deviation expression of $\langle \tau_c \rangle \pm \sigma(\tau_c)$

This is now a reference to the discussion of Figure B1 in Appendix B.1 (where MODTRAN is brought up in the 1st sentence of that appendix)

- If you give AOD values without mentioning the wavelength, do you refer to 500nm?
The reference wavelength of 550 nm was stated explicitly in the footnote on page 2. For good measure we added the 550 nm explicitly in the sentence that contains the footnote in question
- Line 73: provide units for “Arctic dust mass”

“Arctic dust mass” was changed to “Arctic dust mass” with units of mg-m²

- Line 89: Ranjbar paper is it 2022 or 2021?
Citations to both Ranjbar papers (2021 and 2022) are correct (we double checked)
- Given the general title of section 2, this paragraph is quite short. You may also relate to ground-based observations, which are in line with your argumentation, e.g.
<https://acp.copernicus.org/articles/17/8101/2017/>

The reviewer’s suggested paper is clearly a comprehensive analysis but it is rather removed from the issue of remote sensing detectability of columnar aerosol properties (short of performing some rather tedious optical / radiative transfer computations on particle size distribution profiles from the ground-based measurements (extrapolated to the vertical profiles) ... which would be something less than the direct empirical optical proof that we sought in our paper). In response to the reviewer’s “quite short” critique, we replaced our single sentence of (admittedly oversimplified) contextual text by a more substantive supporting narrative (citing a recent paper that we participated in on FM and CM AOD pan-Arctic reanalysis simulations). The first paragraph is now about climatological scale detection of Arctic aerosols while the 2nd paragraph is about the event level mis-interpretation by VCT

- line 152: “Nearly all of our negative BTD ...” This sentence is important and you should be more specific on your data.

This data has now been supplied in the form of supplementary material excel files

- line 159: MODTRAN simulations: you may list in the appendix the important values, assumptions etc. which you have used. Otherwise change the title to ... in the IR from satellite.z

These details were now given in Appendix B.1

- Table 1: I wonder, whether the relations between temperature lapse, emissivity slope and BTD pattern are always so easy and unique (if so: why?) what if there are several layers of dust and cloud in different altitudes? You must not fully answer this, but an idea of the assumptions and limitations behind the results of table 1 are important.

The technical details characterizing those “assumptions and limitations” are given in the text describing Figure B1 and the caption of Figure B1 in the new Appendix B.1.

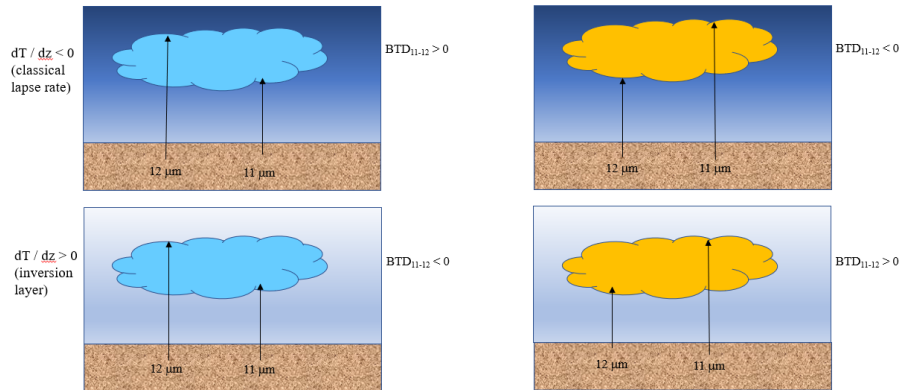
An explanation at the conceptual level (supported by the diagram below) begins with BT_{D11-12} vs BT₁₁ curves being pinned to singular points ($\tau = 0$ and $\tau = \infty$). From a line joining those two

points³ the curves spread concave up ($BTD_{11-12} > 0$) or concave down ($BTD_{11-12} < 0$): the BTD_{11-12} sign depends on whether BT_{11} or BT_{12} from the ground is more penetrating into the cloud/plume⁴ and whether dT/dz across the cloud/plume is negative or positive. No, we did not consider the case of dust and clouds at several altitudes but we do believe that the general behavior of such scenarios can be inferred from the single layer cases.

$d\epsilon / d\lambda$ (from 11 to 12 μm) < 0 (ice or water cloud)
12 μm band is more penetrating from the ground*

$d\epsilon / d\lambda$ (from 11 to 12 μm) > 0 (dust plume)
11 μm band is more penetrating from the ground*

* which is warm relative to the very cold downwelling sky radiation (the upwelling signal dominates the TIR satellite return)



- Line 251: Lhù'ààn Mân is that correct?

Yes. Lhù'ààn Mân' is the Southern Tutchone name for Kluane Lake. Southern Tutchone is one of seven Athapaskan languages in the Yukon and is spoken by Kluane First Nation people.

- Table A2 is not completely clear to me. Can you please confirm or clarify: DOD_m in 3rd column is what you assume to calculate DOD(τ). The last column is what is needed to bring your results in agreement to KA. You are using eq A4 (not 4). If so: the DOD in last column is extremely variable (factor 16 between Resolute Bay and Eureka), I wonder how to interpret this. What are your critical assumptions here?

Our answer here (in order to minimize confusion for the reviewer) is based on the submitted version of the manuscript (before the re-arrangement of sections as described below).

The last column of Table A2 is eliminated in the revised paper (it amounts to an unnecessary distraction that we regret having included⁵). Yes, DOD_m is what we assumed to calculate $DOD(\tau)$. Everything we need to know is really in (old) Table A1⁶. Table A2 (without the irrelevant distraction of the last column) is just a retake of Table A1 with the added element of exploring the consequences of KA's supplementary material finding that their simulations have a negative bias (ΔDOD) relative to CALIOP simulations of local dust. So the column $DOD - \Delta DOD$ addresses the issue of what happens if we apply a CALIOP-inspired correction.

³ whose positions change, respectively with every surface and cloud/plume

⁴ which in turn depends on the "point- volume" emissivity of particles in those two bands for a given type of cloud/plume (ice, water or dust). The relative inter-band, "point- volume" emissivity will be related to the surface emissivity (given for example in VCT's Figure 1)

⁵ It was not an attempt "to bring your [our] results in agreement to KA": it was about an ultimately confusing distraction whose unnecessary goal was to explain the differences between the local dust and Asian dust values of DOD_m

⁶ the new Table A1 is simplified by not being encumbered with repetitive DOD_m information

In our revised paper we point out that, in actual fact, CALIOP “truth” in this case is simply unvalidated (in the end, we make an overarching statement that the amplitude of simulation differences (relative to the AeF climatology) are < 0.002

- Line 336: cancel “a” between “yields” and “an”
The “a” was removed (note that that typo was located in line 356 rather than 336).
- The value of appendix A6 and espec. Table A3 is not clear to me. I would be good to have a statement of the meaning in a broader context.

Indeed Appendices A5 and A6 contained technical information⁷ that was unnecessary (to the point of creating unnecessarily confusing) and a distraction from the key narrative of simply providing DODs that characterized local dust (from Kawai et al.’s mass abundances) and Asian dust (for the April, 2001 event). These unnecessary DOD_m arguments were removed from those two sections (and from Table A2) while retaining the adjusted Table A2 and Table A3 (and providing a broad context for both cases⁸).

We also rearranged Appendix A in general to render its opto-physical development more “bottom up” (with more clarifying titles to accommodate this re-arrangement as well as the transformation of old Appendices A.5 and A.6 into, respectively, new Appendices A.3 and A.4: a Word sample of the new appendix TOC is below). Appendix A.3 includes a new graph (Figure A1) which more explicitly (and clearly, we would argue) compares the AeF CM AODs with the KA DODs for the 4 AeF AERONET sites in the CAA (Canadian Arctic Archipelago). There are still three tables labelled A1, A2 and A3: they have been revised to eliminate everything related to our misguided attempt to compare DOD_m values of local and Asian dust.

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After these revisions I support a publication.

⁷ Most notably, the comparison of DOD_m derived using historical Asian dust information with the DOD_m value derived from the local dust mass abundances of Kawai et al. (2023)

⁸ In the case of Table A3 we added the contextual sentence “This table supports the discussion surrounding the well documented Asian dust event of 2001 and the dust deposition consequences in the Mount Logan region.”