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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Large surface radiative forcing from surface-based ice crystal events measured in the High Arctic at Eureka" *by* G. Lesins et al.

G. Lesins et al.

Received and published: 17 December 2008

Response to review by Dr. S. Warren

We appreciate the very careful reading done by Dr. Warren. In the following we address each of his major and minor comments.

MAJOR COMMENTS

1. We agree that our paper is not about diamond dust but about events that might appear to be diamond dust. We were careful to use the term diamond dust only when discussing earlier work or in the context of its conventional definition. Our paper examines case studies of events that can be misinterpreted as diamond dust but that are,

as pointed out by the referee, residual ice crystals from blowing snow events from the surrounding topography. We added the Walden et al (2003) reference and referred to it in two places in the updated manuscript.

2. We have modified the title to read <Large surface radiative forcing from topographic blowing snow residuals measured in the High Arctic at Eureka> as suggested. We original used the term <surface-based> to indicate that the ice crystals normally extended to the surface, not that the local surface was the source. To avoid this confusion we have dropped that term.

3. We modified the last sentence in the Abstract to read: ‘This work presents .. from residual blowing snow that becomes a source of boundary layer ice crystals distinct from the classical diamond dust phenomenon’. We also added reference to the work of Yamanouchi and Kawaguchi (1985) in the Discussion section.

4. We removed the restriction of <forward> scattering and added ‘halos’, and also added the reference to Tape (1994).

5. We dropped the phrase <generally accepted> and instead indicated that diamond dust likely forms by freezing of aerosols or deposition nucleation with subsequent depositional growth.

6. We avoided cases where the surface observer reported blowing snow. For the 4 case studies that we presented the strongest surface wind speed was 4 m/s but more typically less than 2 m/s which also minimized the possibility of contamination from local blowing snow. We agree that the surface observer cannot really distinguish between ice crystals and residual blowing snow which we mention in the Discussion section.

7. The problem of the lidar minimum height in detecting ice crystal events is addressed 2 paragraphs further down. We recognize that there will be events that are measured by the surface observer but are below the minimum height of the lidar. The case studies presented in this paper considers events which are detected by both the surface

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observer and the lidar.

8. The linear extrapolation of the lidar particle extinction profile from 100 m to the ground is an approximation. We did not know whether this reconstructed part of the profile is linear even if it is linear above 100 m. Hence this becomes a potential error source when comparing the model irradiances with the AERI instrument which is addressed in the Discussion section. The referee points out that a non-linear fit could be attempted since some of the lidar determined profiles above 100 m are nonlinear, but there is no reason to believe that the curvature above 100 m would remain the same below 100 m. Hence we used a linear extrapolation using the measured layer from about 100 to 200 m to the ground. The fraction of the optical depth that was extrapolated varied from 5 to 25 % depending on the depth of the whole layer and the optical depth slope from around 100 to 200 m.

9. and 10. The p value of 7 for the gamma distribution is the default value in SBDART which was used since we do not have a measured size distribution for the ice crystals. Our paper is not examining the properties of the ice crystals nor the details of the size distribution although this is a future goal for us. Instead we varied the effective radius from 25 to 100 microns to test the sensitivity of the SBDART radiative forcing results to a change in the size distribution. The sensitivity was found to be less than 1 W/m2. This and other assumptions are in the Discussion section. We are using an effective radius defined as the ratio of the third to second moments of the radius distribution consistent with the definition used in SBDART. We have added this remark and the source of p=7 after Equation 1.

11. We have removed the first sentence in Section 4.1. It was not our intent to imply that our study is of a climatic nature.

12. We eliminated cases where liquid water might be present because this would represent a different source type for ice crystals. It is well known from SHEBA and other studies that the presence of liquid droplets will greatly enhance the downward IR irra-

diance. We are focusing on cases where only ice crystals are involved. Our original motivation was to understand high optical depth diamond dust but eventually discovered that remote residual blowing snow was responsible. Our paper is not looking at the radiative forcing of all ice crystal events; rather the focus is only that residual blowing snow from surrounding topography that can be misinterpreted as diamond dust and can give a large radiative forcing.

13. We have removed the sentence quoting a result from Yang et al (2005) concerning cirrus particle extinction dependence on size. This was really cherry-picking on our part since it is only one of many studies. We are not attempting to do a literature review of the radiative forcing from nonspherical particles and so to quote one reference is not proper.

14. We agree that observing the ice crystal habits could help to distinguish blowing snow residuals. However this study was conducted after the data was collected from AERI and the lidar and there were no microscopic observations of individual ice crystals. This is something we are planning to do in future trips to Eureka. We added a sentence referring to Walden et al (2003) about the differences between diamond dust and blowing snow ice crystals in our Discussion section.

15. This is a very interesting possibility. We already made mention of errors in the water vapor profile when interpolating between radiosondes 12 hours apart. We have added a sentence about the possibility that some of the increase in radiative forcing could be caused by an increase in water vapor in the layer containing the ice crystals.

16. a. We have added columns for blowing snow cases in Tables 1 and 2. b. We define cloud as any sky fraction above zero.

17. We have removed the sentence about effective radius from the caption of Figure 4 to address another referee’s concern about the caption being too long. In the paragraph preceding Equation 1 we added the sentence: <This is considerably larger than the average ice crystal effective radius of about 12 um measured in the winter

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Antarctica (Walden et al., 2003) because of the warmer temperatures in the Arctic>. We have added Walden et al (2003) to the references.

18. The average inversion height is still being computed and will be added to the final manuscript.

19. From the last 5 rows in Table 4 we see that the top of the ice crystal layer is colder than the inversion temperature because the altitude of the ice crystal layer is less than the altitude of the inversion.

20. The grey color was made darker.

21. The subplots in Figure 3 were rearranged in 2 rows and 2 columns to make them larger.

22. Figure 12 (now Figure 16) has been expanded to cover 3 pages. This makes the labels legible.

MINOR COMMENTS

P17696 Lines 25 and 26: our original text is correct because we are referring to the ice crystal observations from the lidar, not the surface human observers. The numbers of 170 hours and 9.1 % are based on Table 3 not Table 1.

P17698 Lines 6-7: MODIS is not available from 3 to 12 UT because the northern turn around latitude for the satellites is occurring on the other side of the North Pole at 80N and not within sight of Eureka.

P17698 Line 14: Yes, for all four case studies described in this paper there were surrounding clear regions identified by MODIS.

P17698 Line 20: We deleted the qualifier <nearly>.

P17699 Line 11: We included the definition of ECC <(electrochemical concentration cell)> for the ozonesondes used by Environment Canada/University of Toronto at Eu-

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reka.

P17707 Line 25: We clarified the sentence. The uniqueness refers to the frequent daily overpasses at Eureka.

Figure 8d: We added the word <cumulative> to describe the optical depth.

SPELLING AND GRAMMAR

All spelling and grammar corrections were implemented.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 17691, 2008.

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