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Discussions

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Interactive comment on "First Look at the Occurrence of Horizontally Oriented Ice Crystals over Summit, Greenland" *by* Sebastian Cole et al.

Anonymous Referee #1

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After their novel di-annuation lidar technique for the detection of crystal alignment was introduced in a couple of articles published in recent years, the team of authors presents in the underlying manuscript proudly that their lidar system CAPABL is actually capable to collect long-term datasets, even at inhospitable places such as Summit, Greenland.

As an observer I appreciate and honor the efforts which were required for the team to make the system running continuously for the long time reported. Nevertheless, I felt astonished about the main motivation the authors gave for their study, which is the effect of horizontally aligned crystals on the radiation budget of the atmosphere. Because: If the work should really be built entirely around this motivation (enhancing knowledge of HOIC on the rad. budget), the content should be focused on the very same. In the current state I don't think that the results will help modelers to enhance

their radiative transfer models.

While reading through the manuscript I continuously had the feeling that the results are overall interesting but I never got, why the content is so focused on the radiative transfer issue. There is plenty of literature (Platt, Sassen, Thomas, Westbrook,....) available about the nature and effects of HOIC and also their consequences on, e.g, remote-sensing measurements are discussed. For the first, I would see a much bigger strength of the study if it would focus on:

- Introduction: Emphasizing the overall interest in HOIC, giving an overview on existing studies. - Results: Exploring the nature of HOIC in the Arctic - Discussion: Putting the findings for the Arctic in context to existing studies from other regions

I see high potential for a future study that is related to the radiative effect of HOIC, but this should be considered as a next step. In the current version of the manuscript not even quantitative examples are provided about what effect might be expected under Arctic conditions.

In addition to my major comment above, I see several addition major issues which should be addressed in the revised version of the manuscript:

1) Columnar particles: How do columnar ice crystals affect the diattenuation measurement? Are they included in the HOIC category? If the authors want to stay with the radiation-budget focus: What is the expected difference of the radiative effect of columnar vs. hexagonal vs. plate-like crystals?

2) Context to existing studies: When looking on existing studies, the fraction of HOIC seems to be much higher when the standard approach (zenith/off-zenith intercomparsion of the returned lidar signal) is used. How can this difference to the bi-attenuation approach be explained?

3) Is there really an effect of HOIC on sunlight approaching at elevation angles <40°? There is some discussion on page 3, but is this valid for the Arctic?

4) What is the likely contribution of HOIC scattering to the overall error in radiative transfer calculation? Studies as those of Eichler et al. (2009), show for instance, that the assumption of a certain particle shape introduces uncertainties of around 70% in optical depth and 20% in effective radius.

5) Taking into account the 'dark side' of the profile: I see a need for a much more thorough discussion of the amount of time the lidar could not probe the whole atmospheric column because of lower cloud decks obscuring the sky. E.g., it is written in the manuscript that HOIC in cirrus were only detected in February. But it is nowhere discussed if this could be just due to the simple reason that high clouds could not be observed during the other month because of a permanent low-cloud deck. Existing lidar-based cloud statistics (E.g., Kienast et al., 2015 or Gouveia et al., 2017) are a good starting point to evaluate who this representativeness error could be accounted for.

6) Figures 1,2: Backscatter ratios of around 20 and less are defined as 'clear' pixels in the data mask. How is the backscatter ratio defined. E.g., in Kienast-Sjoegren et al., 2016, a BSR of larger than 1.08 (even 1.03 at night) was used to identify cirrus clouds.

7) Abstract: What does it mean when HOIC were observed on 86 days of the 335-day study? What happened on the other days? Low clouds? No clouds? Different clouds? Or is the value '86 days' stating that clouds were observed during in profiles spanning 86*24 hours of the total measurement time?

8) Introduction of the di-attenuation technique. This is briefly done in Sect. 2. and 2.1. But at least the overall principle of di-attenuation measurements should be presented somewhat more extensive (by means of words). The reader is also kept alone with the expression put into parentheses in Sect. 2.1 "80 seconds for a complete 4 polarisation retrieval of diattenuation". I don't agree if the authors expect the reader to study all previous publications to understand this sentence.

9) Definition of the observed variables. The text discusses depolarization, diattenu-

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ation, backscatter ratio. How are the parameters defined? Is it volume or particle depolarization?

10) Wind-speed discussion: Wind speed and direction may vary strongly in the course of the day. Is it really feasible to interpolate the wind fields between two radiosondes? How are wind speeds related to temperature? Is there maybe a co-variance existing between temperature, wind speed and HOIC occurrence?

11) RH discussion: Relative humidity is likely even more strongly variable than the wind field. I would recommend to omit sections dealing with RH effects on HOIC. Or only time periods with in plus/minus 1 hour, or so, around a sounding should be discussed. Also, why is the discussion based on RH with respect to liquid water? I would consider RH with respect to ice to be much more important for the formation of HOIC.

12) HOIC frequency: Is the analysis pixel- or profile based? E.g., in Sect. 3.2, it is stated that HOIC occur with 8.1% and 6.3% frequency in January and December. What is the basis for this frequency? Data points? Or profiles? This is not made clear in the methodology section.

13) Definition of cloud types: How are the cloud types (cirrus, stratiform, precipitation) defined?

14) Sect. 4.1: What is the 'formation process of Shupe et al (2013)'?

In order to get the article published in ACP, it should receive a major revision. As stated several times above: The authors should consider moving the focus/motivation from the very strong view on radiative transfer to a more-broad scope.

References:

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