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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Numerical simulations and Arctic observations of surface wind effects on Multi-Angle Snowflake Camera measurements" *by* Kyle E. Fitch et al.

Anonymous Referee #3

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Review of AMT-2020-296

This manuscript summarizes work focused on snow particle observations from a MASC deployed at the NSA site. Specifically, the authors investigate the impact of wind shielding on the MASC instrumentation both from field measurements and from Computational Fluid Dynamics (CFD) simulations. The authors find that the fall speeds from the MASC agreed much better with Doppler velocities from a co-located KAZR for the wind shielded events. Additionally, the CFD simulations indicated slower particle fall speeds when the MASC was unshielded. In general, I think this work is novel and advances the field of in situ snow particle observations. I would also like to commend the authors on an exceptionally well-written manuscript, which had a clear narrative and was enjoyable to read.

I have one major comment and a few minor that should be addressed prior to publication:

Major Comment:

It is unclear to me how many distinct events were used to comprise the observations that were presented. In the methods, the timelines of the MASC deployment unshielded (Feb 2015 – Aug 2016) and shielded (Aug 2016 – Aug 2018) are outlined (33 months total), however it is not discussed anywhere how many independent events are used in this work. This is key information that is missing from this manuscript as it lends weight to the differences seen between unshielded and shielded observations. This is especially true for Table 3 – as the observations are further divided into wind speed bins and by particle type. A single event could produce 1000s of particle images, so it should be made clear how many independent events were used. This should be added to the methods section – ideally as a table (dates, times). Currently, the manuscript reads as if there are enough observations to say that these fractions of different particle types (in Table 3) are due primarily to the wind shielding impacts, however if there is a low number of independent events (or a low number in a represented wind speed range), then some of these particle type ratios could be from different synoptic or thermodynamic forcing. In addition to including the number of events, the authors should also examine the statistical significance of these differences in particle type (rimed, MR, agg.) for the various wind speed bins (if the N of individual events is large enough).

If only a few independent events were used in this work, I think this should be made clear and the language should reflect that is the case. The implication in the paper (whether purposeful or unconscious) is that the differences in particle type ratios seen in shielded versus unshielded at various wind speeds are a product purely from mitigating the wind to the MASC. However, if very few independent snow events were used in these comparisons the synoptic and thermodynamic conditions could be influencing the ratios of rimed, MR, and aggregate particles.

Minor Comments:

Figure 2 illustrates a CFD simulation across the MASC in the +y direction, which is roughly parallel to the cameras that protrude above the opening. And Fig. 3 shows the impact of the fall speeds for ambient winds in both +y and –x (which I read to be winds toward the cameras). What is the impact of the winds originating from behind the cameras, as this is a large obstacle adjacent to the observing ring? I assume that this direction (+x) will have a larger impact on the particle fall speeds (if I am reading the orientation of the axes correctly). Did you do simulations with the wind originating from behind the cameras?

Along those same lines, wind direction impacts were noted in the discussion about the simulations (minimal), but not in the observations. Was there any analysis on the impacts of wind direction from the observational perspective?

The MASC fall speeds were compared to the KAZR Doppler velocities, and it seems that the mean Doppler velocity from the cloud base to near-surface (I assume) profile was used – is that correct? If so, what was the lowest near-surface bin used in the Doppler velocity profile mean calculation (assuming near-surface to cloud base mean value)? Also, the snow particles can change between the cloud base and the surface – so what is the advantage of using the mean DV value of the profile (near-surface to CB) versus simply using the near-surface Doppler velocity? My instinct is that using the near-surface Doppler velocity value would give you a more direct comparison to the MASC

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