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# *Interactive comment on* "Observations of cloud microphysics and ice formation during COPE" *by* J. W. Taylor et al.

**Anonymous Referee #1**

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The manuscript describes aircraft and radar observations in a line of small cumulus with bases at about +11 C and tops around -15 C. This review focuses on observations from the BAE-146 measurements in relatively new, isolated updrafts that have not been contaminated with ice from older convection.

The main point that needs to be corrected is that the manuscript consistently and persistently attributes high ice concentrations observed in the -3 to -8 C region to the Hallett-Mossop (H-M) secondary ice formation process. In several places the manuscript states that there are sufficient drops smaller than 13 microns and larger than 24 microns in the presence of graupel between -3 and -8 C, and then attributes measurements of relatively high ice concentrations to the H-M process. The manuscript goes on to make the case that the H-M process supplies ice splinters that



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freeze the remaining supercooled drops. However, the observations do not provide confirmatory evidence for the conclusion that the H-M process is actually operating. The observations demonstrate the following:

1. H-M conditions are met in certain regions of clouds.

2. Relatively high ice concentrations are observed

3. Ice crystal habits are typical of growth in the temperature range from – 3 to – 8 C.

4. Large supercooled (drizzle and rain) drops appear to freeze before smaller cloud drops freeze.

From these observations the manuscript concludes that the H-M process is responsible for producing small ice splinters that then collide with and freeze the large supercooled drops. While the H-M mechanism could be responsible for the production of small ice and subsequent freezing of larger supercooled drops, it is not the only process that could be responsible. Secondary ice production in actual clouds is not well understood and not well quantified. The manuscript tends to build its case on circumstantial and sometimes facial evidence. For example, the production of small ice via drop freezing, spicule formation and ejection of small ice is another mechanism (demonstrated in the laboratory by Thomas Leisner). However, this mechanism is dismissed in the manuscript via the following discussion:

Lawson et al. (2015) stated that the presence of drops larger than 200 microns in updrafts At – 6 C was required for significant ice enhancements by droplet fragmentation.

The actual verbiage from Lawson et al. (2015) is:

“Model runs were also conducted by varying the initialized DSD and the initial ice PSD. If the DSD at -6 C does not contain drops larger than about 200 micron in diameter, the conversion to ice via drop-freezing secondary ice production and riming is greatly diminished, resulting in less ice and more supercooled water being transported higher in the cloud.”



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The manuscript has taken the results from a cloud model used as an aid in the interpretation of the observations and implied that the Lawson et al. (2015) stated a conclusion. This is very inappropriate and misleading. The manuscript goes on to state:

"The mean concentrations of NRound > 200 $\mu$m in updrafts in runs 11.1, 11.4 and 13 were 0, 3.6 and 0.9 /L respectively. The average number of fragments expected from a 200 micron drop is 0.04 (Lawson et al., 2015, Fig. 12), meaning if all these drops were to freeze in the H–M zone only a minimal enhancement would be expected."

This is a misinterpretation of Fig. 12, which shows statistically the number of fragments a drop would produce based on the model results. Because this is a statistical result, the proper interpretation is that statistically, the model predicts that one in every 25 drops that are 200 microns in diameter will produce an ice particle. However, this is a cascading process, whereby this ice particle could freeze a millimeter-diameter drop that could produce hundreds of ice particles, and so on, producing rapid glaciation. The statement in Lawson et al. (2015) is sharing a generalized result form the model and this not sufficient justification to eliminate one secondary ice process in favor of another.

This reviewer is not saying that the H-M process was not operating in the subject clouds, that the spicule/ice production process was operating, or that some other (perhaps unknown) secondary ice process was or was not operating. The point is that there is not enough evidence to come to the conclusion that H-M was responsible for the relatively high ice concentrations. The conclusions are built on a house of cards.

For example, from the manuscript: "The crystal habits and ice concentrations of hundreds per litre make it clear that these particles were generated by the H–M process, and were lifted further up in the cloud by updrafts." This statement cannot be proved. What the observations do indicate is that ice with habits characteristic of a certain temperature range were measured in concentrations of hundreds per liter and observed in colder (higher) regions of the cloud.



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The manuscript needs to be significantly modified so that the H-M process is not "promoted". Instead, the observations should be presented, the interpretation that the measured ice concentration far exceeds what is expected from primary nucleation, and that a secondary ice process may be responsible for the high ice concentrations. If desired, the manuscript could then list some candidate processes (e.g., H-M, spicule formation, drop fragmentation, crystal-crystal shattering, etc.), and also state that the basic H-M conditions are met in and near the regions where the high ice is observed. But there is not enough evidence to conclude that the H-M process is responsible. It is also inaccurate to state that the spicule ice formation process is not responsible for the high ice concentrations, because not enough is known about that process. Observational scientific papers should present the data and offer interpretations, not piece together circumstantial evidence in an effort to come up with an explanation.

There are also several minor issues that need to be addressed in the paper. For example, the Abstract overstates the certainty of existence of the H-M process:

"It is therefore clear that the freezing of supercooled drizzle drops not only provides a pathway to advance the onset of the H–M process, it also accelerates glaciation and the formation of precipitation once it has begun."

The observational evidence indicates that the measurements of ice concentration in the – 3 to – 8 C temperature range (i.e. the H-M region) greatly exceed those expected from primary nucleation. However, the manuscript states that freezing of supercooled drizzle provides a pathway to advance the onset of the H-M process, which is an implicit confirmation that the H-M process is responsible for the increased ice concentration. There is no direct evidence that the H-M process is actually operating, only circumstantial evidence, so these types of implicit confirmations of the H-M process need to be eliminated.

Another example: The manuscript cites the Harris-Hobbs and Cooper (1987) technique for estimating the number of ice particles produced by the H-M process. However, this



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approach, while appropriate in its day, uses 30-year old technology, including optical probes without anti-shattering tips, and the results are not applicable today.

This reviewer cannot recommend publication until the major issue, overly promoting the role of the H-M process, which is described in detail in this review, is rectified. The paper needs to focus on the observations, not the H-M process, which is mentioned 57 times in the manuscript. This reviewer recommends that the manuscript be revised to: 1) compare the observations to ice concentrations expected from primary nucleation, 2) point out that the ice concentration measurements far exceed those expected from primary nucleation, 3) show how the role of freezing of supercooled drizzle may play a role, 4) explore the possibility that there is an active secondary ice production process active, and 5) point out that the ice number concentration enhancement falls within the conditions defined by the H-M mechanism and that H-M is one of the possible candidates.

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