A review and enhancement of Chapter 7 of AMS Monograph 58:
"Secondary Ice Production: Current State of the Science and Future Recommendations"

by Field et al. 2017

Arthur L. Rangno[1]

Catalina, Arizona 85739

Abstract

This review consists of three elements: 1) reprising the Hallett-Mossop process and why it cannot explain, of itself, high ice particle concentrations in Cumulus clouds with slightly supercooled tops; 2) a review of the contents of Chapter 7 consisting of selected quotes followed by comments, similar to a formal manuscript review; corrections that should have been caught before Chapter 7 went to press.

Literature that was uncited in Chapter 7 that might have altered, and in some cases, enhanced some of the authors' conclusions, is discussed.

1. Introduction

Field et al. (2017, hereafter, F2017) have done a remarkable job of summarizing a vast amount of work on the continuing enigma of the origin of ice-in-clouds. Not surprisingly,

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[1] Retiree, Cloud and Aerosol Research Group, Atmos. Sci. Dept., University of Washington, Seattle.

considering the abundance of publications in various journals relevant to this mystery, some publications were overlooked that might have helped the reader, and altered some of the conclusions wrought in F2017. This review is meant to "fill in" those blanks; to be an enhancement of Chapter 7 rather than a series of criticisms. It is restricted to the cloud microphysical portions of Chapter 7 concerned with ice multiplication in Cumulus clouds, the writer's specialty.

The "embarrassment of citation riches" to much of our prior University of Washington work[2], is much appreciated. Nevertheless, since it is not possible to be cited too many times, only too few, we dredge up even more of our work relevant to the question of secondary ice that went uncited. The comments contained in this review will range from picayunish errors in F2017 (left until the end) to more significant commentary concerning the workings of the H-M process at the beginning of this review. This is followed by quotes in F2017 followed by comments, a style mimicking that of a pre-publication review, one that I wish I had participated in.

We start with a summary of the Hallett-Mossop riming-splintering process (Hallett and Mossop 1974; Mossop and Hallett 1974, hereafter "H-M") and why the H-M process cannot, of itself, account for the "rapid" development of ice in clouds that F2017 mentions in their abstract. In reading Field et al. it was felt that this distinction between clouds that produce ice rapidly and the inability of the H-M process alone to do that in slightly supercooled Cumulus clouds, beginning with primary ice nuclei (IN) was not made clear.

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[2] Hobbs and Rangno 1985, 1990, and 1998, hereafter HR85, HR90, and HR98, and Rangno and Hobbs 2001 and 2005, hereafter RH2001 and RH2005.

Relevant literature that was not cited or possibly not known about by F2017 is indicated by an “u” after the citation in this review, for “uncited.” ‘

2. Review of F2017

The rapid development of precipitation in Cumulus clouds transitioning to Cumulonimbus clouds, has been noted for many decades via radar (e.g., Battan 1953; Saunders 1965, Zeng et al. 2001) and by aircraft (e.g., Koenig 1963). A process that can explain such rapid transitions in clouds whose tops reach much above the freezing level must act very quickly (<10min) to enhance concentrations of ice particles in such clouds. The H-M process is one that is usually cited in conjunction with this rapid formation of ice. However, of itself, even when the broad droplet spectra is satisfied in a Cumulus turret with a top at -8°C with only primary ice nuclei (IN) as ice initiators, such a cloud can never attain the 10s to 100s of ice particles per liter associated with “ice multiplication”, those in modest Cumulonimbus clouds.

Why can’t the H-M process alone produce significant ice in Cumulus clouds when its criteria are satisfied?

The lifetime of Cumulus turrets is too short, <20 min (e.g., Workman and Reynolds 1949u, Braham 1964u, Saunders 1965u). Its too short for several cycles of splinters to develop, those having to reach fast-falling graupel sizes to be significant splinter producers, starting with ice particles from the very few primary ice nuclei (IN) at -8°C. Even the H-M droplet spectra itself is doomed within a few minutes in the lives of ordinary Cumulus turrets as they fall back and evaporate[3]. Mason’s (1996) calculations, using reasonable assumptions, required 1 h for ice

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[3] Exceptions might be those situations where fresh turrets rise up through remains of turrets in calm or nearly calm situations.

particle concentrations to reach 100 l^{-1} after starting from primary IN, which Mossop noted was untenable for a Cumulus turret. Chisnell and Latham (1976) understood this: “Firstly there are some reported multiplication rates, 10 in 8 min (Mossop *et al.* 1970), 500 in 5 ~ min (Koenig 1973-*sic*), which are inexplicable in terms of a 'riming only' model, but which are consistent with a model containing rain drops.”

. Absent larger (>30 μm diameter) droplets and/or precipitation-sized drops (≥100 μm diameter), tens of minutes to an hour or more is required to raise ice particle concentrations from from primary IN concentrations to 100 l^{-1} (e.g., Chisnell and Latham 1976, “Model A”, Mossop 1985a,u, Mason 1996), times that are not tenable considering the short lifetimes of Cumulus turrets.

Moreover, air translates through Cumulus clouds analogous to lenticular clouds though at a far slower pace (e.g., Malkus 1952u, Asplinden et al 1978u). Thus, while a Cumulus cloud can appear to exist for tens of minutes, its individual turrets cannot. Any splinters that might be formed by a round of very sparse graupel due to primary IN, should an ice crystal have time to become a graupel particle, will go out the side or evaporate as the top declines and evaporates toward the downwind side as illustrated in Byers (1965u, Figure 7.3). One of the lessons learned in the HIPLEX seeding experiments when dry ice, dropped like graupel into supercooled Cumulus turrets, was that it produced ice crystals that drifted out the side of decaying cloud portions (Cooper and Lawson 1984u).

Mossop (1985a,u) himself had trouble explaining the rapidity of ice development in his own Cumulus clouds in the Australian Pacific. Using his measured concentrations of frozen drizzle drops as an accelerator of ice formation, Mossop calculated that it would take about 47 minutes to go from initial ice concentrations due to primary IN (0.01 per liter) at -10°C to 100 ice particles per liter. Mossop knew that this amount of time was untenable for a Cumulus turret. He then reasoned that IN must be about 10 times higher at -10°C to explain that discrepancy, or about 0.1 per liter, to bring the glaciation time he observed down to about 20 minutes (calculating that the concentrations of ice particles increased 10 fold each 10 min beginning with 0.1 IN per liter active at -10°C). The concentration of IN surmised by Mossop (1985a, u) is now close to that in updated concentrations of IN by DeMott et al. 2010 of about 0.3 per liter active at -10°C[4].

However, IN need to be about 10-100 times higher than Mossop's estimate of 0.1 per liter to bring down the time of glaciation to that observed in clouds like his own Australian clouds, namely, ones containing copious droplets >30 um diameter and some precipitation-sized drops. This was demonstrated by Crawford et al. 2012's case of 100 times the DeMott et al. primary IN with a model cloud top at -10°C, a case study that best mimicked the near-spontaneous glaciation of real clouds having modestly supercooled tops and containing drops >30 µm diameter (often with drizzle or raindrops).

In sum, if the droplet spectra does not broaden considerably farther so that droplets larger than 30-40 µm in diameter are in plentiful concentrations (past the Hocking and Jonas 1971;

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[4] It is interesting to note that aufm Kampe and Weickmann (1951) produced virtually the same ice nuclei activity graph as found in DeMott et al. 2010. Also noteworthy is that Blanchard (1957) also using outside air in a city environment as did aufm Kampe and Weickmann, could freeze giant raindrops in a vertical wind tunnel at the same temperatures that this occurs in natural clouds.

Jonas 1972) thresholds for collisions with coalescence to begin, there will be no “rapid” glaciation in slightly to modestly supercooled clouds that only meet the H-M droplet spectra criteria.

3. Descriptions of ice multiplication in literature uncited by F2017

Our follow up studies of ice development in Cumulus and small Cumulonimbus clouds after HR85 and HR90 went uncited in F2017. Those were Rangno and Hobbs 1991u and 1994u, hereafter RH91u and RH94u. We offer a brief summary of our findings before moving on to other relevant uncited findings. We believe that these uncited papers, *en toto*, cast additional light the nature of the problem posed by ice multiplication.

a. Discussion of RH91u with some background on HR85

In our prior study of ice-in-clouds, HR85, only a 6 s time resolution was available for data during most of the sampling period (1978-1984). Therefore, we sampled rather wide cloud complexes to get meaningful statistics. In addition, our 2-DC probe was only operated sporadically, not continuously in cloud.

In RH91u data resolution was 1 s or less, and there was continuous 2-DC coverage of cloud penetrations. Moreover, we carried a vertically-pointable (up or down), mm-wavelength radar, perhaps the first cloud research aircraft to do so.

We often sampled much smaller clouds than in HR85 and we found that maritime, short-lived (<1 km wide) “chimney” Cumulus clouds whose tops fell back into warmer air and evaporated, did not produce much *detectable* ice even if they reached close to -10°C. This was true even as their wider, nearby brethren with the same cloud top temperature produced “anvils of ice”, replicating the findings in HR85 (see RH91u, Figure 1). The low ice concentrations found in chimney Cumulus clouds could also have been due to not being able to sample very small ice crystals, those below about 100 µm in maximum dimension. It forced us to reconsider the role of evaporation that we posited was important in the production of ice in HR85.

The finding in RH91u that wider clouds had considerably more ice corroborated Mossop et al.'s 1970 and Schemenauer and Isaac's (1984u) earlier findings that cloud width had a profound effect on the development of ice in clouds. These findings implicitly address the importance of the duration of cloud and precipitation-sized drops, if any of the latter, at lower temperatures.

Of note is that the maritime Cumulus clouds in Washington State coastal waters during onshore flow are virtually identical to those studied by Mossop and his colleagues in the Australian Pacific in terms of cloud base temperatures, droplet concentrations, ice particle concentrations and in the minimum cloud top temperatures at which most sampling took place (e.g., Mossop et al 1968u, Mossop and Ono 1969u). Our studies were, thus, an attempt at replicating the findings of Mossop and his colleagues without going to Australia.

In RH91u, we found again, as noted in F2017, that Mossop's (1985a, u) report that ice concentrations required 20 min to rise from 0.1 per liter to 100 per liter, was still too great an amount of time to account for the rapidity of the glaciation that we observed in our Washington clouds. Lawson et al. (2015) have arrived at a similar conclusion recently though in a different way.

In RH91u we also compared the explosive formation of ice in our maritime Cumulus to our prior dry ice cloud seeding experiments (Hobbs 1981u) and again in RH94u. The imagery is remarkably similar as a demonstration of the rapidity, the virtually spontaneous formation of ice[5]. We thought that an important comparison.

We also investigated the ocean's influence on ice formation by sampling small to medium Cumulus clouds that developed out of clear air in an extremely cold[6], offshore flowing air mass over the Washington State coastal waters. Cloud bases were -18°C and cloud tops of the deepest Cumulus, -26°C. The sea surface was roiled by estimated 25-40 kt winds with widespread whitecaps. Mixing from the sea surface, about 13°C, to cloud bases was extreme, as marked by the heavy turbulence on that flight and vomting. We sampled those cumuliform clouds as they deepened downwind as far as 100 km offshore that day.

That day stood out in our studies. We measured the lowest ice particle concentrations in all our sampling of cumuliform clouds with top temperatures -24°C to -26°C by measuring maximum concentration of only 7 l^{-1} in clouds up to about 1 km in depth. This day forced us to conclude that the coastal waters of Washington State, anyway, were not a source of high temperature ice nuclei, counter to some more recent work (DeMott et al. 2016). However, we did not measure concentrations of ice particles that were < 100 µm in maximum dimension.

The droplet spectra in those offshore flowing clouds was narrow, as would be expected with such low base temperatures, and again the idea that droplet sizes control ice formation was once again realized by these low concentrations of ice.

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[5] We also found it difficult to arrive at that moment of "explosive" ice development with our aircraft.

[6] The Quillayute, WA, rawinsonde 500 mb temperature was -45°C the morning of our flight!

In sum, from our attempts at replicating Mossop's results in clouds identical to his over many years, we found several departures in ice formation from the operation of the H-M process as it was being described. These discrepancies are somewhat different than those quoted for our research in F2017, hence we reprise them here:

- The formation of ice was far more rapid in clouds with tops between -5°C and -12°C than could be accounted for by H-M, requiring ≤ 10 min, as judged from the small size of the ice particles in high concentrations, ones that had not yet had time to begin forming aggregates; moreover, they were usually coincident with relatively high LWC that had not had time to be depleted (e.g., HR90, RH91u). Newly risen turrets full of LWC could be seen to transition to an icy, fraying, soft, cotton-candy appearance in less than 10 min. What cloud observer hasn't seen this behavior?
- Our maritime clouds had very low concentrations of small (≤13 µm diameter) droplets once appreciably above cloud base and into the H-M temperature zone. Low concentrations of small droplets were once thought to be an impediment to riming and splintering (e.g., Mossop 1978u; Hallett et al. 1980u), though later studies deemed them to have only a "secondary role" (Mossop 1985b).
- Measured graupel concentrations, despite our "optimizations" (using high concentrations over a few meters rather than turret-averaged) to try to make H-M work in RH91u were still not high enough to account for the high concentrations of ice particles that developed so quickly.
- Our fast-glaciating, modest Cumulus and Cumulonimbus clouds with tops between -5°C ≥-12°C did not contain mm-sized raindrops, thought to be critical for rapid glaciation as asserted by F2017. However, copious large droplets (≥30 µm diameter) and drizzle-sized

drops up to about 500 μm diameter were always found, though the latter in relatively low concentrations[7,8]. Drop sizes between 30 μm and 60 μm diameter, deemed an important player in ice multiplication by Ono (1972u), were always copious.

b. Discussion of RH94u

The focus of RH94u was to remove the effects of the H-M process by studying ice development continental and semi-continental clouds found mostly east of the Cascade Mountains of Washington State, clouds that did not meet the H-M criteria. We believed that this was an important next step. The clouds we sampled almost always had base temperatures of 0°C or lower. Droplet concentrations were semi-continental to "continental" ranging from 300 cm^{-3} to 1500 cm^{-3}, many times higher than droplet concentrations in the Washington coastal waters in onshore flow that averaged but ~50 cm^{-3}. Thus, the droplet spectra in the eastern Washington and other cold clouds we sampled were considerably narrower than in our coastal clouds, and due to those cold bases, contained few if any drops meeting the large droplet size ($\geq$23 μm) in the H-M temperature zone. We again carried our vertically-pointable, mm-radar to help elucidate cloud structures below or above the aircraft.

Our findings for the eastern Washington State clouds, simply explained, were that the higher the cloud base temperature, the greater the ice at in a Cumulus cloud, holding cloud top

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[7] We note that in the cloud studied by Mossop (1985u) a drop of 1.5 mm diameter was encountered.

[8] If Ono (1972u) was correct about the importance of drops between 30 μm and 60 μm diameter, then we may have been barking up the wrong "ice tree" by concentrating on drizzle and raindrop sizes.

temperature constant. Thus, a cloud with a base of -15°C and a top of -20°C had far *less* ice than a cloud with a base of 0°C and a top at -20°C with no contribution from H-M. This finding spoke to, as we believed then and continue to believe, the largest droplet sizes of the spectra as being a critical parameter in the production of ice. We continued to find that a measure of the broadness of the FSSP-100-measured droplet spectrum (our "threshold diameter", or large end "tail" of the droplet spectrum, e.g., HR85) in newly risen turrets lacking much ice ($\leq 1\ l^{-1}$) continued to be strongly predictive of later maximum ice particle concentrations.

We also found that for very cold based clouds ($\leq$-8°C) that Fletcher's (1962u) summary ice nucleus curve predicted ice concentrations associated with a range of cloud top temperatures extremely well (r=0.89). This probably indicated that we had little contribution from probe shattering artifacts after accounting for them (see RH91u). The crystal types in those clouds were almost all delicate stellar and dendritic forms where shattering artifacts would be expected to be rampant[9].

Too, ice formation in the eastern Washington State clouds, as it was in our maritime clouds, was extremely rapid, explosive, in turrets with larger droplets ($\geq$~25 µm in diameter) as they reached their peak heights with no contribution from H-M. As with our maritime clouds, the scenario of a few much larger particles (graupel) appeared to be coincident with wholesale formation of high ice concentrations.

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[9] While tedious, we inspected all our 2-D imagery in our Cumulus studies for artifact problems; we didn't just crunch numbers without looking at every 2-D strip!

This did not happen, however, in very cold-based (≤-8°C), shallow clouds with small (~≤20 μm diameter) droplets and tops down to -27°C where ice appeared to form from a "trickle" process likely due to ambient IN concentrations rather than aided by other factors.

Too, our evaluation of the H-M process could not explain the ice multiplication that occurred in those few eastern Washington clouds that did meet the H-M criteria. In our calculations we used a "relaxed" FSSP-100 spectra (as lately invoked by Crawford et al. 2012) that resulted in more >23 μm diameter droplets than were actually observed in our calculations to no avail in an attempt to "break" our conclusions (as good scientists do).

Two very short but illuminating papers were published in 1998 that discussed two viewpoints concerning the H-M process. Blyth and Latham (1998u) "Commented" on the University of Washington findings[2] as completely explicable due to the H-M process, counter to the conclusions stated in our papers in which we felt that H-M might be playing a lesser role. We defended our findings in our reply (Hobbs and Rangno 1998u)[10].

Following Mossop's (1978) nomogram for ice development and ice multiplication boundaries given cloud base temperatures[11], we evaluated the onset of ice based on cloud depth and temperature of the onset of ice in Cumulus clouds using cloud base temperatures for continental clouds in Rangno and Hobbs (1988u), updated with many more data points from

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[10] This colloquy also emphasized an extremely important point in science; we should speak out on findings that we question instead of remaining on the sidelines. We admired Blyth and Latham for questioning our work. After all, we could be wrong!

[11] Isaac and Schemenauer (1979), however, criticized Mossop's 1978 nomogram; Mossop (1979) responded politely with more supportive data.

various locations around the world in Rangno and Hobbs 1995u (Figure 12). These data, for non-severe convection, point to a critical role of droplet sizes as proxied by cloud depth for the onset of ice in clouds (as Ludlam 1952) first noted), and, thus when ice multiplication can be expected.

c. Other uncited findings that impact F2017

Perhaps the most remarkable instance of "secondary" ice formation was left out of the field studies described by F2017: that of Stith et al 2004u in clean tropical updrafts. Stith et al. reported tens of *thousands* per liter of spherical ice particles in tropical updrafts that led to nearly complete glaciation by -12°C and total glaciation by -17°C. As Stith et al. pointed out, and was obvious, there is no mechanism presently known that can explain those observations. The remarkable findings of Stith et al. should have been "front and center" in F2017. (Or, it should have been called out as bogus in a footnote.)

Another finding, one that resembles the findings of Stith et al. 2004u, and is also inexplicable by H-M, is that of Paluch and Breed (1984u). High ice particle concentrations (100 l^{-1}) formed in a Cumulus cloud updraft at moderate supercooling.

Other examples of H-M "exceptionalism" that went uncited in F2017: Cooper and Saunders 1980u, Cooper and Vali 1981u, Gayet and Soulage 1982u, Waldvogel et al 1987u.

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4. A line-by-line critique of F2017, analogous to a pre-publication manuscript review.

P7.1: F2017, their introduction: "Airborne observations of ice crystal concentrations are often found to exceed the concentration of ice nucleating particles (INPs) by many orders of magnitude (see, e.g., Mossop 1985; Hobbs and Rangno 1985; Beard 1992; Pruppacher and Klett 1997; Hobbs and Rangno 1998; Cantrell and Heymsfield 2005; DeMott et al. 2016). In the 1970s (Mossop et al. 1970; Hallett and Mossop 1974) the discrepancy between expected ice particle concentrations formed through primary ice nucleation and observed ice particle concentration motivated the search for mechanisms that could amplify primary nucleation pathways.”

While it was gratifying to have our work cited in the Introduction of F2017, the observations of unexpectedly high ice particle concentrations at slight supercoolings (≥-10°C), goes no farther back than Mossop et al. 1970. One wishes some the earlier workers who reported ice at unexpectedly high cloud top temperatures would have been cited in this first grouping[12], such as Coons and Gunn 1951u; Ludlam 1955u; Murgatroyd and Garrod 1960u; Borovikov et al. 1961u; Koenig 1963; Hobbs 1969u; Auer et al 1969u.

P 7.2, Section 2, F2017: “The consensus is that H-M occurs within a temperature range of approximately -3°C to -8°C, in the presence of liquid cloud droplets smaller than ~13µm and liquid drops larger than ~25µm in diameter that can freeze when they are collected by large ice particles (rimed aggregates, graupel, or large frozen drops).”

It is now believed that the small droplets play a far less important role than once envisioned. Goldsmith et al. (1976), later confirmed by Mossop (1978) appeared to find strong

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[12] It has been said that references to ground breaking early work is disappearing in publications due to the presence of younger authors.

evidence that droplets ≤13µm diameter played a critical role in ice multiplication. In fact, it was thought for a time that very low concentrations of those small drops would lead to clouds absent in ice multiplication in clean locations (e.g., Hallett et al. 1980u). However, Mossop 1985a, u himself, in later laboratory experiments determined that small drops played a much-reduced role in H-M. Cloud studies in pristine environments where ice multiplication was rampant (RH91u in the Washington State coastal waters in onshore flow, HR98 in the Arctic, Rangno and Hobbs (2005) in the Marshall Islands, and Connolly et al. (2006a) in England, would seem to have confirmed the minor role of droplets ≤13 µm diameter in riming and splintering in clean conditions.

Section 2, p7.3-7.4: The F2017 Table 1 and the discussion of laboratory and field observations of secondary ice particles.

While Section 2 was remarkably thorough, some important findings were not cited, or listed in Table 7.1 of the many studies of secondary ice particles. Ono (1971u, 1972u) should have been included in Table 7-1 and in the accompanying F2017 discussions; he appears to have preceded Hallett and Mossop (1974) concerning the importance of larger cloud droplets coincident with graupel in ice multiplication[13]. Two elucidating quotes from Ono:

Ono (1971u), his abstract:

"(Ice crystal) sizes, concentrations and microphysical conditions of occurrence support the hypothesis that they were formed when ice fragments were thrown off from water drops freezing after accreting on ice crystals."

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[13] Ono worked with Mossop (e.g., Mossop and Ono 1969u), perhaps there was some "cross-pollination" of ideas…

Ono (1972u):

"However, from our present observations, it has been found that in the clouds where moderately large drops of 30 to 60 µm in diameter and graupel-like rimed ice particles occurred simultaneously, we have a high concentration of secondary ice crystals. The presence of drops with some hundreds of microns in diameter is not a crucial factor for crystal multiplication."

Moreover, Ono's (1972u) findings above would appear to square better with our own findings (e.g., HR90, RH91u) for maritime clouds in the Washington coastal waters concerning high ice particle concentrations since our cumuliform clouds in onshore flow always had plenty of supercooled droplets >30 µm diameter in their middle and upper portions, sizes that Ono implicated in ice multiplication. Also, our Washington maritime clouds have virtually no mm-sized drops as F2017 erroneously conclude are necessary for the "rapid" ice formation.

At the top of p 7.4: "…and observations are compromised by the potential of ice to break on contact with the aircraft or instruments (e.g., Field et al. 2006)."

A single reference to Field et al (2006) regarding probe-related ice artifacts could lead the reader to believe that shattering on probe tips was a very recently discovered problem. Shattering on probe tips has been a well-known problem and was obvious in the imagery as soon as 2D probes began to be used in the late 1970s. Those of us in airborne research have been addressing this problem for more than 30 years to minimize the contribution of artifacts to ice particle concentrations (e.g., Harris-Hobbs and Cooper 1987).

Many of reports of ice multiplication have originated at ground sites (e.g., Hobbs 1969u, Auer 1969u, Burrows and Robertson 1969u, Ono 1971u, 1972u, Vardiman 1978). Citing these reports and emphasizing that they were ground sites would have made it clear to the reader that airborne artifacts have not reduced this enigma very much.

In fact, in view of the complexity of aircraft measurements of ice particles, MORE ground observations are *critical*, particularly at sites where the H-M process should be frequently active in clouds at the ground as in the Cascade Mountains of Washington State (e.g., Paradise Ranger Station). Such ground measurements are vitally needed as well in the Middle East at sites where there has been a dearth of ice-in-cloud measurements[14]. Some authors now claiming that even modern outfitted research cannot derive accurate concentrations of ice particles (i.e., Freud et al. 2015). Hence, the need for more ground work if, in fact, the assertion in Freud et al. 2015 is true..

Section 2, last paragraph on p7.4: "Splinter production following the freezing of a large millimeter size droplet that subsequently shatters (droplet shattering; e.g., Mason and Maybank 1960.."

The authors in citing Mason and Maybank (1960) several times are apparently unaware that Mason and Maybank's results were compromised by CO_2, as discovered by Dye and Hobbs 1966u. CO_2 is a gas that promoted the shattering of drops that Mason and Maybank observed. Later, however, Hobbs and Alkesweeny 1968u, did find that a *few* splinters were shed by drops that rotated in free fall as they froze, far fewer than reported by Mason and Maybank. Hobbs and Alkesweeny's work should have been cited along with that of Brownscombe and Thorndike

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[14] Sites to consider might be at Mt. Hermon, Israel, or at ski resorts in Lebanon. In-cloud situations with snow and graupel precipitation would be common at these sites.

(1968).

P7.2, Section 2, laboratory evidence for secondary ice formation:

The role of water supersaturation in ice formation was ignored as a possible source of secondary ice. Gagin and Nozyce 1984u reported the appearance of ice crystals in the environment of freezing mm-sized drops in lab experiments. They attributed the formation of the new ice crystals to a pulse of high supersaturation with respect to water as the freezing drop warmed to 0°C in their chamber. This could be an important secondary ice-forming mechanism, similar in effect to that used by Chisnell and Latham (1976), who incorporated splinters derived from freezing drops. This process might explain the simultaneous appearance of ice splinters that appear so quickly, side-by-side, with frozen precipitation-sized drops.

P7.4, Section 3. In situ observations of SIP and the discussion of the role of IN.

The work of Rosinski (1991u) goes uncited. Rosinski did a lot of work on maritime IN, ones that he claimed were active at slightly supercooled temperatures in concentrations of tens per liter. His work should have been mentioned, even if it's only to state that his measurements are not generally accepted. However, if he was even partially correct, his findings would go a long way to explaining the rapidity of ice development in maritime clouds.

P7.5, "In addition, the measurements may be affected by the possibility that ice particles generated by the passage of the aircraft through the cloud (Woodley et al. 2003) from previous cloud passes could have mixed into the measured samples."

The authors only cite Woodley et al. (2003) regarding aircraft-produced ice due to the passage of an aircraft. This unexpected phenomenon was first reported 20 years prior to

Woodley et al. by Rangno and Hobbs (1983u, 1984u)[15]. Scientific etiquette requires that those who went first be cited. Not citing benchmark papers that roiled the airborne research community due to the temperatures at which ice was produced (>-10°C) is remarkable. John Hallett (2008) termed this finding, "an embarrassment to the airborne research community."

Too, not being cited when you should be inflicts material damage since one's impact in one's field, likelihood of promotions, awards, etc, is measured by citation metrics.

P7.6 "Lawson et al. (2015) suggest that the rapid glaciation in these strong updraft cores (~10ms^{-1}) occurs at temperatures too cold and too fast to be attributable to the H-M process."

Citing the report of Stith et al. (2004u) would have been *perfect* here, as would have been Paluch and Breed (1984u).

P7.7, discussion of Heymsfield and Willis (2014): "Heymsfield and Willis (2014) found that SIP evidenced by observations of needles–columns throughout the range -3°C to -14°C was observed predominantly where the vertical velocities were in the range from -1 to +1 ms^{-1}. The LWCs in the regions where SIP are observed are dominantly below 0.10 gm^{-3}. Median LWCs in these regions were only about 0.03 gm^{-3} with no obvious dependence on the temperature."

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[15] Our first two submitted manuscripts, ones that preceded RH83u, were rejected. The editor, B. Silverman wrote, concerning the 2nd manuscript, "The reviewers are still unconvinced by these controversial claims

The Heymsfield and Willis (2014) finding is not only counter to most of the Washington experience but also that of other workers (e.g., Mossop et al. 1968u, Figure 4[16]; Mossop et al. (1972u. Figure 2; Mossop 1985u, Figure 1), Paluch and Breed 1984u; Lawson et al 2015's "first ice"). Why? The initiation and observation of small ice particles in high concentrations usually occurs in the higher (short-lived) LWC zones (≥0.5 g m^{-3}). These contrary findings are not mentioned by F2017, ones that would have presented a different picture of the origin of the high concentrations of ice. Perhaps Heymsfield and Willis (2014) encountered their high ice particles in cloud "death throes"; evaporating anvil shelving, rather having encountered them close to where they formed?

P7.7, discussion of Taylor et al. (2016): "Taylor et al. (2016) analyzed aircraft measurements in maritime cumulus with colder (11°C) cloud-base temperatures that formed over the southwest peninsula of the United Kingdom. They found that almost all of the initial ice particles were frozen drizzle drops [;(0.5–1) mm], whereas vapor-grown ice crystals were dominant in the later stages. Their observations indicate that the freezing of drizzle–raindrops is an important process that dominates the formation of large ice in the intermediate stages of cloud development. In the more mature stage of cloud development the study found high concentrations of small ice within the H-M temperature range."

Virtually identical findings to Taylor et al.'s was reported for even cooler based clouds a quarter of a century earlier by RH91u which should have been cited along with Taylor et al.'s.

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[16] Mossop et al. 1968u also found columnar ice particles in dissipating, anvil-like regions as well as in high LWC zones.

P7.7, 2nd: "It has been speculated that graupel does not need to play the rimer role. In situ observations from frontal cloud systems suggest that riming snowflakes may be able to mediate the SIP (Crosier et al. 2011; Hogan et al. 2002.)

The 2002 and 2011 references to non-graupel ice particles shedding splinters seem out of place since this was considered so many years prior to these references. For example, riming by other than graupel particles was part of the "potential" H-M scheme of Harris-Hobbs and Cooper in 1987, in Mason 1998, and by Mossop 1985b.

P7.8. last three lines: "Finally, it should be noted that conditions where cloud tops are -12ºC and drizzle-sized supercooled droplets are present do not always result in the production of large numbers of ice crystals. Bernstein et al. (2007) and Rasmussen et al. (1995) identified these conditions as long-lived clouds and hazardous for aircraft."

Some elaboration on the interesting and important findings of Bernstein et al. (2007) and Rasmussen et al. (1995):

The University of Washington aircraft observed drizzle drops aloft in orographic clouds in the Oregon Cascade Mountains during IMPROVE 2 (Stoelinga et al. 2003); we had not observed them in the more aerosol-impacted clouds of the Washington Cascades in many years of sampling them, though we did not fly in the kind of strong synoptic situations encountered in IMPROVE 2.

However, those Oregon drizzle drops that we encountered in IMPROVE 2, as usually happens, didn't make it to the ground as liquid drops. IMPROVE 2 had ground measurements in support of airborne work; no freezing rain or drizzle events were reported, a finding compatible with long term records in the Sierras, and Cascades with precipitation at below freezing temperatures under westerly flow situations and when the temperature decreases with height (unpublished data). There is a duration-below-freezing-temperature factor, as well as the temperature itself, that together control the freezing of precipitation-sized drops. The deeper the sub-freezing layer at temperatures below about -4°C, the more likely drops will freeze on the way down becoming sleet/ice pellets.

Supercooled layered cloud tops, sometimes colder than -30°C, are common and persistent, and they have been known about since 1957 (Cunningham 1957u, Hall 1957u; this situation is shown in Byers 1965u), and were described later by HR85, HR98, and explained by Rauber and Tokay 1991u. Supercooled tops, usually ones having a broad droplet spectrum if they are shedding ice (RH85), persist because the ice that forms within them falls out, as do precipitation-sized drops, if any, and those drops freeze on the way down. Altocumulus clouds sporting virga is a common example of this phenomenon. In this "upside down" storm situation, ice particle concentrations have been observed to increase downward (e.g., HR85; Rasmussen et al. 1995) likely due to the breakup of fragile crystals. This phenomenon can mislead researchers solely using satellite data to infer the phase of entire cloud systems below those liquid tops.

p7.15, Section 6, discussion and conclusions section, second bulleted item: "The onset of the rapid glaciation of convective clouds is observed to occur shortly after millimeter-size drops freeze."

If Ono's 1972u findings are correct the glaciation process is also triggered by drops smaller than even drizzle drops whose sizes range officially from 0.2 to 0.5 mm diameter. In our cool-based, modest-sized Washington State maritime clouds (bases rarely >6°C) with mm-sized drops were rarely encountered; nevertheless, ice formation was usually rapid and prolific.

P7.15, Section 6, 2nd paragraph, last sentence: "It has been suggested by, for example, Koenig (1963) and Lawson et al. (2015) that supercooled raindrops play an important role in the initiation of the glaciation process and there is evidence that this can occur at temperatures greater than -10°C."

The phrasing that "there is evidence", which was likely unintentional, makes it sound like the appearance of ice in clouds with tops $\geq$ -10°C is a rare phenomenon which the authors know is hardly rare! It happens globally over the oceans in clean conditions, and in continental convective clouds with warm bases.

P7.6 "Figure 7-6 shows aircraft observations taken within a few hundred meters of cloud top by repeatedly penetrating a rapidly growing convective plume"

Can the authors rule out aircraft production of ice?

P7.7: "They found that almost all of the initial ice particles were frozen drizzle drops ~ (0.5–1) mm], whereas vapor-grown ice crystals were dominant in the later stages."

Drizzle drops are defined by the AMS and WMO as *close together* drops between 0.2 mm and 0.5 mm diameter. They virtually float in the air. The 0.5 to 1 mm diameter drops that F2017 refer to are raindrops, not drizzle ones.

P7.2, Section 2, Laboratory Studies:

Amid citations of laboratory experiments that "have produced secondary ice", we point out that Choularton et al (1980) only produced protuberances and spicules, not actual ice particles. Later, F2017 again cite Choularton et al. a bit incorrectly by suggesting the drop sizes for spicule production he studied was ">~25 µm". Choularton et al. reported the main increase in protuberances was for droplets >20 µm diameter.

P 7.4, Section 3, In Situ Cloud Studies, first paragraph, 2nd line: "Ice particles are often observed in abundance in convective clouds that are colder than 0°C but with cloud-top temperatures warmer than about -12°C…"

Slightly more accurately: "… clouds whose tops have ascended past -4°C but have not been colder than about -12°C..."

P7.5, Section 3, last paragraph: "Hobbs and Rangno (1985, 1990, 1998), in a series of aircraft investigations of maritime cumulus off the coast of Washington…"

F2017 indicates that HR98 concerned Washington State coastal clouds. It concerned Arctic stratiform clouds sampled mainly over the Beaufort Sea. This seems like a remarkable error for 29 authors to make. Moreover, in HR98 we discussed ice multiplication in pristine, slightly supercooled Arctic Stratus clouds with extremely low (<20 cm^{-3}) droplet concentrations. We found little correlation between droplets <13µm diameter droplets and small (≤300 diameter) ice particles as some have reported (Harris-Hobbs and Cooper 1987) in support of their importance in riming and splintering process. Yet ice was plentiful (10s per liter) regardless of the concentrations of those small droplets in boundary-layer Stratocumulus clouds with tops of just -4° to -6° C.

P7.5, Section 3, the discussion of Harris-Hobbs and Cooper 1987: "Harris-Hobbs and Cooper (1987) used airborne observations from cumulus clouds in three different geographic regions to estimate secondary ice production rates."

The California clouds that HHC87 examined were not Cumulus but were long stretches of orographic stratiform, banded cloud systems.

Editorial note concerning the popular phrasing, “warm or “cold” temperatures in numerous places.

A quote from Peter Hobbs on this common error; “A cup of coffee can be warm or cold, but not a temperature.” A temperature is a number and can have no physical state itself, but rather refers to the state of a tangible object.

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