Reply to "Comments on 'A Relationship between Reflectivity and Snow Rate for a High-Altitude S-Band Radar'"

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Before responding to Smith's comment (Smith 2013), we want to state a point of view that was not explicit in our paper (Wolfe and Snider 2012). Like several previous investigators (e.g., Puhakka 1975; Matrosov 1992; Rasmussen et al. 2003), we take the view that a constant α in the relationship between radar reflectivity Z_e and snow rate S ($Z_e = \alpha S^{\beta}$) is not sufficient for capturing the range of conditions within snow-producing systems. Thus, we are not in agreement with Smith's assertion that a valid Z_e–S relationship must have an α that is independent of moments of the particle size distribution. In what follows, we expand on our perspective and also reply to Smith's other criticisms of Wolfe and Snider (2012).

The form of the Z_e–S relationship questioned by Smith (2013)—that is, Eqs. (2) and (3) of Wolfe and Snider (2012)—is easily derived by following parameterizations and assumptions stated in our paper. We reintroduced Eq. (2), originally derived by Rasmussen et al. (2003), to acknowledge the prior work and to set the stage for Eq. (3). Smith asserts that empirically based relationships between S and the slope of the particle size distribution, and between S and the intercept of the size distribution, should be applied in an assessment of Eqs. (2) and (3). Starting with those relationships (Gunn and Marshall 1958; Sekhon and Srivastava 1970), Smith concludes that Eqs. (2) and (3) do not have an α that is independent of S and therefore do not exhibit "a clearly established value for the exponent β" and that "the mathematical analyses in both papers should be disregarded." We disagree with his assessment.

The basis for our rebuttal is twofold. First, we note that in our analysis (section 5c), we focused on ice particles we thought were unaggregated and unrimed whereas Sekhon and Srivastava (1970) concluded, for particles formed by aggregation, that a fit of particle concentration N and S "is hardly possible." So, even if Sekhon and Srivastava can be applied to our analysis, there is uncertainty in Smith's claim that "N itself . . . is a function of S" in our Eq. (3). Second, in our application of Eq. (3) [section 5c in Wolfe and Snider (2012)], we envisioned a cloud-top source of crystals maintaining a temperature-dependent snow rate at the surface. Admittedly, N and S are related in that scenario, but, when we applied Eq. (3) with historical ice nuclei spectra and compared the result with observationally based values of α, reasonable agreement between observation and the equation's prediction was obtained (our Fig. 7). Because of that consistency, and its implication for ongoing investigations (e.g., Fridlind et al. 2012), we disagree with Smith's assertion that the existence of an N–S relationship invalidates Eq. (3).

Smith also critiques our assumption that the size distribution (unmelted particles) is an exponential. Further, he provides an analysis demonstrating that a Weibull distribution with three parameters—in contrast to the two parameters evident in the distribution function we applied—is the correct form for unmelted particles. Because Smith's analysis introduces a parameter, the slope of the melted particle size distribution, that we cannot relate to the slope of the distribution for unmelted particles, the distribution he recommends cannot be applied to our analysis. Smith also states that our exponential

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distribution ''is based on a questionable assumption,'' implying that for unmelted particles an exponential is questioned, while for melted particles it is unquestioned. In fact, both are idealizations of actual distributions. Further, we note that the exponential distribution (unmelted particles) has been applied in useful analyses of snow particle imagery for at least 30 years (Lo and Passarelli 1982; Woods et al. 2008; Petty and Huang 2011).

Smith also critiques the form of the function we adopt for the air/ice density of the snow particles. He states that there is an inconsistency between our parameterizing function and the one adopted by Pokharel and Vali (2011) and expresses concern about the inconsistency. Two clarifications are needed. First, the Pokharel and Vali density function is for rimed particles, whereas we assumed—with corroboration from snow-depth and precipitation measurements—that the snow particles were unrimed. Second, as illustrated by Pokharel and Vali (their Fig. 4a), their density function does overlap, at smaller unmelted sizes, the form we assumed for our analysis [based on Brown and Francis (1995)]. We also note that the absence of overlap at larger size is consistent with the invariance of rimed particle density at diameters (unmelted) larger than about 1.5 mm (Locatelli and Hobbs 1974, their Fig. 7).

Smith concludes with formulas for reflectivity [his (C8)], snow rate [his (C9)], and reflectivity–snow rate [his (C10)]; however, he does not mention that the particle's air/ice density is assumed to be independent of size. The result is that, other things being equal (N and V_t), his (C10) predicts an α that is 3 times as large as that of Eq. (3). Hence, ensembles of particles whose air/ice density does not vary with size are expected to reflect substantially more than those with density varying inversely with size (assuming N, V_t, and S are the same in both scenarios). Validation of this prediction would require, among other things, concurrent determinations of air/ice density and fall speed.

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