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Interactive comment on "A Radar Reflectivity Operator with Ice-Phase Hydrometeors for Variational Data Assimilation (RadZIceVarv1.0) and Its Evaluation with Real Radar Data" *by* Shizhang Wang and Zhiquan Liu

Anonymous Referee #2

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This paper presents a forward operator, tangent linear model and its adjoint for reflectivity data assimilation (DA) in the variational framework. The procedures are very well documented in details, and the new systems are thoroughly evaluated. This manuscript could serve as a guideline for those who wish to develop tangent linear and adjoint models for new observation types. Although I believe this paper will make a valuable contribution to GMD, I have some concerns about the performance of the forward operator that need to be addressed. I have outlined my concerns below, and my overall recommendation is for acceptance pending major revisions.

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Discussion paper

1. Some sentences are a bit confusing; for example, "A fixed N0r value is only available for a single-moment microphysics scheme." This could be rephrased to something like, "N0r values are typically fixed (or constant) in single-moment microphysics schemes." Other sentences could be revised to improve readability. 2. Tables 1-2: The coefficients shown in Tables 1 and 2 were adopted from J08. In J08, they derived those coefficients for snow and hail. Therefore, the coefficients the authors adopted for graupel in this manuscript are indeed valid for hail and would result in reflectivity that is too high for graupel, which has a lower density than hail. Those coefficients should be replaced with coefficients for graupel. 3. Fig. 1b: It is hard to understand why the reflectivity for snow is much higher than the reflectivity for graupel. Because snow has a significantly lower density than graupel and the authors adopted hail coefficients for graupel, the reflectivity for graupel should be higher than the reflectivity for snow. Fig. 2 of J08 shows that backscattering amplitudes of hail as a function of the water fraction are larger than those of snow, which is opposite to Fig. 1b here. 4. Page 4, eq (5): Which value is used for the density of graupel? Is it 500 kg/m3 (typical value for graupel) or ∼ 913 kg/m3 (typical value for hail)? 5. Page 10, line 10: Why is the Thompson microphysics scheme used in this study? There are big differences between the snow and graupel size spectra assumptions used in the Thompson microphysics scheme and used in their DA system. This mismatch likely requires significant internal adjustments among state variables when the forecast model is launched after DA. For the purpose of evaluating the performance of their new system, it would make more sense to use a single-moment microphysics scheme that is consistent with their radar DA system. 6. Page 11, line 10: One dBZ observation error is too small, even if the performance does not change significantly with larger observation errors. 7. Page 11, line 19: The authors may use root-mean-square innovation (RMSI) instead of root-mean-square error (RMSE). 8. Page 12, lines 19-24: The operator implemented in CAPS-PRS is not the operator presented in J08 but the one developed by Jung et al. (2010, JAMC), which uses the numerical integration of the T-matrix scattering amplitudes over the particle size distribution (PSD). This one includes the Mie effect as well. By default, CAPS-

PRS would use the particle size distributions that are consistent with those used in the Thompson microphysics scheme. This means that the snow PSD is the combination of the gamma and exponential distributions, and the graupel PSD uses a diagnostic intercept parameter instead of a fixed value. Therefore, the almost exact fit between J08orig and RefZIceVar in Fig. 4 surprises me. I wonder if a mistake was made here. 9. Page 13, lines 27-29: However, 0 dBZ observations are available in clear air. Are they not used to suppress spurious echoes? 10. Fig. 14: Please add the line for thresholds for a skillful forecast and climatology.

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