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Effects of Mid- and Upper-Level Dry Layers on Microphysics of Simulated Supercell Storms

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ABSTRACT

Conceptual differences are presented among supercell storms simulated with midlevel and deep dry layers of varying magnitude. Initial patterns are identified which should be studied more comprehensively using observed or simulated data. These initial results indicate that mixing ratios of small ice particles are most sensitive to the depth of a dry layer rather than to its magnitude, with fewer particles in simulations containing a deep dry layer. Hail from frozen drops may be most abundant when a deep layer is dried, and bursts of hail species reaching low levels may be followed 15–20 min later by an increase in low-level vertical vorticity associated with the mesocyclone. Warm rain occurs repeatedly on the upshear side of the echo appendage, is especially variable in quantity, and is disfavored in simulations with a dry layer at midlevels. Increases in warm rain mixing ratio may be followed 10–20 min later by an increase in low-level vertical vorticity, though this association is sensitive to location of the warm rain and concurrent microphysical and dynamical processes. In simulations with substantial dry layers, vertical vorticity was concentrated more rapidly in association with the mesocyclone at low levels. Storms in simulations with deep dry layers produced larger areas of updraft >10 m s^{-1} at 1000 m AGL, and produced strong updraft more quickly than moister soundings. These results may be applicable when storms move into areas with different moisture characteristics from where they form, and should be supplemented by additional microphysical observations.

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1. Theoretical background and goals

Several observational studies have examined the effects of varying environmental moisture on deep convection. These studies typically focus on moisture at low levels, often in the lowest kilometer, and often do not consider varying midlevel and upper-level moisture. Long-lived supercells may move parallel to an axis of enhanced low-level moisture (Bunkers et al. 2006); a moister environment can prolong the life of a supercell even when wind shear is marginally favorable. Greater humidity in the lowest kilometer, manifested as a lower lifted condensation level (LCL) height, discriminates fairly well between nontornadic and tornadic supercells (e.g., Rasmussen and Blanchard 1998; Markowski et al. 2002; Thompson et al. 2003). Lower LCL heights may signal an environment favoring weaker hydrometeor evaporation and thus warmer downdrafts (e.g., as suggested in Markowski et al. 2002). Moister air below cloud base, in High Plains storms, has been shown observationally to result in less evaporative cooling, leading to less-intense downdrafts (Knupp 1988).

Favored storm evolution is sensitive to environmental moisture characteristics. In an early study of low-precipitation (LP) versus classic supercell storms (Bluestein and Parks 1983), depth of the low-level moist layer averaged the same for each. Moisture content within this moist layer, however, was quite different: in environments producing LP storms, mean water vapor mixing ratio averaged 1.6 g kg^{-1} lower, and precipitable water and mean humidity were less by a statistically significant

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amount (Bluestein and Parks 1983). In a large sample of supercells, tornadic storms were found to contain warmer rear-flank downdrafts (RFDs) on average, while non-tornadic RFDs tended to contain cooler, more stable air (Markowski et al. 2002). Less entrainment of dry midlevel air, resulting in less RFD cooling, may account for this difference in tornadic storms.

Several effects will not be considered here. Supercell storms may increase low-level moisture nearby (e.g., Johnson et al. 1987), lowering the LCL and thus possibly increasing tornado potential (e.g., Rasmussen and Blanchard 1998). A modified near-storm environment also may affect storm microphysics as the modified air is ingested. Also not considered here is the advection of hydrometeors between storms (e.g., Browning 1965; Knupp et al. 2003). This process undoubtedly affects supercell microphysics, especially when the upper-level storm-relative wind is strong. In this work, only the effects of pre-existing environmental moisture will be considered.

A few numerical studies have looked at effects of dry air on downdrafts. In an idealized study, entrainment of dry air *weakened* downdrafts via decreased precipitation loading (Srivastava 1985). In a follow-up study, Srivastava (1987) found that inclusion of the ice phase led to lower average mixing ratio and relative humidity at low levels around a simulated downdraft.

In a modeling study that examined the effects of environmental moisture in the lowest 2.8 km, increasing low-level moisture allowed updrafts to persist under increasing vertical wind shear (Schlesinger 1973). This result agrees with observations of storms that often struggle on dry days, and may be related to the increased CAPE of environments with greater low-level moisture. Tropical convective systems produce less precipitation when the mid and upper levels are dry (Ridout 2002), though the applicability of these results to midlatitude supercell convection is uncertain.

A modeling study with liquid-only microphysics explored the effects of mid-tropospheric dry air on supercell evolution (Gilmore and Wicker 1998). Storms in environments with drier midlevel air generally contained stronger outflow. In moderate-shear environments, this strong outflow tended to weaken the updraft by lessening inflow, though if strong shear was present, surging outflow was less likely to weaken the updraft. This result agrees with other studies which have shown mesocyclones are less likely to occlude with stronger wind shear (e.g., Adlerman and Droegemeier 2005), and with the observation that microphysical effects become less important in supercell temporal evolution as the shear becomes stronger and dynamical influences predominate (Gilmore et al. 2004a). A recent study with mixed-phase microphysics concluded that downdraft mass flux and cold pool intensity may be *lower* in supercells with dry air aloft (James and Markowski 2010). In this study, relative humidity was reduced to 50% or 70% in a roughly 1.5-km-deep layer centered on 3.39 km AGL.

In high-CAPE situations, simulated storms were less sensitive to the environmental moisture profile, while low-CAPE storms needed more moisture to form substantial updrafts. Low-CAPE storms also contained cold pools of similar or reduced size and strength. This was attributed to a smaller precipitation loading contribution offset by greater evaporative cooling, and to less melting of hail. Reduced midlevel updraft size and strength in simulations with dry air was attributed to the detrimental effects of dry air entrainment. The authors also note that simulations with mixed-phase microphysics may describe more correctly the effects of midlevel dry air on low-level supercell outflow (James and Markowski 2010).

Prior supercell modeling work has not examined how distributions of specific rain, hail, and small ice species may vary as the environmental moisture profile changes. Since spatial and temporal variability of dry layers aloft is particularly high in the Great Plains where supercells are prevalent, more consideration should be given to how this environmental variability may influence storm evolution. Here we present effects of midlevel and deep dry layers on simulated supercell storms, specifically examining distributions of several hydrometeor species, low-level vertical vorticity and updraft magnitude. Given relationships between particle distributions and the vertical moisture profile, we present potential operational implications for storms moving from moister to drier environments. Our focus will be on *qualitative and conceptual differences* among storms, given the large sensitivity of storm-scale simulations to model setup and environment. Particle trajectory analyses would be a helpful next step, but are beyond the scope of this work since our focus is to identify initial patterns which can be studied more comprehensively using observed and simulated data.

2. Methodology

A control sounding was chosen which produced a long-lived, isolated, classic supercell. The hodograph was characterized by a half-circle turn over the lowest 10 km (radius 25 m s^{-1}), and constant wind in the 10–20 km layer (Fig. 1).

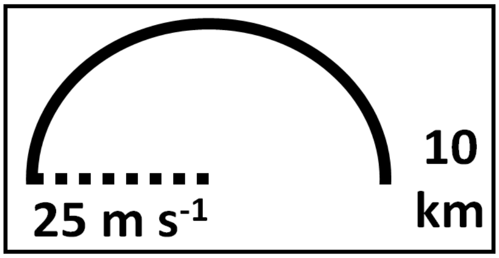

Figure 1: Hodograph used in all simulations.

Midlevels were defined as the 3.14–6.28 km layer, while upper levels were defined as 6.28–12 km. Five simulations were run (identifier in brackets following description identifies each simulation through the remainder of the paper):

1) A *control* moist simulation (simulation C);
2) A simulation with a *moderate midlevel dry layer*, in which a maximum of 1 g kg^{-1} was subtracted from the 3.14–6.28 km layer (simulation MM);
3) A simulation with a *substantial midlevel dry layer*, in which a maximum of 2 g kg^{-1} was subtracted from the 3.14–6.28 km layer (simulation SM);
4) A simulation with a *deep moderately dry layer*, in which a maximum of 0.175 g kg^{-1} was subtracted from the 3.14–12 km layer (simulation MD); and
5) A simulation with a *deep substantially dry layer*, in which a maximum of 0.367 g kg^{-1} was subtracted from the 3.14–12 km layer (simulation SD).

Maximum drying relative to the control simulation was applied at the grid point nearest the center of the layer, with relative magnitude of drying tapering to zero at the edges of the layer using a sine curve. No other modifications were made to the sounding. The melting level was at 643 hPa (3728 m), with a 120-hPa (1428-m)-deep subsaturated layer below the melting level. Maximum dewpoint depression in this layer was 3.4°C at the melting level. Resulting soundings contained a moist absolutely unstable layer (MAUL; Bryan and Fritsch 2000) above a 0.7-km subsaturated layer (maximum dewpoint depression 2.9°C). The soundings (Fig. 2) are reasonable for convective inflow with substantial mesoscale ascent. A different environment in other regions may produce very different results.

The simulations used the Straka Atmospheric Model, a three-moment, nonhydrostatic model with open side boundaries and free-slip upper and lower boundaries. Model dynamics and microphysics are detailed in several prior works (Straka and Mansell 2005; Straka 2009; Straka and Gilmore 2010). The model has been used in simulations of thunderstorm electrification (Straka and Mansell 2005), deep convective storms including supercells (Gilmore et al. 2004a, 2004b), and in simulations of a right-moving supercell storm (Ziegler et al. 2010). The model was run with a time step of 1 s and horizontal grid spacing of 250 m on a 100-km × 100-km domain. Vertical resolution ranged from 155 m near the surface to 520 m near the top of the domain (20 km), with a lowest vertical level 75 m AGL. Convective initiation was accomplished via a warm spheroidal bubble at the center of the domain with temperature perturbation of 3 K. The horizontal radius of the warm bubble was 10 km, with a warm-bubble depth of 2.8 km.

The microphysical parameterization was ice-inclusive with fifteen hydrometeor species. Hydrometeor distributions were specified by gamma distributions (Straka 2009), which were solved using a three-moment scheme including reflectivity factor, liquid water content, and number concentration. Species included cloud droplets (4–82 μm) and drizzle (82–500 μm), which can grow further by collection to become warm rain (500+ μm). Rain also can be formed by shedding from and melting of ice particles (rain from shedding; rain from melting). Small ice particles include frozen cloud droplets, frozen raindrops (initial size $\geq$500 μm), and graupel, which grows by riming of smaller ice particles to diameter $\geq$500 μm. If graupel continues to grow via riming to diameter $\geq$5000 μm, it is classified as hail from graupel. Hail can also form around a frozen raindrop, and is classified as hail from frozen drops. Several species of variable-density ice crystals include plates, columns, dendrites, bullet rosettes, and crystal aggregates.

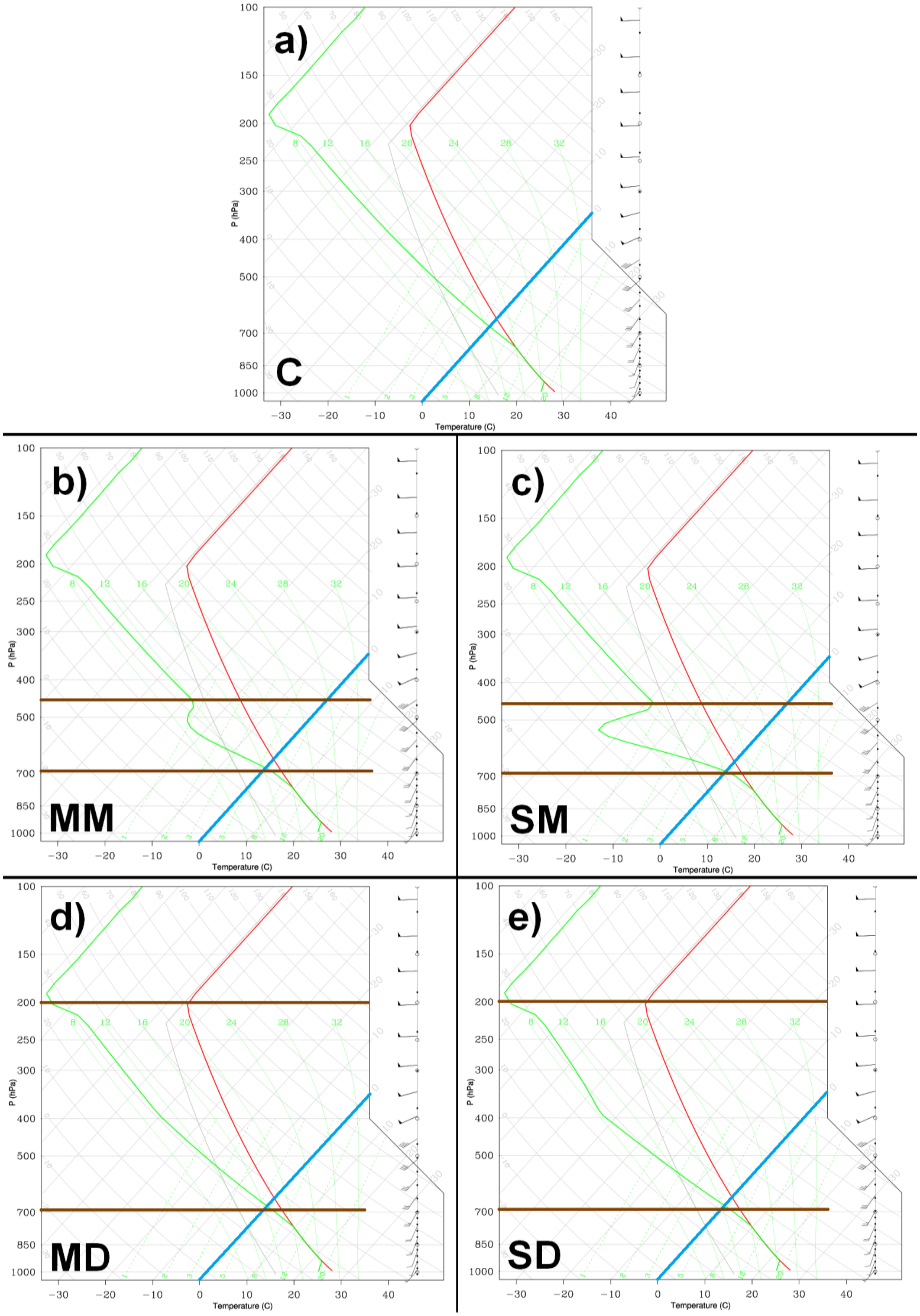

Figure 2: Skew T–log *p* diagrams representing the five simulations discussed in this paper: a) control, b) moderate midlevel dry layer, c) significant midlevel dry layer, d) moderate deep dry layer, and e) significant deep dry layer. Blue line highlights the 0°C isotherm, which intersects the sounding at 643 hPa (3728 m AGL). Brown lines indicate the dried layer in each simulation. *Click image for enlargements*.

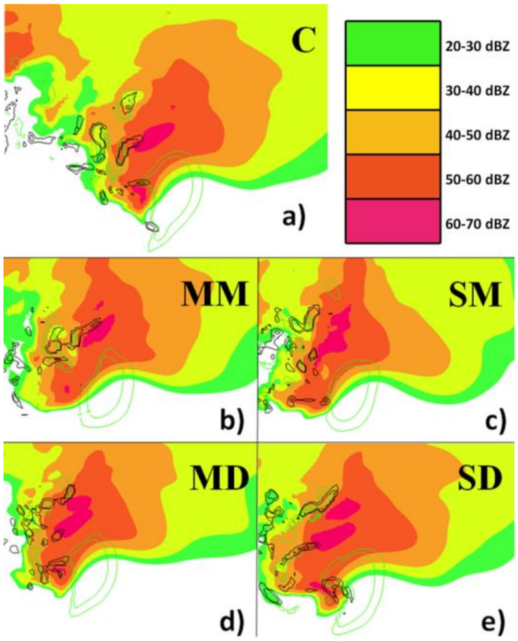

Figure 3: Configuration of 1000-m updraft region (green contours at 3, 5, and 10 m s^{-1} when applicable) and areas of enhanced vertical vorticity values (black contours at 0.005 and 0.01 s^{-1} when applicable). Simulated reflectivity (color shading) starts at 20 dB*Z*, with contour interval 10 dBZ. Time is 4200 s past model initialization for a) the control simulation, b) simulation MM, c) simulation SM, d) simulation MD, and e) simulation SD.

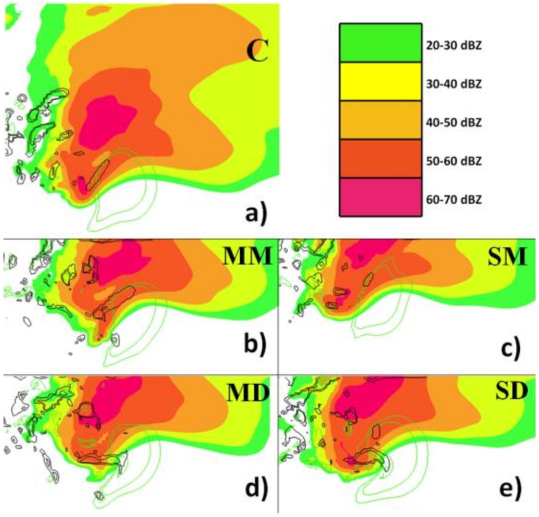

Figure 4: As in Fig. 3, except time is 6000 s past model initialization.

At each 5-min model output step, maximum 1000 m AGL mixing ratio was recorded for graupel, frozen drops, hail from graupel, hail from frozen drops, rain from shedding, rain from melting, and warm rain, along with maximum vertical vorticity at the lowest model level (75 m) associated with the mesocyclone (hereafter 'vertical vorticity') and extent of the 10+ m s^{-1} updraft region at 1000 m AGL. The 1000-m level was analyzed since most RFDs are influenced there (e.g., Markowski et al. 2002), though air in the tornado vicinity may originate at lower heights, especially in strongly tornadic supercells. Maximum mixing ratio of a species was highly correlated to storm total mixing ratio of that species.

3. Storm-scale differences between simulations

The control (relatively moist) and dry-layer simulations showed considerable storm-scale variability, particularly near the mesocyclone. Storms all developed supercell characteristics by 3000 s (50 min) past model initialization. The storm in the control simulation, and especially storms in deep dry layer simulations (MD and SD), produced earlier surges of westerly momentum on the west side of the updraft region. This was associated with increased low-level vertical vorticity there (Fig. 3; 4200 s). At this early time, storms in simulations with a deep dry layer displayed the most differences in the mesocyclone region compared to other storms.

More intense RFD westerly momentum surges were noted persistently in storms with a deep dry layer. Consequently, they had stronger updrafts compared to storms in other simulations, with more concentrated areas of enhanced vertical vorticity to the north of the RFD surge (Fig. 4; 6000 s). The mesocyclone of the control simulation was generally similar to mesocyclones in simulations with only a midlevel dry layer. This is an important finding since storms with stronger updrafts, such as those in the deep-layer drying simulations, are expected to produce larger total amounts of precipitation (e.g., Gilmore et al. 2004a). Finding stronger updrafts, then, may lead to anticipation of higher mixing ratio of some hydrometeor species reaching low levels in these storms, as examined in this study.

Simulated radar reflectivity showed longer-lived storm-scale organization in simulations with a deep dry layer—in midlevel-dry-layer-only simulations, the mesocyclone became ill-defined after approximately 7500 s (2 h 5 min). The control simulation maintained well-defined supercell structure for the longest time, and was still intense at 9000 s when the simulation ended (Fig. 5).

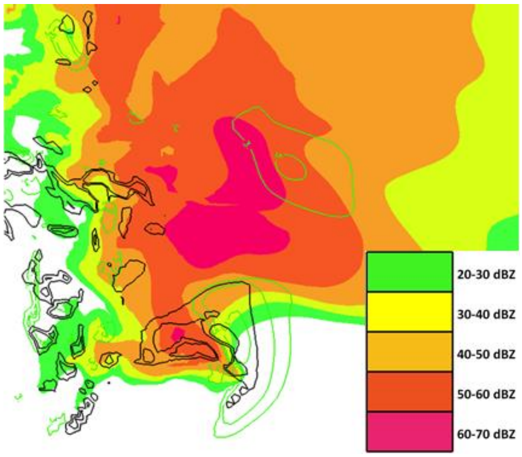

Figure 5: As in Figs. 3 and 4 for the control simulation at 9000 s past model initialization.

4. Spatial and temporal hydrometeor distributions

a. Graupel and frozen drop distributions

Spatially, small ice particles were similarly located regardless of moisture profile. Frozen drops typically were found just downwind from the updraft region within the precipitation core (Fig. 6). This was consistent with liquid drops forming in the updraft, freezing, being advected by the storm-relative wind, and falling out slightly downstream. Frozen drops melted before reaching the lowest model level, and wrapped around the west side of the updraft region at 3 km AGL. Graupel was typically located slightly downstream (northeast) from the frozen drop maximum (Fig. 6), for two primary reasons: 1) since graupel forms when an ice crystal accretes supercooled drops, the wind would advect growing particles farther from the updraft before they became classified as graupel, and 2) ice crystals are low-mass particles relative to raindrops, advecting farther at a given wind speed.

Average maximum frozen-drop mixing ratio varied from 0.08 g kg^{-1} in SD to 0.17 g kg^{-1} in SM (Table 1). *Frozen-drop mixing ratio was not very sensitive to the magnitude of drying, but rather to the depth of the dry layer.* Larger hydrometeor mixing ratios were expected in storms with a deep dry layer since they had stronger updrafts, but frozen drop mixing ratios were lower in these storms. This suggests the lower frozen drop content may be attributed to sublimation of small ice particles as they leave the saturated updraft region. Rain from melting and shedding had relatively high mixing ratios with these moisture profiles (Table 1). Thus these drops either were not freezing, or had sublimated before reaching the 1000-m level. Less evaporation and sublimation would be expected given a shallower dry layer, so frozen drop mixing ratios should be higher, as observed (Table 1).

Graupel mixing ratio was more variable. Lowest average graupel mixing ratio was again seen when the deep layer was substantially dried, indicating the importance of evaporation and/or sublimation. Sublimation may contribute as for frozen drops, and evaporation of the supercooled drops required for graupel formation also may be important. In particular, dry-air entrainment may increase evaporation of the supercooled droplets responsible for graupel growth. Simulations with only a midlevel dry layer averaged approximately 60% more graupel content (Table 1). Thus, as for frozen drops, *depth of a dry layer was more important than magnitude of drying*, likely because of the influence on supercooled droplet evaporation. Simulations with only a midlevel dry layer may contain higher average graupel content (Table 1) since these storms contain a higher frozen drop mixing ratio as shown above; frozen drops may serve as nuclei on which riming leads to graupel formation.

Table 1: Average maximum mixing ratio (g kg^{-1}) of frozen drops (FD), graupel, hail from frozen drops (HFD), hail from graupel (HG), rain from melting (RM), rain from shedding (RS), and warm rain (WR) from 3000 s–7200 s past model initialization for all simulations. Elevation is 1000 m AGL. Cells indicating values with ≥15% lower (higher) mixing ratio than the control simulation are brown-shaded (green-shaded).

Simulation	FD	Graupel	HFD	HG	RM	RS	WR
1 C	0.11	0.23	1.35	1.85	2.60	5.00	0.32
2 MM	0.13	0.36	0.97	2.18	2.95	4.56	0.28
3 SM	0.17	0.40	1.01	1.73	2.94	4.18	0.24
4 MD	0.09	0.26	1.08	2.24	3.14	4.55	0.33
5 SD	0.08	0.22	1.13	2.13	3.49	4.74	0.31

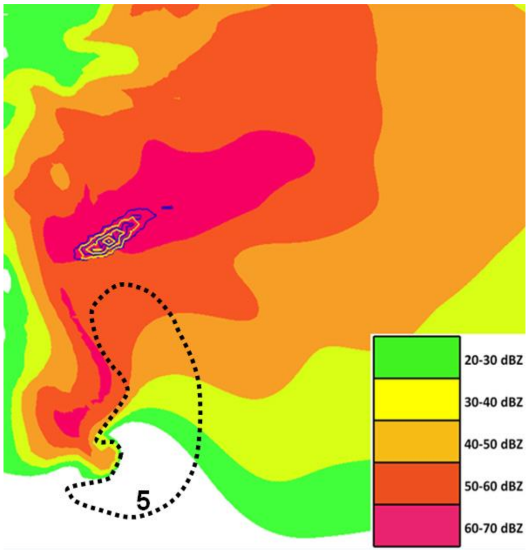

Figure 6: Typical distribution of graupel (blue contours) and frozen drops (yellow contours) at 1000 m AGL in the control simulation (7800 s past model initialization). Contour interval for graupel and frozen drops is 0.1 g kg^{-1}. Simulated radar reflectivity is color-contoured starting at 20 dBZ with contour interval 10 dBZ. 5 m s^{-1} updraft contour indicated by dashed line.

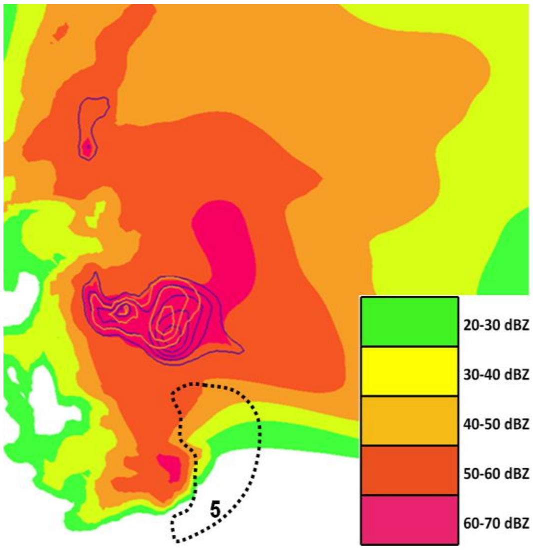

Figure 7: As in Fig. 6, except orange contours represent 1000-m HFD and blue contours represent 1000-m HG at 8700 s past model initialization. Both have contour interval of 0.5 g kg^{-1}.

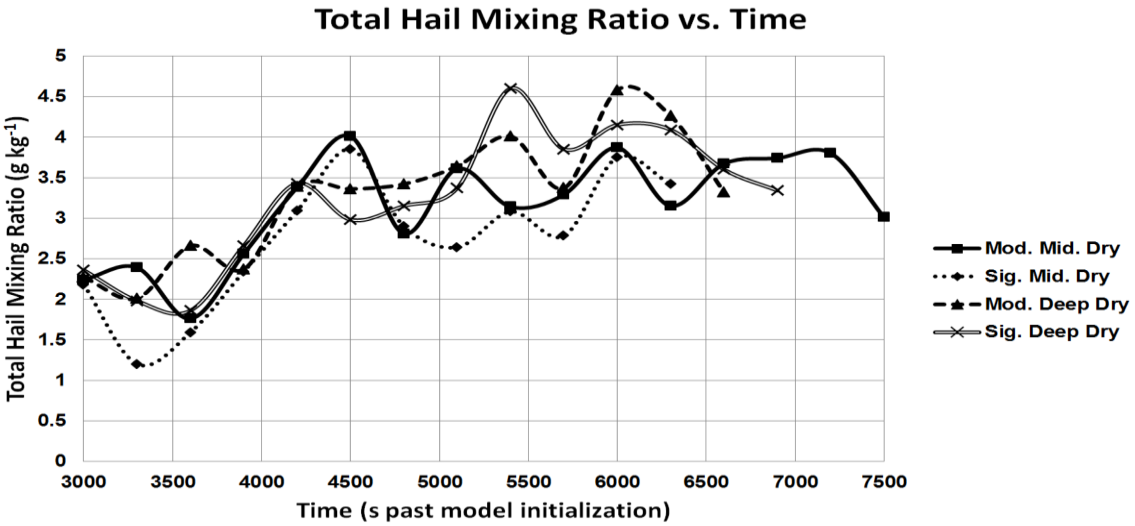

Figure 8: Total maximum hail mixing ratio (maximum hail from graupel + maximum hail from frozen drops; g kg^{-1}) at 1000 m AGL vs. time past model initialization(s). Solid line with squares denotes simulation MM, dotted line with diamonds denotes simulation SM, dashed line with triangles denotes simulation MD, and double line with “×” marks denotes simulation SD.

b. Hail distributions

Hail from graupel (HG) and hail from frozen drops (HFD) occurred north of the updraft region in all simulated storms (Fig. 7). Maximum HFD mixing ratio at 1000 m AGL was typically located near the interface of the updraft and precipitation core. HG often had a slight downstream bias compared to HFD (Fig. 7), reflecting the average downstream location of graupel compared to frozen drops. Total maximum hail mixing ratio (maximum HFD +

maximum HG) at 1000 m AGL was similar between simulations (Fig. 8), with a marked increase in total hail content centered on 4000 s (1 h 7 min) after model initialization. After this sharp increase in total maximum hail mixing ratio, simulations with a deep dry layer typically had higher maximum hail mixing ratio. This is consistent with more hail mass surviving as hailstones fall through the subsaturated layer below the melting level, and possibly with dry-air entrainment leading to drier hail-bearing downdrafts in simulations with deep-layer drying.

Drier downdrafts can lead to greater surviving hailstone mass via increased evaporative cooling (e.g., Rasmussen and Heymsfield 1987). More hail may also be present in these simulations since larger overall hydrometeor production is expected in storms with stronger updrafts (e.g., Gilmore et al. 2004a). Total maximum hail mixing ratio averaged approximately 11% (0.32 g kg^{-1}) higher in simulations with a deep dry layer. Content of HFD was higher with greater drying, and the average mixing ratio of HG was approximately twice that of HFD (Tab. 1). These five simulations produced cyclic bursts of higher total hail mixing ratio, consistent with supercell observations which often indicate hailfall in distinct bursts (e.g., Kumjian and Ryzhkov 2008; Van Den Broeke et al. 2008).

A comparison of 3000-m AGL HFD distributions at 6000 s is presented for storms simulated with the control (moist) profile and the four dried profiles (Fig. 9). Given stronger mesocyclones in storms with deep-layer drying, higher HFD mixing ratios wrapped around the northwest side of the updraft in those storms (Figs. 3b–c vs. 3d–e). Thus, the axis of highest HFD values was generally west-east oriented in storms with only a midlevel dry layer, and generally southwest-northeast in storms with a deep dry layer and in the control simulation.

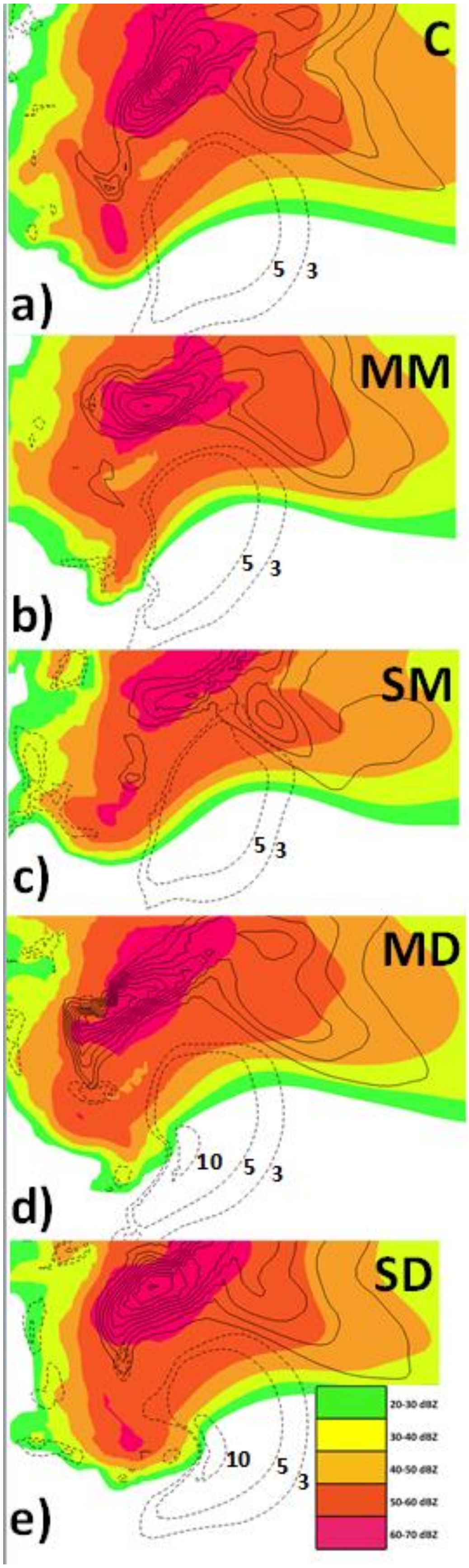

Figure 9: Comparison of HFD spatial distribution and mixing ratio in simulations with varying moisture profiles: a) C (moist), b) MM, c) SM, d) MD, and e) SD. Time is 6000 s past model initialization. Reflectivity factor at 1000 m AGL is shaded with a minimum contour of 20 dB*Z* and a contour interval of 10 dB*Z* (values in color bar; panel e). The updraft region at 1000 m AGL is represented by dashed contours showing vertical velocities of 3, 5 and 10 m s^{-1}. HFD mixing ratio at 3000 m AGL is represented by solid contours with contour interval 0.5 g kg^{-1}.

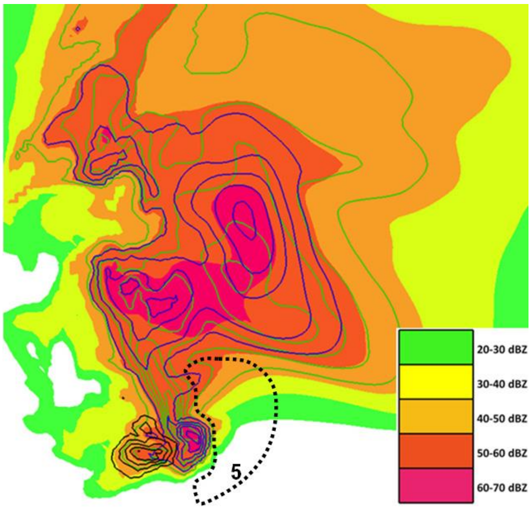

Figure 10: As in Figs. 6 and 7, except blue contours represent RM (interval 0.5 g kg^{-1}), green contours represent RS (interval 0.1 g kg^{-1}), and black contours represent WR (interval 0.05 g kg^{-1}) at 8700 s past model initialization.

c. Rain distributions

Rain from melting (RM) and shedding (RS) occurred throughout the precipitation core of each simulated supercell. RM often had two maxima (Fig. 10): 1) just north of the echo appendage region, where the storm-maximum RM mixing ratio was often located, and 2) well north of the mesocyclone, near the center of the region enclosed by the simulated 60-dB*Z* reflectivity contour. The first maximum may represent the melting of hail near the updraft, while the second represents a preferred fallout region for graupel particles given this hodograph. Two maxima in the RS distribution, located in the same locations as for RM, likely represent shedding from hailstones and droplets shed from melting graupel, respectively.

In most simulations, RM content was roughly cyclic (not shown), likely related to the roughly cyclic production of ice species, especially hail, in these storms. *RM content was greater when a deep layer was dried*, and less sensitive to magnitude of drying. A drier environment favored stronger updrafts, which have been associated with larger total precipitation

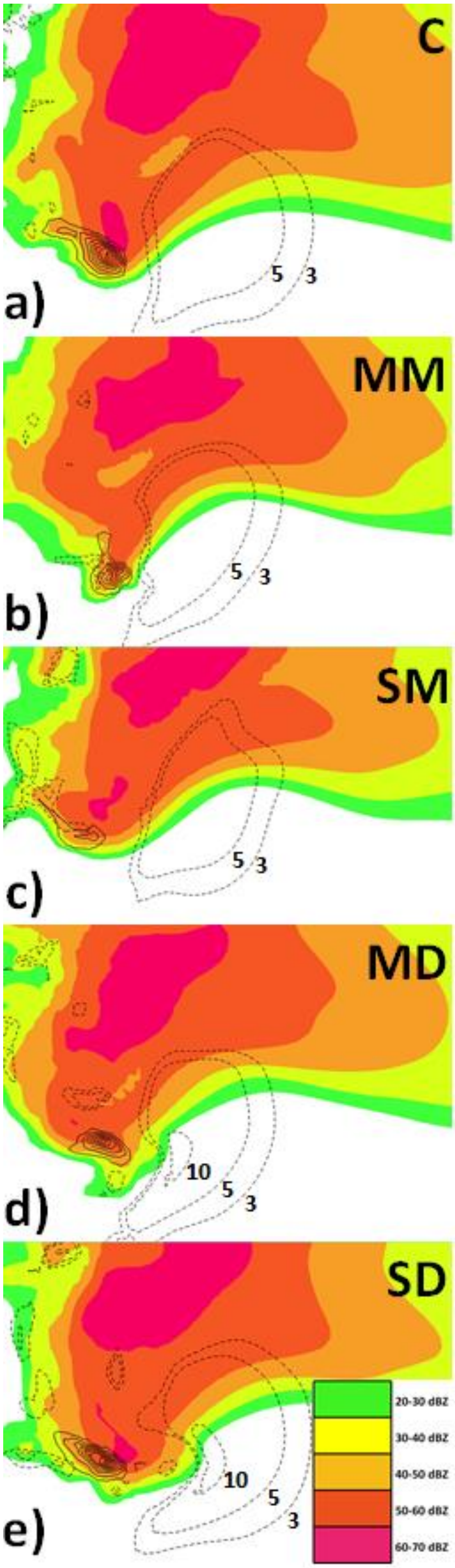

Figure 11: As in Fig. 9, except solid contours here represent WR mixing ratio at 1000 m AGL. Contours of WR have an interval of 0.05 g kg^{-1}.

production at low levels. This may include more small ice particles, which would melt more easily. Maximum RM mixing ratio varied by ≈34% between simulations (Table 1). Average values of maximum RS mixing ratio varied by ≈20% (Table 1). Less RS occurred with a substantial midlevel dry layer. Once liquid is shed, it may evaporate more readily in a dried environment, though in our simulations, most drying was applied to the control profile above the melting level (Fig. 2).

Warm rain (WR) occurred in a specific and repeatable location across all simulations, on the west and southwest side of the echo appendage and just west of where the RFD may originate (Fig. 11). Observational evidence for a similar spatial distribution has been presented for Southern Plains supercells (e.g., Kumjian 2011). Regions of higher reflectivity extending westward from the appendage were often strongly dominated by WR. WR may occur on the west side of the appendage because seeding by ice particles is relatively disfavored on the updraft's upshear side. This distribution should be less clearly seen, and the amount of WR less, when upshear storms spread ice crystals over a storm of operational interest.

Magnitude of maximum WR mixing ratio varied substantially between storms. The control simulation had a large quantity of warm rain (maximum mixing ratio around 0.35 g kg^{-1}; Fig. 11a), while simulations with a midlevel dry layer had much lower content and maximum values (0.2 g kg^{-1} in simulation MM, Fig. 11b, and 0.05 g kg^{-1} in simulation SM, Fig. 11c). Storms with a deep dry layer contained 0.3–0.35 g kg^{-1} maximum WR mixing ratio at this time (Figs. 11d–e), coincident with strong RFD westerly surges. Larger mixing ratio of warm rain, which is dominated by small drops, should indicate more evaporative cooling and thus greater potential for westerly surges if the warm rain can influence the RFD formation region.

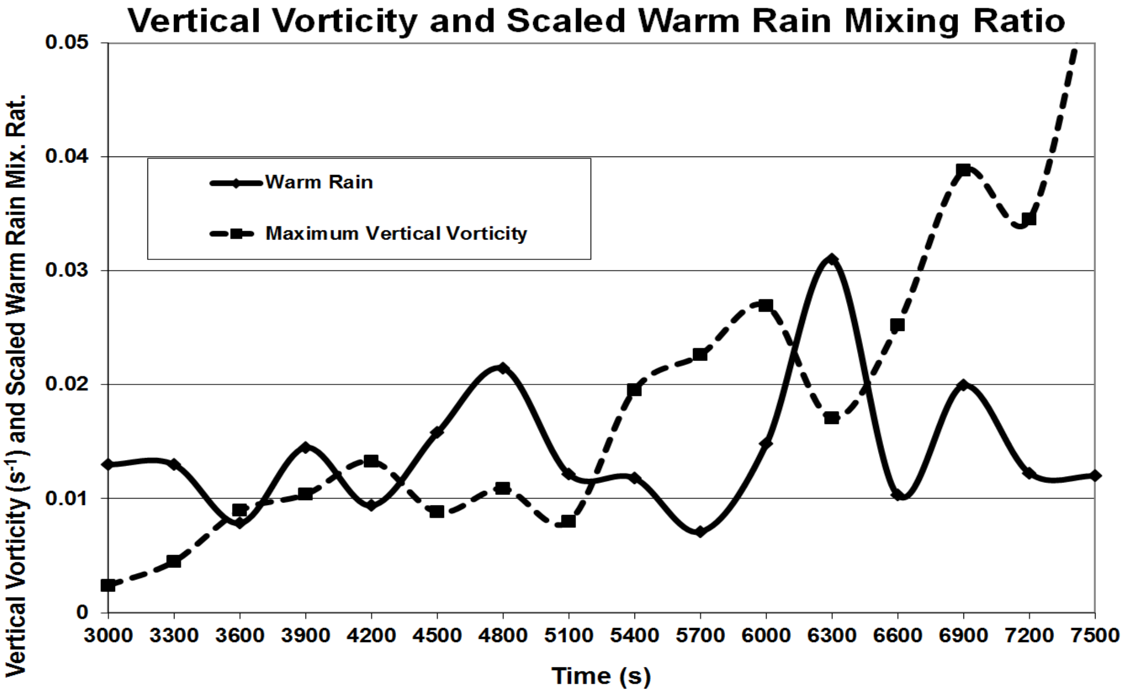

Figure 12: Time series of maximum vertical vorticity (s^{-1}) at 75 m AGL (dashed line) and warm-rain mixing ratio at 1000 m AGL (solid line) for simulation MM. Warm-rain mixing ratio is scaled by a factor of 50 for plotting. Time runs from 3000–7500 s past model initialization.

Maximum WR mixing ratio values were highly cyclic; a typical example of this behavior is shown (Fig. 12). Each simulation typically contained four to five maxima in the WR time series punctuated by deep minima when WR content dropped by 50%–75%. These fluctuations were larger than those of RM or RS, suggesting a more cyclic process by which WR is produced or favored in these storms. *Midlevel dry layers of any magnitude were unfavorable* *for WR relative to the control simulation* (Table 1). In addition, simulations with a substantial dry layer produced storms with smaller maximum WR mixing ratios than simulations in which the same layer was only moderately dried, but this effect was smaller. These observations may be attributable to evaporational depletion of small liquid droplets when descending in an RFD which has been dried by entrainment of environmental air—given the small droplets dominant in a WR distribution, any dry air entrainment should act to increase evaporation of the drizzle droplets involved in WR formation. Also, WR may be supercooled and may exist well above the 0°C isotherm, depending on the ambient aerosol distribution (e.g., Huffman and Norman 1988; Rosenfeld et al. 2013). Thus, a much deeper layer of subsaturated air may be available to increase WR evaporation than indicated by the relatively shallow dry layer between the 0°C isotherm and the top of the MAUL.

Simulations with midlevel dry layers had maximum drying relative to the control applied at ~4.7 km, vs. ~7.6 km in deep dry layer simulations. Thus, midlevel dry layer simulations would be expected to produce lower WR mixing ratio values than simulations with a deep dry layer, as defined in this study.

5. Environmental dry layers related to RFD and updraft evolution

When a deep layer was dried, simulated storms showed a more pronounced RFD westerly surge with a large zone of enhanced vertical vorticity to its north (Fig. 13). These storms also more often contained a strong updraft pulse (magnitude of vertical motion >10 m s^{-1}). From 3000 s–7500 s past model initialization, when all simulations contained a well-structured supercell, only 28% of time steps in the midlevel dry layer simulations contained an area of updraft >10 m s^{-1} at 1000 m, while in the two deep dry layer simulations, 48% of time steps had an updraft at least this strong. The 1000-m updraft was strongest and most extensive in simulation SD.

Simulations with a substantial dry layer might be expected to produce more rapid mesocyclone evolution (e.g., Gilmore and Wicker 1998; Adlerman et al. 1999), given the arrival of stronger downdrafts at the surface due to more hydrometeor evaporation and associated cooling.

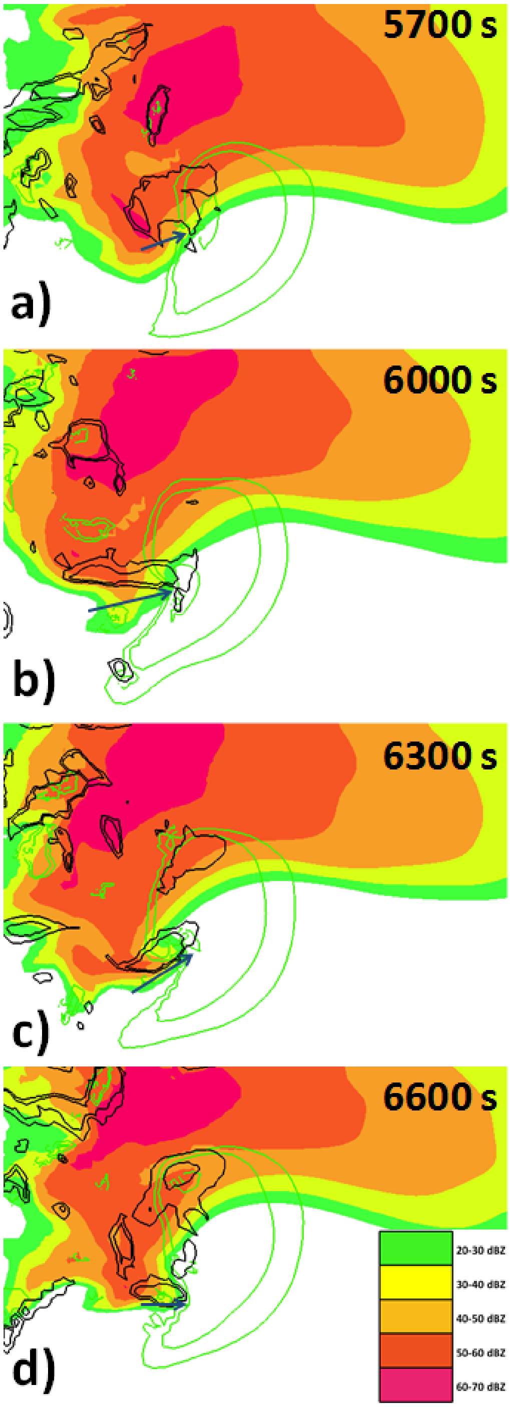

Figure 13: Typical configuration of 1000-m updraft region (green contours at 3, 5, and 10 m s^{-1}), RFD westerly momentum surge (indicated by arrow), and areas of enhanced vertical vorticity values (black contours at 0.005 and 0.01 s^{-1}). Simulated reflectivity (color-filled) starts at 20 dBZ, with interval 10 dBZ. Example is taken from simulation MD from 5700–6600 s past model initialization.

One way to assess rapidity of mesocyclone evolution is to compare the time of first occurrence of half-maximum and quarter-maximum vertical vorticity values at the lowest model level. These measures were derived by observing the maximum lowest-level vertical vorticity value from 3000–7500 s past model initialization, and noting the first occurrence of values 50% and 25% this magnitude. Quarter-maximum vorticity was reached an average of 4800 s past model initialization in the four dried simulations, and at 6600 s in the control simulation, a difference of 30 min. Half-maximum vorticity was reached on average 600 s (10 min) earlier in the dried simulations compared to the control simulation, but results were less consistent between simulations.

Though a larger number of simulations would add credibility to this result, it indicates that storms may be able to organize the low-level vorticity field more rapidly in environments with midlevel and deep dry layers, all else equal, consistent with prior numerical studies. On days with drier environments and stronger downdrafts, a low-level vertical vorticity maximum may be expected to develop more rapidly once a storm has developed supercell characteristics.

6. Microphysical influence on near-surface vertical vorticity

Mixing ratios of several hydrometeor species appeared to be related to changes in the low-level vertical vorticity field. It is useful to investigate such associations, since polarimetric weather radar can be used to distinguish certain broad classes of hydrometeor species in a nowcasting environment (e.g., hail, small ice particles, warm rain; Straka et al. 2000). Changes in the quantity of some species may be a useful indicator of subsequent changes in the low-level vorticity field. Melting, sublimation, and evaporation associated with bursts of particular hydrometeor species may be a mechanism for the generation of internal RFD momentum surges, which have been associated with tornado intensification and genesis (e.g., Lee et al. 2012; Karstens et al. 2013). Also, a burst of cooling due to hydrometeor fallout does not necessarily cause an RFD to become cold when occurring within an otherwise-warm RFD. Thus, the results shown here are compatible with the assessment of Markowski et al. (2002) that tornadoes are not typically associated with cold RFDs.

For each microphysical variable examined in our study, lag correlations were calculated between low-level (75-m) vertical vorticity and mixing ratio of the hydrometeor species at prior times (5, 10, 15, and 20 min prior). Graupel, rain from melting, and rain from shedding showed very low lag correlation values with vorticity, and it appeared there was little relationship between these species and the mesocyclone-associated low-level vorticity field. Other microphysical variables showed larger associations.

Both hail variables (HFD and HG) showed moderate positive lag correlation values with the 75-m vorticity field. HFD maxima were followed 15–20 min later by an increase in 75-m vorticity in simulations with a deep dry layer (average lag correlation values of 0.50 at 15 min and 0.62 at 20 min), though this pattern was not consistent in simulations with only a midlevel dry layer. HG showed a more consistent pattern, as might be expected given maximum mixing ratio values which averaged nearly twice that of HFD (Table 1). In all simulations, large HG mixing ratio values were followed 15–20 min later by an increase in the 75-m vorticity field (Fig. 14 shows a representative time series from simulation MM). Lag correlation values averaged across all four dried simulations were 0.46 at 15-min lead time and 0.47 at 20-min lead time. This relationship was strongest in simulations with a midlevel dry layer, which had lag correlation values greater than 0.50 for some lag times. Melting associated with hailfall may produce a westerly RFD momentum surge, as was often seen in our simulations leading to a surface vorticity increase on its north side. This behavior closely resembles a pattern reported from observations of supercells (e.g., Browning 1965, Van Den Broeke et al. 2008).

Frozen drops showed a *negative* lag correlation with the 75-m vorticity field at 10–15 min lag times in all simulations. Thus, maxima in the frozen drop mixing ratio were associated with decreases in 75-m vorticity 10–15 min later. Lag correlations averaged over the four dried simulations were –0.48 at 10-min lag time and –0.43 at 15-min lag time. Speculation on the reason for this relationship has not been developed.

Bursts of higher WR mixing ratio reaching the 1000-m level were often associated with subsequent increases in 75-m vorticity, though the lag time of this association was not consistent between simulations, with maximum values (0.35–0.55) ranging from 10–20 min. A representative example is shown from simulation MM (Fig. 12), in which lag correlation values of 0.48 were observed at 10-min lag and 0.61 at 20-min lag. The apparent association between WR mixing ratio and the vorticity field is more remarkable given that total WR content is generally lower in the RFD formation region than the other rain species (e.g., Fig. 10). Since the WR drop-size distribution (DSD) is biased toward small drops, greater WR content may lead to more evaporative cooling due to the greater total liquid surface area, and thus to an RFD momentum surge. This mechanism may be more important when the environment is drier and thus when evaporation can occur more readily, though the mechanism by which dry air affects the WR distribution should be investigated in future studies. The effectiveness of this mechanism also likely varies depending on where WR is occurring with respect to the RFD formation region, on dynamical influences, and on microphysical influence from nearby storms.

Dynamical effects also have important influence over the low-level vertical vorticity field. Processes related to storm dynamics may cause both the observed precipitation fallout and vorticity increase. For instance, updraft collapse has been observed around the time of tornadogenesis (e.g., Lemon et al. 1978). This collapse could cause a fall of hail in the minutes prior to tornadogenesis. It is thus unclear how much of the subsequent low-level vorticity increase is related to microphysical variability, and how much is tied to the larger-scale dynamics which caused the updraft collapse. Further research should be focused on decoupling these effects.

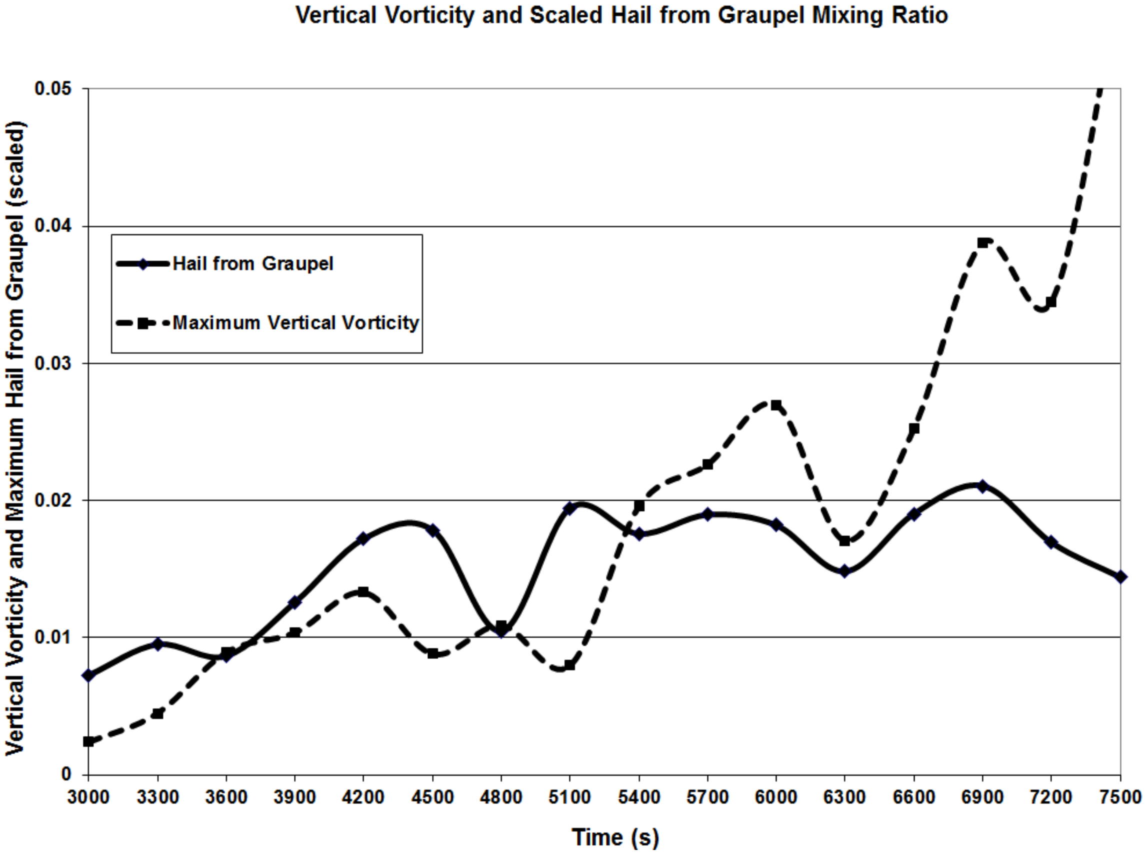

Figure 14: As in Fig. 12, except here the solid line denotes hail from graupel at 1000 m AGL scaled by a constant factor of 7.

7. Discussion and conclusions

Environmental dry layers substantially impacted microphysical distributions in simulated supercell storms, with effects on updraft characteristics and low-level vertical vorticity. Small ice particles were strongly influenced by depth of a dry layer rather than the magnitude of drying within that layer. Simulated storms with stronger updrafts contained fewer small ice particles, indicating the importance of evaporation and sublimation. Environments with shallow dry layers may produce storms with high mixing ratios of small ice particles, though on days with deep dry layers, fewer small ice particles may be expected to reach low levels. The environmental vertical temperature profile and height of the freezing level also must be considered before generalizing these results to other model output or nowcasting situations. The graupel distribution may take longer to become mature in supercells with a substantial midlevel dry layer, since supercooled droplets are more likely to evaporate. Mixing ratio of hail from frozen drops was greater in simulated storms with more drying, attributed to stronger updrafts and greater evaporative cooling of falling particles due to entrainment of dry air. A critical balance is likely to exist, with an environment dry enough to allow sufficient evaporative cooling for hail to reach the surface at one extreme, and a too-dry environment that destroys hail embryos on the other. In an environment with substantial midlevel dry air, the graupel distribution likely takes longer to mature, limiting the production of HG until later in the storm's lifecycle.

Observations indicate internal surges, or fluctuations in the magnitude, of the RFD (e.g., Lee et al. 2012). These fluctuations may be driven in part by microphysical variations. Based on simulations herein, storms in dried environments may produce substantial RFD surges if sufficient moisture is present to maintain a healthy updraft. Bursts of hail species and especially hail from graupel, followed by RFD westerly surges, preceded concentration of near-surface vorticity in most simulations by 15–20 min. This pattern was most pronounced in simulations with a deep dry layer. Thus, if a supercell is moving into an environment characterized by drier air at mid and upper levels, these results indicate that bursts of hailfall may be more favored to be followed by substantial RFD surges and subsequent increases in low-level vertical vorticity. Consequent impacts on storm structure and longevity likely depend on whether the storm is able to maintain unstable inflow (e.g., French et al. 2008). From a nowcasting perspective, our result may indicate that observations of a hail burst reaching near or to the surface, inferable from polarimetric radar data, may be followed by an increase in low-level vorticity. Radar observations of storms moving through changing environments would be useful to assess the value of these results.

WR mixing ratio often showed large lag correlation values with the low-level vertical vorticity field, though this association was not as strong as with hail variables and was more temporally variable. WR occurred repeatedly on the upshear side of the echo appendage, and was extremely variable in quantity. WR was especially disfavored in simulations with a midlevel dry layer. Mixing ratio increases in the echo appendage were often followed by increasing low-level vertical vorticity. The temporal variability of this relationship across simulations suggests the effect of WR on the RFD is very sensitive to additional factors. Given the distinct characteristics of the WR DSD, it may be possible to detect rapid increases in WR mixing ratio using polarimetric radar data. The potential operational usefulness and scientific value of these environmental–microphysical linkages suggest that future studies should consider these themes more thoroughly.

ACKNOWLEDGMENTS

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REVIEWER COMMENTS

[Authors' responses in *blue italics*.]

REVIEWER A (William A. Gallus):

Initial Review:

Recommendation: Accept with minor revisions.

General comments: I found this paper to be generally well-written and of potential great value to forecasters dealing with supercells and possible tornadogenesis. I recommend publication after a few rather minor revisions are made. These revisions are listed below:

The author thanks this reviewer for several excellent suggestions which have improved the manuscript.

Substantive comments: It would be nice to know why midlevel drying did not have the negative impact with strong shear that it did with moderate shear.

In more strongly sheared environments, the low-level outflow is less likely to 'occlude' the mesocyclone, so cold air is less likely to cut off the unstable inflow (e.g. Adlerman and Droegemeier 2005). This effect may also be partially attributable to the lesser importance of microphysics in supercell temporal evolution as the shear becomes stronger and dynamical influences tend to predominate (e.g. Gilmore et al. 2004a). A note to this effect has been added to the text.

The last paragraph in section 3 is misleading. You say that significant drying at any level was unfavorable for warm rain, but Table 1 shows for deep drying, very little negative impact for the significant drying (and actually an increase for moderate drying) compared to the control. You should change the wording. It would be nice to see some comment about why the deep-layer drying had such little impact here when the midlevel drying had a pretty big impact.

After further thought, the author agrees that this statement was misleading. Simulations with significant drying (SM, SD) had lower mixing ratio values than the corresponding simulations in which the same layer was dried but by a lesser magnitude. The significant-drying simulations had lower WR mixing ratio values than the control (though for SD this was not a significant difference). Both simulations with midlevel dry layers had lower WR values relative to the control, while simulations with deep dry layers had WR mixing ratio values relatively similar to the control. The text has been updated to more accurately reflect the values in Table 1.

The formation process for WR begins at relatively low levels [drizzle droplets collide and coalesce, rather than droplets being shed from ice particles (RS) or being created as ice particles melt (RM)]. Maximum drying was imposed at an altitude around 4.7 km (midlevel dry layer simulations) vs. 7.6 km (deep dry layer simulations). Thus, it is the author's belief that midlevel dry layers should more strongly impact the WR mixing ratio, especially at lower levels (such as 1000 m, the focus of this study), than dry layers in which the maximum drying compared to a control situation is at higher elevation. A note to this effect has been added to the text.

In the conclusions, I'm not exactly sure what you mean when you say WR was the thing most associated with storm evolution. The numbers in Table 1 do not seem to set WR apart as being particularly sensitive. I'm assuming your discussion is more related to Fig. 6 but it also looks to me the variations shown there are not much more remarkable than those in Fig. 5, and in terms of actual magnitudes, less than those shown with hail in Fig. 2. You probably just need to tighten up your wording and be more explicit about what exactly you are saying here. It is particularly important you clarify this item since it seems to be the key finding from the research, or at least the item you choose to focus on as a possible useful future tool for forecasters anticipating possible tornadogenesis.

Section 5 (the new Section 6), 'Microphysical influence on near-surface vertical vorticity', has been substantially rewritten in response to other reviewer comments. Lagged correlation statistics are now included linking particular species more concretely to low-level vertical vorticity values. Thus, there are some new conclusions about which hydrometeor species are most associated with storm evolution. The Discussion and Conclusions section has been updated accordingly, and the author thinks these changes address the reviewer's concern in this point.

Second Review:

Reviewer recommendation: Accept with minor revisions.

Substantive comments: I believe the authors have made some good changes to address my previous concerns. However, I believe there are still a few other areas where the clarity needs to be improved, and I've listed them below:

It is unclear what you mean when you talk about the mesocyclone being similar in the control and midlevel dry layer runs, and then how that relates to Gilmore's work. I think you need more explanation here (perhaps just one clarifying sentence). I do not see the connection between your finding and theirs.

Since these storms have stronger updrafts, they might be expected to contain more precipitation, and it's shown later that these storms did indeed have larger mixing ratios of many types of precipitation. So, the importance of the stronger updrafts is that this finding would lead us to anticipate the larger precipitation quantities which were later noted in the simulations. I have added clarification in the text.

[W]hen you mention a strong updraft pulse and then in parentheses mention >10 m s^{-1}, it is unclear if this is just the magnitude of vertical motion here, or if you are referring to the pulse itself—meaning the magnitude of some sinusoidal variation superimposed on a background vertical motion. Please clarify.

This is the magnitude of vertical motion within the updraft pulse. This has been clarified.

In section 6, you speculate that the WR might be correlated with increased near-surface vorticity via increased evaporation, and that the same might be true for hail due to melting. You imply the increased evaporation and melting, both of which produce cooling, would be responsible for increased RFD and enhanced vorticity. Yet, Markowski et al., whom you reference early in the paper, talked about how storms with cooler RFDs were less likely to be tornadic (implying less strong low-level vorticity, I would think). This result seems to oppose your findings. Because you do mention the Markowski et al. results early on, you owe it to your readers to discuss this apparent contradiction in section 6 or the conclusions. It would seem to me that your mechanisms for explaining the increased near-surface vorticity would lead to colder RFDs, which should be less conducive to tornadogenesis. Please provide some extra discussion about this issue. I'm guessing maybe I am just overlooking some step, but if so, you should spell that out more clearly.

These downdrafts are colder than they would be without the sublimation/melting/evaporation, but this does not necessarily mean they are cold. In an earlier modeling study with many more simulations, we often saw that storms with a lot of evaporation or melting didn't necessarily contain cold RFDs, but moderate-temperature RFDs. There seemed to be some intermediate point between very warm and very cold RFDs that was optimal for low-level vorticity spinup, and this often seemed to be reached via some evaporation or melting influencing an otherwise-warm RFD. I have added a note mentioning this possible effect in the text.

Also, while giving this more thought and over the course of reading recent literature, I have come to think that the key importance of precipitation bursts (hail, warm rain) may be in the generation of internal RFD momentum surges. While simulated storms typically contained an RFD all the time, it was much stronger at particular points, often corresponding to a period just following a burst of warm rain or hail. Such internal surges have been linked to tornadogenesis and intensification (e.g. Lee et al. 2012; Karstens et al. 2013). This potential mechanism for generating RFD surges has been added to the paper.

These discussions have been added primarily to the first paragraph under Section 6, with some minor wording changes throughout Section 6 to better-reflect these concepts.

REVIEWER B (Daniel T. Dawson II):

Initial Review:

Reviewer recommendation: Decline.

General comments: This study examines the impact of mid- and upper-level drying on idealized simulations of supercell storms, with an emphasis on the differences in microphysical behavior. The topic is certainly scientifically important, as the interplay between thermodynamics and microphysics in supercell storms is still poorly understood, and is relevant for EJSSM. However, after carefully reading the paper, it is my opinion that the paper in its current form cannot be acceptable for publication in EJSSM, mainly due to lack of sufficient detail in the discussions of both the methodology and the results. In general, most of the analysis and interpretation of the results strikes me as rather superficial, even given the study's stated goal of discussing "qualitative and conceptual differences" amongst the experiments. There are many claims made in the article about the behavior of the hydrometeor distributions vis-à-vis the impact of the mid-level drying, but with a dearth of supporting arguments from the analysis presented, when it seems to me that even a modestly more detailed analysis (such as examining vertical structure of the distributions) would provide such. While many of the explanations given regarding the differing microphysical behavior and the impact of the mid-level dry air aloft are at least plausible, this lack of detail is systematic and makes it extremely difficult to assess the scientific validity of the conclusions of the manuscript in its current form. Some of the findings, taken at face value, are interesting and worthy of further investigation and as such this recommendation should be considered a "soft reject". I encourage the author to re-assess the study and provide sufficient detail in a future submission. Specific substantive comments are provided below.

The author thanks this reviewer for the many excellent suggestions provided for improvement, and for the substantial effort taken by the reviewer to produce this helpful review. The most substantial recommendation is for additional analysis leading to better-supported speculations of microphysical behavior in simulated supercell storms. The author agrees that the suggested analyses and explanations would substantially strengthen the paper, and has attempted to address the reviewer's comments where possible. Shortly after this particular piece of research was completed, however, the author left the organization where it was completed, and the author's original research group moved their research in a different direction. The author was allowed to publish results obtained in this work, but without further use of related resources. This included the author's ability to generate graphics or further simulations. The author has taken substantial time trying to find a way to visualize the simulations to address some of the reviewers' concerns, but has not been successful. Thus, some requested graphics are not included, and some of the additional analyses the author would like to complete (and that the reviewer has requested) have not been possible. It has been suggested by the editor that it may be possible to rework this submission into a shorter article, or into a research note. After seeing how the reviewer's comments have been addressed below, would it be possible for the reviewer to indicate if they think this article can remain in its current format, or if one of these other options may be more appropriate?

Substantive comments:

Methodology: The author presents a figure showing the hodograph that is used for each simulation, but somewhat mystifyingly to me, does not show a corresponding figure showing what was actually different between the simulations, to go along with the discussion in the text. It would be most helpful to visually see what the thermodynamic profile looks like for each of the simulations, since the impact thereof is the main focus of the paper. Also, in two of the drying simulations, the amount of drying is given in absolute units (g kg^{-1}), while in the other two, it is given as a percentage. This makes it impossible to assess the relative differences between these experiments without knowing what the control profile looks like. Also, another substantial omission is the location of the melting level relative to the dried layers. Obviously this has a direct impact on the melting of, e.g., hail particles that might fall through a drier layer below the melting level, and thus have their rate of melting slowed, but without knowing what the melting level actually is, this impact cannot be assessed.

The author agrees that an illustration of the thermodynamic profile for each simulation would be helpful, and has added a Skew-T diagram for the control simulation and each of the 4 dried profiles. The 0 °C isotherm has also been highlighted on these diagrams to show the freezing level, and the dried layer has been delineated. This will facilitate the comparison between the dried layers and freezing level.

The discussion of amount of drying has been modified so drying in all simulations is discussed in the same units (g kg^{-1}).

It was discovered in the process of making the Skew-T diagrams that the wind profile for all simulations was not as originally reported. The hodograph diagram has been modified to reflect the correct wind profile.

Description of model: More detail about the model setup needs to be provided. What was the size of the domain? What was the location and dimensions of the initiating thermal bubble?

Information about the size of the domain and the location/dimensions of initiating thermal bubble have been added.

Description of the microphysics scheme: More detail simply needs to be provided here, especially since no reference is made to a previous study that uses or describes this scheme. This strikes me as a serious omission, since the reader has no way of evaluating the efficacy of the scheme, having no reference point. Either a reference to a previous study describing this scheme needs to be provided, or a detailed description (perhaps in an appendix) should be provided within the current manuscript. Specifically, more information needs to be provided about the nature of the individual hydrometeor distributions. Don’t expect the reader to know the difference between hail from frozen drops and hail from graupel without a description, for instance. Also, why was a height of 1000 m chosen for the analysis, especially in light of the fact that the drying was applied over layers much higher than this? Of course, effects could be seen at 1000 m as the hydrometeors sediment out of these layers, but why not also discuss what they look like within this layer? More justification simply needs to be provided for this choice.

Several references have been added which describe the model and its microphysics scheme. Also, several references have been added describing prior studies which used this and earlier versions of this model/microphysics scheme. In addition to the references, a brief overview of model microphysics has been added near the end of the Methodology section in which the primary hydrometeor species are described.

1000 m was initially chosen as a level at which microphysical influence on the RFD would be noticeable, since most RFDs originate near or above this level (Markowski 2002). In a later paper, Markowski found that air in the immediate tornado vicinity within the RFD may originate at altitude <1 km in many significant tornado events (Markowski et al. 2002), though the air farther back in the RFD likely came from at least 1 km elevation. So, looking at 1 km should provide a useful measure of microphysical influence on the RFD. Justification along these lines has been added to the paper. Looking at a higher level (or better, cross-sections) would be ideal, but the author lacks the resources to produce these visualizations for these simulations.

Section 3a,b,c: Why are no figures provided illustrating these results? Without them, evaluating the microphysical explanations made in this paragraph is extremely difficult. Again, this is another example of a lack of detail. And while the claims given in this paragraph about the reasons for the relative locations of the hydrometeors are plausible, more evidence should be provided supporting these statements. Figures showing the horizontal and vertical structure of these fields and possibly their time evolution would go a long way toward providing this evidence.

Figures have been added showing typical distributions of the microphysical variables discussed in the first paragraphs of sections 3a/b/c.

The author strongly agrees that cross-sections and temporal variability of these fields should be assessed, but is currently unable to produce the required visualizations of model output.

Section 3a: I find the simple use of a maximum mixing ratio by itself with no other supporting metrics somewhat problematic (presumably this is the maximum mixing ratio at 1000 m AGL averaged over the times indicated in Table 1). I think that some sort of sum of total hydrometeor mass or flux at this level (or better, at more than one level) in addition to the mere maxima, would be much more revealing, especially since the author repeatedly talks about this or that simulation producing more or less hydrometeor mixing ratios, but the maxima alone aren't enough! I'm not saying don't use this metric at all, but it would be most helpful in evaluating the results if other relevant metrics were used alongside it. Also, again, why only look at 1000 m? Many of these hydrometeor categories (such as frozen drops), would be expected to be most prevalent at higher altitudes, within the very dry air layer the experiments vary. Vertical cross sections might also help with this. You say, e.g., "Thus these drops were either not freezing, or had sublimated by the time they reached 1000 m." Well, you presumably have the capability of checking this by looking at levels higher than 1000 m. This criticism applies also to the description of other hydrometeor distributions elsewhere in Section 3.

Yes, this is the maximum 1000-m AGL mixing ratio averaged over 3000–7200 s. The author strongly agrees that a summed measure of hydrometeor content (total mass; mass flux through a layer) and/or cross-sectional analysis would improve the quality of results, but does not have the visualization tools to investigate this further. Ice content is higher aloft, but as noted above, 1000 m was the altitude of focus for investigation of microphysical impact on the RFD.

Total mass of a given hydrometeor species and maximum mixing ratio of that species were seen to be <u>highly</u> correlated while the work was ongoing, but the author cannot quantify the degree of correlation. The author has added some text along these lines to the final Methodology paragraph.

Section 3a: A (speculative) explanation is given here for why deep-layer drying might result in less graupel (assuming there actually is less graupel, and not simply lower maximum values, see above), but no explanation is given for why the experiments with only midlevel drying produce more graupel.

Yes, there actually was less graupel in this simulated storm. The author is not convinced of a reason why this was the case. It may be related to the increased concentration of frozen drops, which often serve as nuclei on which riming occurs, leading to graupel. This speculation has been added to the end of the referenced paragraph.

Section 3b and Figure 2: The metric here "total hail mixing ratio" is misleading. I know that it is supposed to be the sum of hail from frozen drops and from graupel, but it is still only the maximum value at 1000 m AGL, right? If so, this needs to be explicitly stated.

Yes, this is the sum of 1000-m maximum values. The text and figure caption have been altered to make this clear. The metric has also been renamed 'total maximum hail mixing ratio' for clarity.

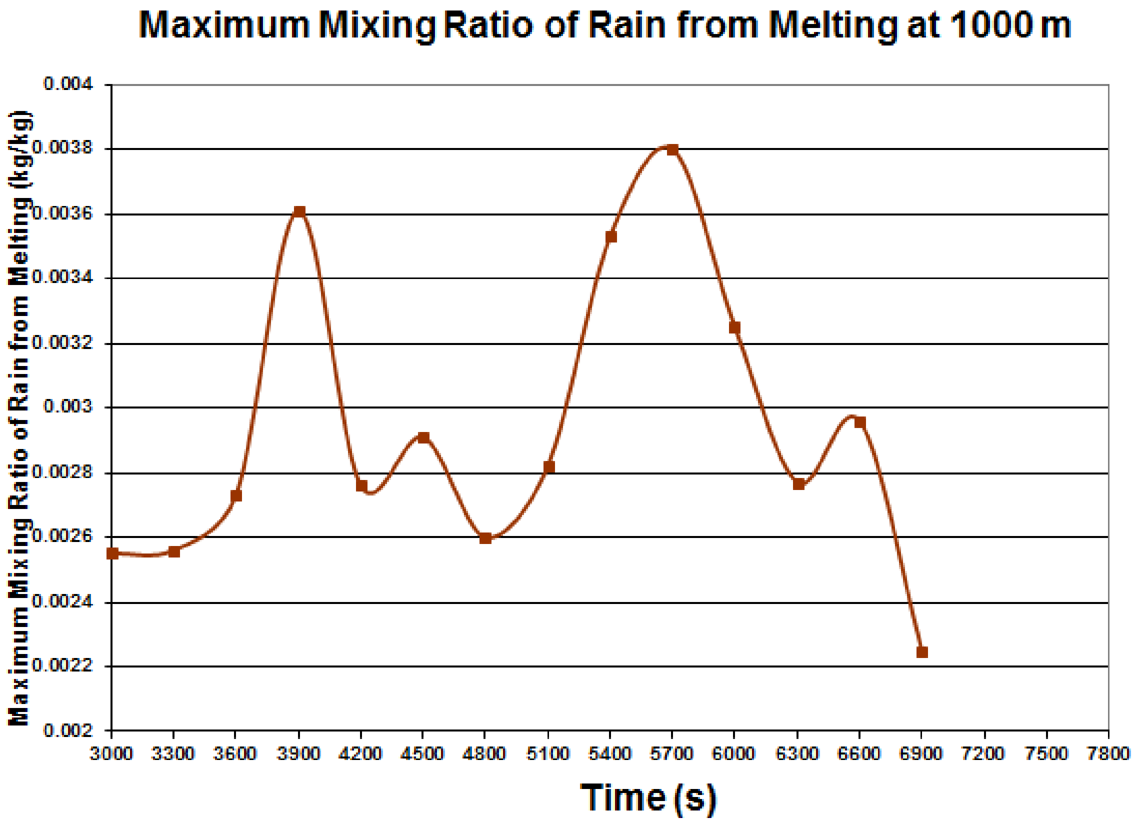
Section 3b: "though HFD generally extends farther south relative to the echo appendage when the deep layer is dried", and "HFD may also remain farther west relative to the updraft region in storms with deep-layer drying": I confess to failing to see a substantial difference in the simulations along either of these lines when closely examining Figure 2. Incidentally, it might help guide the reader if the experiment names were given some simple abbreviations and the figure panels were labeled with these names.

First, each simulation has been given a unique one- or two-character identifier, introduced in the Methodology. References to the simulations through the rest of the paper, including in the figures, have been converted over to these identifiers.

The author believes what was meant in the 2 statements referenced regarding the hail distribution under different drying regimes can be refined and clarified as follows:
“The axis of highest HFD values was generally west-east oriented in storms with only midlevel drying, and generally southwest-northeast oriented in storms with deep-layer drying. As a result, higher HFD mixing ratios wrapped around the northwest side of the updraft region in storms with deep-layer drying.”
This text has replaced the original wording…

9. Section 3c: “In most simulations, RM content was roughly cyclic…”: where is this shown?

This is not currently shown in the paper. Here is a typical example of how RM content varied with time (from the simulation with significant midlevel drying, SM):



This is an expected result, given that cyclic bursts of ice species (especially hail) occur. I’ve added a note to this effect and a (not shown) at the location in the text referring to cyclic RM content, since this seems incidental to the main point of the paper.

Section 3c: “A drier environment may favor production of smaller ice particles, which melt more readily”: Presumably, it would also retard the melting of said particles, provided the drying was at least partially below the melting level, of course (see point 1 above).

Looking at the vertical profiles now provided, little of the drying was experienced by ice crystals below the melting level. ≈50 mb of drying would be experienced above the melting level, and given how the drying was applied as a sine curve with maximum drying at the center of the layer tapering to zero drying at the edges of the layer, ice crystals would experience very little of the drying. Thus, though melting would be somewhat reduced by the drier air, this effect is likely small. So, the increase in RM makes microphysical sense given the location of the dry layer. A note to this effect has been added in the paper.

Section 3c: “Once liquid is shed, it may evaporate more readily in a drier environment”: This seems uncontroversial, but depends again on where the melting level is relative to the dry layer.

Given the comment above, shed melt water would only have ≈50 mb for extra evaporation to occur if shed at the melting level. Some liquid could also be shed from hailstones above the melting level, and these drops (which have mean diameter of 1 mm) would experience more evaporation. Minor changes have been made to the text.

Section 3c: "Maximum WR mixing ratio values were highly cyclic…": again, where is this shown?

The original Fig. 6 (new Fig. 12) shows a typical example of how WR content varies cyclically. The author has added reference to this figure and noted that it is a typical example.

Section 3c: "Significant drying at any level was unfavorable for warm rain…any significantly dry layers should readily evaporate the drizzle droplets involved in WR formation.": but, the drying imposed in this study was aloft (>3 km), whereas the warm rain distributions discussed here were in the low-levels (1 km). Without further information, it is not possible to ascertain whether the dry air aloft directly resulted in the evaporation of the WR. That is, did the WR fall through this dry air layer directly, and thus get partially depleted by evaporation? Or, did the dry air descend into the low levels in the RFD, causing greater evaporation of WR? Neither of these possibilities are discussed, when it strikes me that this question could easily be answered by looking at the vertical structure of the WR and the downdraft thermodynamic properties within the simulation. Even a cursory examination should suffice.

The author agrees that this analysis would significantly strengthen the conclusions presented here, and would be of great importance in understanding WR variations in supercell storms in general. Unfortunately with the lack of resources relative to model output visualization I cannot examine this question in more depth, in this study. This section was also modified in response to another reviewer's comments, and evidence was provided that evaporation was likely a key contributing mechanism.

Discussion of the two mechanisms noted by the reviewer has been added, and the end of the paragraph has been reworded to better account for these possible mechanisms.

Section 4: At what level was the updraft examined? From the methodology section, it would seem that the updraft at 1000 m was only examined, but presumably the updraft in the midlevels would be more appropriate to examine in the context of this study. This is especially so since a reference is made to James and Markowski (2010) in regards to the midlevel updraft strength in their study. Why not compare apples to apples here? Again, the simulation has this information.

The updraft was examined at 1000 m (this was clarified in the text). Since the author does not have the ability to examine other levels, the reference to James and Markowski (2010) has been removed from this discussion. The author believes that an ideal study would include total updraft volume in addition to midlevel and low-level measures of updraft intensity and extent.

Section 4: "Simulations with significant drying produced a first well-defined westerly surge 5–15 min prior to simulations with only moderate drying.": Where is the evidence shown for this? It occurs to me at this point that many of these claims with no supporting evidence or illustration in the form of figures/tables could be qualified by putting a "(not shown)" within the sentence, but this should be done sparingly, of course, and only for points that are incidental to the overall point of the study. Most of these claims don't appear to fall into that category.

The author agrees that this should be shown if the result is to be retained. This information was well-quantified for other simulations, but not rigorously quantified in these simulations. Given the lack of rigorous quantification (e.g. magnitude of a 'well-defined' westerly surge; exact temporal difference between simulations), the author has decided delete much of this paragraph. The latter part of the paragraph has been retained (more rapid low-level vorticity concentration in simulations with significant drying). This material has been updated to include more quantified discussion of the differences between simulations, including time to first production of quarter-maximum and half-maximum vorticity values at the lowest model level.

Section 4, regarding the discussion of low-level mesocyclone evolution in the face of stronger outflow: It's worth pointing out here that these ideas are not new. Very similar arguments are made in Brooks et al. (1994), Davies-Jones and Brooks (1993), and Gilmore and Wicker (1998) to name a few.

Agreed. The author added Gilmore and Wicker (1998) and Adlerman et al. (1999) references to the restructured form of this paragraph.

Section 5: Overall, I think the claimed link between graupel/hail/WR bursts and subsequent increase of low-level vorticity appears rather tenuous. Can the author provide more substantive analysis, perhaps through the use of lagged-correlation statistics or more supporting figures? Otherwise, this section as it stands is too speculative in its current form, and as with many previous sections, it seems that digging a little deeper into the analysis of the simulations could help alleviate this somewhat.

Lagged-correlation statistics have been computed for all microphysical variables at 300, 600, 900, and 1200 s (e.g., correlating the vertical vorticity value to the mixing ratio of a given species 5, 10, 15, and 20 min prior). This method produced a much nicer, quantified look at how the larger parameter space behaved. It was found that the hail variables each showed a significant positive lag correlation at particular lags (larger hail mixing ratios were associated with higher low-level vorticity values at a future time). Frozen drops showed a substantial negative correlation with vorticity at 10–15-min lags (larger frozen drop mixing ratios corresponded to decreased vorticity values). Graupel and rain variables showed little pattern, except warm rain, with which moderate 10- or 20-min lag correlation values were often observed. These results have been added to the text. The associated figures have been updated to reflect more appropriate hydrometeor species. The author considered adding a table of lag correlation values, but thought this would add too much clutter when the same information could be conveyed in words.

Section 6: Overall, this section contains far too much of what strikes me as speculative language (i.e. too many uses of the word "may") that has only tenuous connections to the results of the study. Some examples include, "The environmental temperature profile must also be considered before applying these results"—applying these results to what? "Greater evaporative cooling should occur with a deeper dry layer, though the importance of the cooling effect may decrease as the storm ages and creates its own environment."—How? "If a supercell is moving into an environment characterized by drier air at mid and upper levels, the storm may in some cases be anticipated to produce more significant RFD surges."—In what cases?

Speculative language has been reduced in this paragraph. In response to the examples noted by the reviewer:

1) "The environmental temperature profile must…": This has been altered to note the importance of considering the vertical temperature profile and height of the freezing level before generally applying the results presented here to either model output or nowcasting situations.

2) "Greater evaporative cooling…": The idea here was that once a storm has well-developed updrafts and downdrafts/cold pool, and has moistened its surroundings (Johnson et al. 1987), the effects of drier air aloft are likely of lesser importance. Given that it's impossible to quantify this effect, though, the author has removed this statement.

3) "If a supercell is moving into…": This section has been modified in the process of rewriting the portions of the conclusion relating to specific hydrometeor species and RFD surges (reflecting the results of the lagged correlation analysis).

Second Review:

Reviewer recommendation: Accept with minor revisions.

General Comments: This paper has improved substantially from the first submission, and I feel the author has satisfactorily addressed most of my original points of criticism, particularly in regards to providing additional detail and physical explanations. Ideally, I still think it would be most desirable (and the author agrees) for the author to provide a more detailed investigation (i.e., examining specific microphysical processes, particularly at mid to upper-levels). In this regard, I strongly encourage the author to consider one or more follow-up studies along these lines. However, after much consideration and given the logistical constraints revealed by the author for the current study, I think there are enough interesting new findings and open avenues for future research that the manuscript could be acceptable for publication in EJSSM pending some remaining, mostly minor issues described below. In particular, there are still a few conceptual issues regarding the role of dry air on the frozen-hydrometeor phenomenology that need to be cleared up.

Substantive Comments: The new figure (Fig. 2) showing the Skew-T's for the different experiments is most helpful for visualizing the differences. First, I find it worth pointing out that each profile features a prominent low-level moist absolutely unstable layer (MAUL), and a very small dry layer near the surface, which hardly seems representative of an environmental supercell sounding as stated in the text [although it's plausible that a storm-modified inflow sounding may exhibit a MAUL—see Bryan and Fritsch (2000)]. How does this affect the interpretation of the results? Clearly, rain falling into this layer in the inflow of the storm will have virtually no evaporation potential, for example. Second, at approximately what height AGL is the melting level? Please provide this information in the text, if not also in the figure caption.

This moist layer is a good example of a MAUL, and this has been acknowledged in the text. Following Bryan and Fritsch (2000), it has also been noted that this type of sounding may be present in a convective inflow region when strong mesoscale ascent is occurring. The dry layer near the surface is approximately 0.7 km deep with a maximum dewpoint depression of 2.9 °C. So there is still evaporation potential in this lowest layer, though certainly less than if the air was subsaturated to higher elevation. Evaporation [and] sublimation potential is mostly focused above 700 hPa.

Cooling by evaporation of precipitation falling into this layer would occur mostly in the lowest half-km, meaning that a cooler pool of air forming as a result would be quite shallow. It doesn't appear that cold pool depth has been investigated often with respect to the vertical moisture profile, but this might be a quite interesting study.

The melting level is at approximately 643 hPa (3728 m); this has been added to the text and figure caption.

Section 4a: "Sublimation may contribute as for frozen drops, though evaporation of the supercooled drops required for graupel formation is likely more important.": Could the author explain this in more detail? It seems that both of these might indeed be important, but without further information, it's difficult to see how a judgment can be made either way. Also, most of the graupel formation by this mechanism would be in the updraft and/or cloudy regions of the storm, where saturated conditions would be expected. On the other hand, if significant dry air entrainment is occurring, this could indeed reduce the overall amount of supercooled drops available for conversion to graupel.

The author's original thoughts were along the lines of what the reviewer has proposed here: entrainment of drier air should deplete the supercooled droplet population which would be involved in graupel growth. Thus graupel would grow more slowly, but it can't be said with certainty that this effect is larger than sublimation of existing graupel. So, the sentence about evaporation being relatively more important than sublimation has been modified to stress the potential importance of both, and a brief discussion of the role of entrainment has been added.

Section 4c: "Stronger updrafts may also produce more small ice particles, which would melt more easily. This is especially true given the location of the dried layer primarily above the melting level (Fig. 2), which would lower the melting efficiency of falling ice crystals by evaporative cooling." (See also my point number 10 in my original review, and your response): First, given the fact that most of the dry air is located above the melting level, I completely agree with you in your response to my original point that the melting would not be substantially retarded by evaporative cooling, and in fact the nearly saturated low levels would rather serve to enhance the melting instead. However, with this in mind, I see two problems with the explanations given in the text. The first refers to the quoted sentence, where I think the second part should read "which otherwise would lower the melting…". Second, this fact (apparently) directly contradicts the following statement made earlier in Section 3b: "…simulations with a deep dry layer typically had higher maximum hail mixing ratio, which is consistent with more hail mass surviving as hailstones fall in drier environments due to increased evaporative cooling (e.g. Rasmussen and Heymsfield 1987)". But, that only occurs in subsaturated conditions below the melting level…

One way this might work is if substantial mid-level dry air is entrained in the hail-bearing downdrafts below the melting level, which would indeed lead to less melting of the hail, all other things being equal, than if the downdrafts were moister. Is there a way of examining the relative humidity and vertical velocity at 1000 m with the dataset currently available? If so, an argument might be made either for or against this hypothesis.

First point: completely agreed. I think this wording may have been left over from a prior draft, so I have modified it as suggested.

The sub-saturated depth below the melting level is ≈120 hPa (1428 m), with a maximum dewpoint depression (at the melting level) of ≈3.4 °C. This seems to be sufficient for some reduction in the mass of hail melted while falling through this layer. The author believes the reviewer's speculation that dry-air entrainment within hail-bearing downdrafts may be a key contributor as well. The text has been reworded to reflect this speculation (in section 4b) and the observation of sub-saturated layer depth below the melting level (in the sounding description).

Unfortunately there is not a way to examine relative humidity in association with downdrafts to test the dry-air entrainment hypothesis.

Section 4c, discussion of warm-rain (WR) evaporation: Now that the new information about the very shallow depth of the dry air below the melting level has been provided, it seems implausible to me that any substantial amount of WR would evaporate by falling through the relatively undisturbed environment, since most of the WR would be produced well below the melting level and thus fall into mostly saturated air (and most of the rest would freeze in the updraft above the melting level. Thus it seems much more plausible to me that the reason why there is less WR in the mid-level-drying experiments (particularly in SM) is due to the greater prevalence of drier downdrafts in the rear flank entraining and evaporating the WR once formed. Again, can vertical velocity/RH be examined at 1000 m?

The author believes that entrainment of dry air is likely a strong contributor, and has added this speculation. Unfortunately, relative humidity or an equivalent measure is not available.

Another consideration is that WR can exist at temperatures much colder than 0°C. Though I don't think it's well-known how common this is in deep convective clouds and to what temperatures supercooled drops can exist in such environments, supercooled droplets grown by collisions have been observed with cloud-top temperatures of –10°C to –12°C (e.g. Huffman and Norman 1988; Cober et al. 1996), and in relatively pristine orographic convective clouds down to a temperature of –21°C (Rosenfeld et al. 2013). Though temperature to which WR can be cooled before freezing depends strongly on the aerosol distribution, it is evident that WR often exists at temperatures below freezing, and probably fairly often down to –10°C. This increases the depth over which WR may evaporate for the environment represented by the soundings. For instance, if we allow supercooled WR to be present down to –10°C, this increases the sub-saturated environmental depth to 2.9 km. I'm hesitant to include too much about this in the paper because it is so speculative in the deep convective environment, but have added the possibility that a layer of supercooled WR may be present, increasing the depth over which sounding drying would have an impact on WR evaporation.

[Minor comments omitted...]

REVIEWER C (Adam J. French):

Initial Review:

Recommendation: Accept with major revisions.

General comments: This manuscript examines the impacts of middle and upper tropospheric dry air on the evolution of hydrometeor species within supercell storms using idealized model simulations. A control simulation using a moist environment is compared to a series of simulations run with profiles that have two different magnitudes of drier air present over differing depths of the sounding. Results suggest that the depth and magnitude of dry air have differing impacts across the different hydrometeor fields examined. Some connections are made between the changes to hydrometeor mixing ratios and low-level RFD and vertical vorticity evolution within the different environments.

This study presents some useful findings, and advances our knowledge with regards to the environmental sensitivity of hydrometeor evolution. This of great importance given the growing body of work suggesting that features such as RFD temperature, which is tied to microphysics, is likely an integral part of tornadogenesis. However, while some novel results are presented, I feel that some additional analysis and discussion is necessary to bolster some of the claims being made. In particular, it is not entirely clear the degree to which some of the dynamical responses to the different background environments (namely updraft strength) may be altering some of the hydrometeor fields presented, rather than just the interaction between the hydrometeors and the dry air. In light of this, I am recommending that this manuscript undergo major revisions prior to being accepted for publication in EJSSM.

The author thanks this reviewer for a thorough review and for many excellent suggestions provided for manuscript improvement. Most substantially, the reviewer has recommended additional discussion of storm-scale differences between simulations, which the author has attempted to provide. Also, the link between various hydrometeor species and the low-level vertical vorticity field has been strengthened via lagged correlation statistics. The manuscript has also undergone significant alterations in response to the recommendations of another reviewer.

Substantive comments: There appear to be a number of areas where some additional figures would be useful to supplement the descriptions provided in the text. In particular, the first paragraphs of sections 3a, b, and c and Section 4 all spend time discussing the location of features within the simulated storms. I think this could be done a lot more clearly if a simple overview figure was included for each section that highlighted the relevant fields being discussed (e.g. the relevant hydrometeor mixing ratio fields overlaying simulated reflectivity in 3a,b,c, and surface momentum and vertical vorticity in section 4). A few other suggestions for additional figures are included in the minor comments below.

Figures have been added showing typical distributions of the microphysical variables discussed in the first paragraphs of sections 3a/b/c. A figure has also been added to section 4 to illustrate the typical configuration of the updraft region, RFD westerly surge, and region of associated higher vertical vorticity values.

While I understand that the microphysical evolution is the focus of the paper, it would be helpful to include a short section between the current sections 2 and 3 to provide a brief comparison of the storm-scale differences between the 5 simulations, with a focus on comparing the control simulation to the various dry environments. This would provide an overview to help the reader assess and understand the microphysical results presented in section 3 compared to the overall storm-scale picture. Some simple overview plots showing simulated reflectivity, w, and perhaps vertical vorticity along with a discussion of key differences would likely be sufficient.

A new section has been added as requested (the new Section 3). In it, the author briefly notes some storm-scale differences between simulations. Figures have been added in which simulated Z_{hh}*, w, and vertical vorticity are plotted for the control and dried simulations at early and later times.*

One of my biggest concerns with the manuscript is that there seems to be an underlying assumption that the differences in microphysical output [are] completely due to the interaction between the various hydrometeor mixing ratios and the dry air (i.e., that the changes in mixing ratios are likely due to evaporation/sublimation of the various hydrometeor types). While this is likely part (and perhaps a large part) of what is going on, the analysis seems to be neglecting differences in overall storm structure and intensity between the different systems. For instance, based on figure 4, it appears that the simulated storms in the "deep drying" configurations both produce stronger updrafts (maximum values > 10 m/s) than the "mid-drying" or "control" simulations. These likely impacts the microphysical processes in the storm, playing a role in the larger mixing ratio values observed for several species in these simulations (e.g., the larger hail mixing ratios discussed in section 3b). In short, I'd like to see some additional discussion as to the degree that the differences between simulations may be a result of the changes in storm structure/dynamics owing to the different background environments in addition to the microphysical processes responding to the dry air (i.e., evaporation/sublimation).

The author agrees that storm-scale differences were generally neglected in the original text as a source of hydrometeor variability. The discussion throughout the paper has been modified to acknowledge this contributor to increased mixing ratios of various hydrometeor species, with a reference to Gilmore et al. (2004a) who found simulations with increased updraft strength also had larger total precipitation production (though their simulations compared ice and liquid microphysics schemes, and found stronger simulated updrafts with ice microphysics). The additional discussion was set up in the new Section 3 (storm-scale variability between simulations). Relative importance of evaporation/sublimation vs. updraft strength appears to be different for different particle types. In Table 1, small ice particles had smaller mixing ratios with deep-layer drying simulations. This is attributable primarily to evaporation/sublimation. Hail and rain variables generally had higher mixing ratios in deep-layer drying simulations, which is almost certainly mostly attributable to the higher precipitation particle production accompanying stronger updrafts. Given that the only difference between simulations was the vertical moisture profile, the discussion now hopefully better reflects the potential role of a variable environment affecting storm characteristics such as updraft strength, which affect hydrometeor distributions, in addition to the effects of evaporation [and] sublimation.

In section 5, I think that the connection between the oscillations in HFD and WR and vertical vorticity are somewhat tenuous, especially since they only are apparent in one simulation. To more effectively make this point I think the author needs to quantify the correlation between the hydrometeor (HFD and WR) fields and vertical vorticity pattern in some way. While I agree with the apparent connections illustrated in Figs. 5 and 6, the fields are quite noisy. I think some further analysis and discussion of not just the correlation between the fields, but the physical processes that might link them would provide a much better case for the claims being made. On page 8 it is suggested that the bursts in WR may produce additional evaporational cooling and drive a strong RFD surge. If this is the case, is there evidence of this in terms of the RFD characteristics? Furthermore, is there something special about the HFD and WR species that contributes to this, or are these just the predominant types in this region of the storm? In short, I would like to see some quantification that the HFD/WR fields are correlated with vertical vorticity (and how this correlation compares for the other hydrometeor fields too) and this correlation should be linked with some physical mechanism in that explains why the increase in the hydrometeor fields would be followed by spikes in vertical vorticity.

Lagged-correlation statistics have been computed for all microphysical variables at 300, 600, 900, and 1200 s (e.g., correlating the vertical vorticity value to the mixing ratio of a given species 5, 10, 15, and 20 min prior). This method produced a much nicer, quantified look at how the larger parameter space behaved. [T]he hail variables each showed a significant positive lag correlation at particular lags (larger hail mixing ratios were associated with higher low-level vorticity values at a future time). Frozen drops showed a substantial negative correlation with vorticity at 10–15 min lags (larger frozen drop mixing ratios corresponded to decreased vorticity values). Graupel and rain variables showed little pattern, except WR, with which moderate 10- or 20-min lag correlation values often were observed. These results have been added to the text. The associated figures have been updated to reflect more appropriate hydrometeor species. The author considered adding a table of lag correlation values, but thought this would add too much clutter when the same information could be conveyed in words.

As a result of the lagged-correlation analysis, it was found that hail from graupel had a more consistent influence on low-level vorticity values than hail from frozen drops. This is likely because, as the reviewer suggests, the total hail content is dominated by HG rather than HFD. A note to this effect has been added to the text. WR is more common in the RFD formation region relative to other locations within simulated supercells, but still has smaller mixing ratios there, generally, than the other rain species (e.g. the new Fig. 10). This suggests fluctuations in WR content may have some special significance to RFD characteristics. The description of physical mechanisms which may link the identified species with RFD characteristics (and therefore to the vertical vorticity field) have been strengthened.

[Minor comments omitted...]

Second Review:

Reviewer recommendation: Accept with minor revisions.

General comments: I would like to commend the author on the extensive revisions completed in regards to my comments on the original version of the manuscript. I have found the revised version has addressed my concerns and has clarified many of the questions I had originally. I particularly find the addition of section 3 and figures therein to be very helpful in illustrating the differences between the simulated storms before delving into the intricacies of the microphysical evolution. I do have some (very) minor additional comments listed below, most of which are just requests/suggestions for further clarification of a few points. I do not need to review a revised version unless the editor deems it necessary.

[Minor comments omitted...]