General Comments:

The authors are to be commended for a manuscript and research that works toward answering a novel and important question in the study of precipitation processes. The leveraging of a suite of ground observations, combined with additional fields that must be obtained from NWP, works quite well to provide an analysis of this case. However, there are some important issues that need to be resolved before it should be accepted for publication. One of these issues, as I mention below, is fundamental to the entire purpose of this study, and therefore it is imperative that it be fully rectified.

Specific Comments:

P2,L1-16: This section needs a brief discussion of Atmospheric Rivers (ARs) and IVT, and a few citations about them. The first question I would like answered in here is: "are WCBs and ARs the same thing by definition? Is every WCB, and every location within a WCB, an AR? Is every AR a WCB?" I have seen both in the literature but do not know the difference.
A good start for citations would be Rutz et al. (2014):
https://journals.ametsoc.org/doi/full/10.1175/MWR-D-13-00168.1
The reason I suggest it as a good start is the usage and definition of IVT. The current study uses IVT, but I cannot find it defined anywhere. Also, since Rutz et al. define an AR as having 250 kg $m^{-1}s^{-1}$, the authors should consider beginning their IVT contours at 250 in Fig. 2.

P2,L22-30: This portion of the review needs improvement. The general suggestion is that the authors need to provide the reader with the relevant background on dual-pol signatures in ice-phase and mixed phase situations, so that they are prepared to properly interpret the upcoming results they are about to encounter. Specific suggestions:

a) The sentence "Grazioli et al. (2015) suggested that similar peaks in Kdp can result from secondary ice generation" is not representative of the Grazioli work. This should be changed to "Grazioli et al. (2015) suggested that similar peaks in Kdp can result from secondary ice generation or the riming of ice crystals with anisotropic shapes."

b) There are three quotes in here (doi:10.15191/ nwajom.2013.0119) that contain great info for the reader:
"The exact value of Z_{DR} depends on the crystal density; solid ice particles such as hexagonal plates can have intrinsic Z_{DR} values larger than 6 dB, in some cases even approaching 10 dB (e.g., Hogan et al. 2002), whereas the Z_{DR} in dendrites generally remains below about 4–5 dB. However, because of particle wobbling, imperfect shapes, and a mixture of crystal types usually present in clouds, observed Z_{DR} values in ice crystals usually do not exceed about 4–5 dB."
"In contrast to the pristine ice crystals, large aggregates are observed to have very low Z_{DR} (<0.5 dB). This is primarily attributable to their very low density (usually <0.2 g cm^{-3}, compared

to the density of solid ice of 0.92 g cm^{-3}), which makes their exact shape less important from the radar's perspective. "

"Additionally, increased fluttering of aggregates tends to keep Z_{DR} quite low. Note that, because of their large sizes compared to pristine crystals, snow aggregates tend to have larger Z_H values. Observations of Z_H increasing towards the ground coincident with Z_{DR} decreasing towards the ground are consistent with ongoing aggregation." Please include this information (in a few sentences) in this section.

Figure 2: the authors should consider decreasing the size of the geographic domain of the subplots, as it would be easier to see what is happening over Korea. I know there is a need to see the broader environment for cyclogenesis, etc., but I think a sizable reduction could be accomplished without losing any important large-scale details. Also, the locations of the labels of IVT contours could be made smaller and moved to locations that have fewer things drawn over the top of one-another. For example, the 1000 label in 2c is probably not needed – the reader can infer that.

P6,L10: why not also "(iii) advection of liquid droplets from below to above freezing level"? This would technically become new SLW, correct?
Or change the sentence to "The increase on LWC is likely a result of…", and the rest of the sentence would be correct.

P8,L3: Does the partial melting of the hydrometeors at low levels during this warmest period help explain the spike in brightness temperature? Does the presence of liquid on the MASC hydrometeors correlate at all with this spike?

Fig. 6d: please use a retrieval (perhaps from Küchler et al. 2017) to obtain LW content and add it to 6d. The brightness temperature by itself is helpful, but does quantify the LWC.

P9,L1-3. As mentioned here, Fig. 8 indicates roughly the same fraction of graupel for this "vapour deposition" period as the fraction for Figs. 11 and 14. It also has the highest brightness temperature (and therefore LWC) of the entire study. It seems a bit odd to call this the "vapour deposition" period. I understand that the authors hypothesize that the riming happened below 2000 m, but that makes the claim of dominant vapor depositional growth during this period entirely dependent on the dual-pol hydrometeor classification scheme and inference from the sounding profile. It also undermines the main purpose of this study, as whatever mass was added by riming below 2000 m is quite important to the synoptic-microphysical connections that the authors are attempting to make in this paper. Where is this riming in the conceptual diagram (Fig. 16)? This is the most critical problem facing this manuscript.

Section 5.1. Would extending this period to 0500 UTC allow for a selection of MASC images without liquid water on them? The surface temperature cools a bit from 0400 to 0500. This would also make it the same 2-hour length (and sample size of radar scans) as the other two periods.

P10,L5-7: It seems a bit optimistic to call any of those layers below 3000 m at 0600 potentially unstable. If those wiggles are averaged for even 200 m, there is a net increase in θ_e. "Moist neutral" is probably more accurate. The flow has strengthened from period 5.1, and in Fig. 6b, from 789–2000 m, I instead see increased turbulence from the stronger flow potentially playing a role. The intense vertical motion in the 3000–6000 m layer in Fig. 6b most definitely cannot be attributed to anything but turbulence from the intense shear. I would focus on the shear-induced turbulence as the mechanism for the lifting here, with perhaps a brief mention that there could be a small contribution of buoyancy from the wiggles in the θ_e line.

P10,L24-P11,L4: This section does not add much to the paper. The updraught being discussed is already the most salient feature in Fig. 6b, and Figs. 12 and 13 do not provide much in the way of information that cannot be gleaned from Figs. 6a,b. I would suggest dramatically shortening this section.

P11,L2-4: Why does this suggest that convection is responsible? There is no substantial evidence to support that conclusion. There is no instability anywhere near this level in the 0600 sounding, and why could it not be a particularly strong KH billow?

P11,L3: It would be more convincing if the "intense riming" were obvious in the MASC image (Fig. 11). If anything, the riming in period 5.3 (Fig. 14) seems more intense to my eye, but it is difficult to assess with much certainty from the images. Was there any sort of disdrometer, especially a PARSIVEL, available at the site? If so, the density of the hydrometeors could be calculated from it. If the density were compared for particles of similar size, this would provide a more precise quantification of the degree of riming than the MASC.

P11,L13: "there are substantially more crystals and graupel particles than during other periods". This is true of the crystals, but the graupel concentrations are 9.1%, 8%, and 9.2% for the respective periods. I'd say that's about the same amount of graupel for each period.

P12,L11-12: The authors attribute the change from low Kdp in period 5.1 to high Kdp in 5.2 and 5.3 to the appearance of secondary ice. However, it seems there are two other possibilities. (1) as the authors mention, the number concentration of crystals is low in 5.1, and the greater concentrations of oblate targets/more intense precipitation in 5.2 and 5.3 are responsible for the increased Kdp. (2) The crystals in 5.1 are lightly rimed, and the onset of heavier riming in 5.2 and 5.3 is responsible for the increased Kdp. Please discuss this somewhere, and explain why either or both of those hypotheses can be ruled out.

Technical Corrections:

Multiple Locations: the phrase "associated to" would be more correctly written as "associated with".

Multiple Locations: is there a “Wernli” and a “Werni” citation, or is this a typo? Correct throughout the manuscript.

P5,L27: change “more and more” to “increasingly”

Fig 5. Can the authors plot wind barbs somewhere on here? It helps to visualize the shear layers.

Fig. 6. Please enlarge the figure slightly if possible. The details in 6c are the most difficult to see. Fig. 6c: please add a colorbar with labels for the hydrometeor information, instead of listing it in the caption – I don’t know what “cerise” is, and there appear to be more colors than you describe in the caption. Please also provide a citation for the hydrometeor classification algorithm that is used.

Figs. 7, 9, 15: Please add a 5th subplot of temperature. Shouldn’t the line corresponding to the lower limit of the WCB change for each period?

Fig. 15. Caption should instead say “Same as Fig. 7”, correct?