There are some interesting and useful components of this manuscript related to differences in ground-based and airborne wind retrieval methods. The writing and organization are clear, for the most part. However, there are several major concerns with the paper as listed below that will take a significant amount of time to address. I therefore recommend that the paper be declined and re-submitted once these issues are addressed.

Major:

(1) The total error in the radar comparisons is a summation of instrument effects (e.g., signal-to-noise ratio), sampling effects (e.g., gaps in data coverage, spatial/temporal resolution) and algorithm effects (e.g., geometry, approximations, solution method). The comparisons between dual Doppler and single Doppler retrievals for WV#0 tangential winds have some differences in the eyewall region (especially pass 4) that appear to be due to differences in the GVTD method (algorithm effect). The authors are trying to isolate the effects of the "steady-state" assumption (sampling effect) in airborne radar analysis by comparing WV#1 and WV#2 tangential winds from dual Doppler and single Doppler retrievals. However, it is not at all clear how much of the differences the authors are seeing are due to instrument effects, algorithm effects or other sampling effects. The WV#1 and WV#2 tangential winds should have larger errors due to algorithm effects when compared to the WV#0 tangential winds. This makes it very difficult or impossible to isolate the effects of just the steady-state assumption and thus the conclusions from this analysis are uncertain. The authors need to isolate these effects through some type of simulated analysis in order to make definitive conclusions.

(2) Regarding the improved GVTD method: the authors have done a thorough analysis of the impacts of storm motion on the ground-based retrieval. I think this analysis is useful for the community. However, I think the revised method only makes minor improvements on the errors in the retrieved mean tangential winds, but the authors have overstated their importance at several places in the paper, including the abstract. Thus, the tone of the paper needs to be revised in several places and more details are found in the additional comments section.

(3) Regarding the radar analysis with P3 data: the authors have used a grid spacing of 1 km in the horizontal direction. With the fore/aft scanning technique and antenna rotation rate of 10 RPM in 2016 data, there is really no way to arrive at 1 km horizontal grid spacing for the wind analysis. The spacing of radials is on the order of ~ 1.4 km so ~ 2 km horizontal grid spacing is about as good as it gets. The authors would need to redo their analysis with ~ 2 km grid spacing and scale the ground-based analysis accordingly. This could change some of the results. In addition, the storm core passed through on the far edge of the ground-based radar coverage when the P3 data was compared. The beam spread at this far range is

substantial and the grid spacing of the ground-based analysis of 1 km in the horizontal and 0.5 km in the vertical may not accurately reflect the pulse volume. Some discussion and/or analysis of this effect is also needed.

(4) Heavy use of NOAA TDR data is used in this study. This data is collected and processed (i.e., level 1 data) by NOAA and HRD. Unfortunately, the authors have not either (1)included a co-author from HRD or (2)made any mention of HRD in the acknowledgements section of the paper. NOAA/HRD has a data policy statement about these kinds of things that requires at the least, an acknowledgement for use of the TDR data. The authors need to rectify this in a re-submission of the paper.

<u>Additional:</u>
Lines 10 – 11; "A comparison between the two techniques shows that the axisymmetric tangential winds are generally comparable between the two techniques after the improvements to GVTD retrievals." The comparisons of WV#0 tangential winds are generally comparable **before** the GVTD improvements as well. The improvements don't change the values that much. Sentence needs re-wording.

Line 21; don't need measurements from two or more radars if the platform is in motion, such as airborne Doppler radar. Please clarify.

Line 25; There are some papers that have analyzed wind retrieval techniques for TCs and with varying platforms. Please cite some of those papers here.

Line 27; I highly doubt that this is the first study to compare ground-based and airborne wind retrievals. The NOAA ground-based and airborne radars have been around for decades!

Lines 56 – 57; sentence doesn't read right, "…only a small portion of TC…"? Please re-write.

General comment on the writing; at several places in the paper the word "the" needs to be inserted. Go through the paper again and look for these.
Some examples:
Line 96, "…and P3 TDR…" needs a 'the' before 'P3'
Line 98, "…of KAMX radar…" needs a 'the' before 'KAMX'

Line 122, Does this "mean" wind have the hurricane removed?

Equation (4), The two angles in the second terms on the RHS of (4) should be THETAt, not THETA. This is probably just a typo.

Line 150, what is the lowest elevation angle used here and what error does this incur? The method must only work where cos(phi) ~ 1, so lowest scan level only.

Lines 177 – 179, this assumption is probably only valid above the boundary layer and below the outflow layer. Radial wind asymmetries can be substantial in the boundary layer. Some discussion of this is needed.

Page 8, this entire page could be significantly shortened because the "dynamic" centers are ultimately used, not the GBVTD centers. This can be summarized briefly.

Discussion regarding Figure 4 on page 9: I would say the results are mixed on the improvement of the "optimal solution" over the "original solution". For example, in one pass the green dashed line looks better than the blue dashed line, but in another pass, it looks worse and in the other passes the differences are negligible. Similar things for the orange and red lines.

Also, on page 9: what are the heights of comparison between the TDR and the 88D? Since the storm core is on the far edge of the 88D coverage, the beam heights are probably fairly high, and the vertical velocities could be significant in this region. What is the impact of significant vertical velocity in the hurricane core on the TDR and 88D comparisons, given that the 88D retrievals don't take this into account?

Lines 265 – 268, These improvements are quite small, and I am wondering if they are statistically significant? I think the authors are overstating the impact of the improvements to the GVTD technique here and some rewording is needed.

Discussion around lines 295 – 296: these comparisons have significant differences between Ao and A1 coefficients in the eyewall region and it is not fair to say that they are "roughly consistent". Deviations of 2 m/s or less are a major error for Ao and A1 coefficients that have small values.

Lines 303 – 308, please see major comment (1).

Table 3, The differences between the original and improved GVTD method are only 0.35 m/s and this difference is likely not statistically significant. See major comment (2).

Table 4, I don't understand the wavenumber magnitudes listed here. Why is the magnitude of WV#0 so low? This should be azimuthal mean, correct? There is some confusion in the naming conventions listed in the table and the text that needs fixing.

Figure 6, should label these figures with the corresponding physical wind components because it is hard to follow.