amt-2020-240

Thank you for reviewing the manuscript and providing constructive comments. We have made edits to the manuscript incorporated with your suggestions. Reviewers' comments are shown in black, our response to each comment is shown in blue, and changes to the manuscript are shown in red.

Reviewer #1:
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.

Thank you for the comment. We agree we did not adequately discuss the total error in the radar comparisons in the manuscript. Additional discussion, previous literature reviews of the sources of error, and a new figure supporting our claim that the sampling error is a large contributor to the differences have been added to the manuscript. The instrument and algorithm errors are relatively small, but the sampling effect, particularly a long period of data collection for the airborne Doppler radar observation, could result in aliasing in the analysis with the steady-state assumption in a snapshot. To provide further support for this hypothesis, we performed an idealized experiment with a rotating wavenumber 2 tangential wind sampled by an aircraft with a realistic scanning strategy. We 'fly' a simulated aircraft track through an idealized vortex in a typical straight-line flight track during P3 operational reconnaissance. A wavenumber 2 Rankine edge wave propagating around 20 km RMW at 1/2 V_{max} is sampled by the simulated flight, which takes 22 minutes to finish a 160 km flight leg.

The new Figure 8 in the manuscript, reproduced here (Fig. 1), shows the derived tangential wind for a rotating wavenumber 2 during the flight pass. In this idealized experiment, we have minimized all the other errors such that a steady-state vortex is retrieved nearly

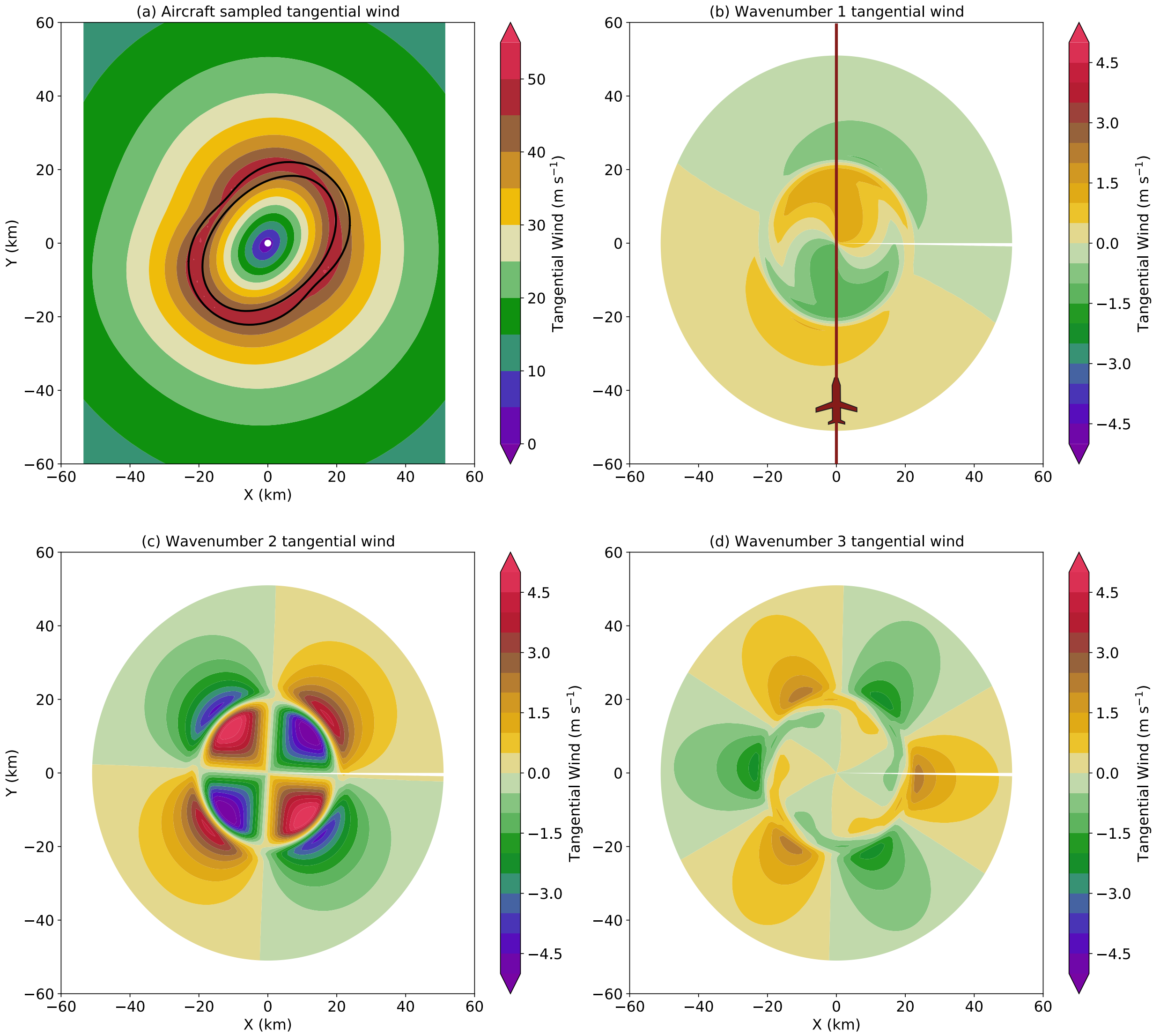

Figure 1: (a) Idealized dual-Doppler tangential wind speed retrieved from a straight flight pass through propagating wavenumber two asymmetry (color in m s^{-1}) and 50 m s^{-1} contour of time-averaged wavenumber two in black. The initial phase of the propagating wavenumber 2 tangential wind is oriented from east to west and final phase is from north to south. (b) Wavenumber 1 plus aircraft flight track from south to north, (c) Wavenumber 2, and (d) Wavenumber 3 tangential wind components retrieved by the Fourier decomposition of the tangential wind field shown in panel (a).

exactly. The distorted tangential wind field suggests temporal aliasing from the extended sampling period of the aircraft pass due to the propagation of the wavenumber 2 asymmetry. Although we only prescribed a propagating wavenumber 2 Rankine wave in the experiment, both wavenumber 1 and 3 components are with non-trivial amplitude. Based on comments from another reviewer, we have also clarified that there are in effect two different types of sampling errors that arise from the steady state assumption – the first is due to the time lag between fore and aft beams, and the second is due to the length of time used to composite the multi-Doppler into a single snapshot. While both produce some errors, the latter is more consequential when considering low-wavenumber asymmetries since it takes longer to capture the larger scale structure, resulting in evolution over the flight pass. This idealized experiment shows that the steady-state assumption of one straight flight leg to synthesize the data into one snapshot with the presence of rapidly evolving features could result in temporal aliasing and result in potential misinterpretation of the asymmetric flow field. Additional errors are possible from instrument and algorithm effects, and this has now been clarified in the revised manuscript.

We have included the discussion on different sources of error here for reference. Please see the revised manuscript for the full description of the new Figure 8.

Although dual-Doppler observations can be used to assess snapshots of high resolution kinematic and convective structure, airborne reconnaissance and research missions are rare events in most of the countries impacted by TCs. The three-dimensional airflow structure can also be retrieved from the dual-Doppler observations when the system is detected by two ground-based radars. General sources of error in the inter-comparison of ground-based and airborne dual-Doppler observations include instrument effects, algorithm effects, and sampling effects [Hildebrand and Mueller, 1985]. Instrument effects include the effects of attenuation and signal-to-noise ratio that could be caused by the radar processor design or measurement technique. These effects are likely to be most influential with marginal signal-to-noise, but random velocity errors up to 1 m s^{-1} are possible with many radar designs, including airborne radars [Hildebrand et al., 1994]. Algorithm effects include the effects of the interpolation to the Cartesian grid, multi-Doppler geometry, the solution method and its associated assumptions, and the derivation of the vertical velocities. Sampling effects include the effects of data spacing and density, geometry of flight tracks, temporal changes in the storm, advection, and data collection period. One of the long-lasting problems is the length of time required for each flight leg with airborne Doppler radar [Ray and Stephenson, 1990].The temporal effects can degrade the analysis if the data collection takes too long. [Jorgensen et al., 1983] quantitatively compared the wind fields of homogeneous precipitation derived from the two pseudo-orthogonal flight legs and two ground-based dual-Doppler observations. Their measurements showed agreement in the horizontal wind fields, but small discrepancies in the vertical velocities about 0.5 - 1 m s^{-1} for the airborne system and about 0.2 m s^{-1} for the ground-based system. The discrepancy was attributed to uncertainties in the pointing angle of the airborne system and a long data collection period.

(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.

Thank you for the comment, and we have revised the tone throughout the paper. As the reviewer has mentioned above, the error sources can come from instrument effects, sampling effects, and algorithm effects. While the improvement is small, as discussed in Jorgensen et al. 1983 , the accumulation of different sources of errors can contribute to a large amount, such that a minimization of all the known errors is needed. The errors from the contribution of storm motion could be large if the storm moves fast or when the storm tracks closer to the radar (R_T is small). The estimated storm motion of Hurricane Matthew (2016) is 8.4, 8.2, 3.8, 6.1 m s^{-1} on pass 1 to 4, respectively, and Matthew is about 200 km away from the radar. Therefore, the averaged RMSE of the four passes shown in the manuscript is not that large, but we think that the improvement on the errors from the improved GVTD technique are important and contribute to the reduction in total error. The accuracy of the estimated hurricane intensity is important when the storm moves inland or closer to the radar observation range. We have modified the tone of the description for the results, with two examples given here:

Abstract: A comparison between the two techniques shows that the axisymmetric tangential winds are generally comparable between the two techniques, and the improved GVTD technique improves the accuracy of the retrieval.

While it is a relatively small reduction in the RMS difference in the current case, the statistically significant difference in this algorithm error contributes to an overall reduction in the total error from instrument, algorithm, and sampling contributions.

Conclusion: A comparison between the two techniques shows that the retrieved axisymmetric (wavenumber 0) component of tangential winds are generally comparable between the two techniques, and the improved GVTD technique improves the accuracy of the retrieval.

(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.

Thank you for the comment. We realize we neglected to describe the fact that low-pass filtering was applied to the P-3 analysis to ensure that the resolved scales were in fact consistent with the data spacing, but this has now been corrected in the revised manuscript. As shown by Koch et al. 1983 and others, we believe the grid spacing should be smaller than the data spacing in order to accurately resolve the maximum spatial scales available for the given sampling. In SAMURAI, the ‘grid spacing’ is actually a ‘nodal-spacing’ since the resolved wind field is a function composed of finite elements. The nodal-spacing determines the minimum spatial scale resolved by the spline function of 2dx. In the current analyses, we use a 4dx Gaussian filter in the horizontal and 2dx filter in the vertical. Therefore, the resolved spatial scales are $\sim$4 km in the horizontal, which is well above both the along-track spacing, radar range volume, and azimuthal beam volume. While the vertical resolution may be a bit fine even after filtering, we focus on the horizontal structure in the paper and feel our analysis accurately resolves the appropriate spatial scales for the given sampling.

A low-pass filtering was applied to the P-3 analysis to ensure that the resolved scales were in fact consistent with the data spacing. The grid spacing should be smaller than the data spacing in order to accurately resolve the maximum spatial scales available for the given sampling [Koch et al., 1983].

(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.

Thank you for the comment and we apologize for the oversight. We have added the acknowledgement of NOAA/HRD, and are very grateful for the data collection efforts of NOAA Aircraft Operations Center and HRD.

We would like to thank NOAA Aircraft Operations Center and the Hurricane Research Division of the Atlantic Oceanographic and Meteorological Laboratory for collecting the airborne tail Doppler radar data used for this study, and the National Weather Service for the ground-based radar data.

Additional:

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 WV0 tangential winds are generally comparable before the GVTD improvements as well. The improvements don't change the values that much. Sentence needs re-wording.

Thank you for the comment. We have reworded the sentence.

A comparison between the two techniques shows that the axisymmetric tangential winds are generally comparable between the two techniques, and the improved GVTD technique improves the accuracy of the retrieval.

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

Thank you for the comment. We have clarified the sentence.

In addition to the presence of an airborne Doppler radar with fore/aft capability or multiple radars with sufficient range and geometry around the TC, a steady state assumption during the Doppler radar observation period is required to synthesize the wind fields into one snapshot in time.

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.

Thank you for the comment. Several references have been added.

Previous studies have shown the intercomparison of dual Doppler wind fields from two orthogonal flight legs and a ground-based two-radar network [Jorgensen et al., 1983, Hildebrand and Mueller, 1985]. Several other studies have investigated both single and multi-

Doppler techniques for retrieving TC wind fields [Lee et al., 1994, Crum et al., 1998, Reasor et al., 2000, Lee et al., 1999, Jou et al., 2008, Bell et al., 2012], but the strengths and weaknesses of different techniques have not been compared and addressed fully. In this study, ground-based single Doppler and airborne dual Doppler observations simultaneously sampling Hurricane Matthew (2016) are analyzed to provide the first comprehensive comparison between ground-based single and airborne multi-Doppler wind retrieval techniques in a TC.

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!

Thank you for the comment. We have added several references on these types of comparisons, and clarified that we mean specifically that the comparison between single-Doppler wind retrievals and airborne dual-Doppler has not been conducted. The comparison between ground-based dual Doppler and airborne wind retrievals was done in 1980s to 1990s, but the wind field retrieved by a single Doppler radar observation has not been compared with the airborne dual Doppler observations to our knowledge. Please refer to the previous response for the revised sentences and additional references.

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

Thank you for the comment. We have revised the sentence.

Ground-based dual-Doppler radar observations of TCs are usually limited to the observation of storms that happen to develop or move within the domain covered by the radars and extensive radar baselines [Jou et al., 1996].

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"

Thank you for the comment. We have gone through the paper and added the missing word.

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

Thank you for the comment. The mean wind magnitude and direction were derived from the airborne dual Doppler analysis, following the procedure proposed by [Marks et al., 1992]. The storm-relative horizontal wind field (V_r)in a cylindrical coordinate system centered on the storm can be decomposed into:

$$V_r(r, \theta, z) = \bar{V_r}(z) + V'(r, \theta, z) \tag{1}$$

where r is radius, θ is azimuth, z is height, $\bar{V_r}(z)$ is the horizontally averaged wind vector over the radius and azimuth, and $V'(r, \theta, z)$ is the deviation from $\bar{V_r}(z)$. $\bar{V_r}(z)$ can be expressed as:

$$\bar{V_r}(z) = \frac{1}{2\pi} \int_0^{2\pi} \int_0^{r_{max}} V_r(r, \theta, z) dr d\theta \tag{2}$$

If the horizontal wind field is from a circular symmetric vortex with no steering flow, $\bar{V}_r(z)$ would be zero. Nevertheless, if the vortex is embedded in the steering flow, the averaged horizontal wind field would equal the mean wind component. Thus, the local wind shear can be approximated by subtraction of the mean wind component at different altitudes. In our study, we calculated the mean wind component averaged from the vortex inner core area within the radius of 60 km.

The mean wind (V_M) is the horizontal average of the environmental flow at each altitude following the procedure proposed by [Marks et al., 1992], which can be used to calculate the vertical wind shear.

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.

Thank you for the comment. We have corrected the typoes.

$$
\begin{aligned}
Dcos\theta_d &= Rcos\theta + R_T cos\theta_T \\
Dsin\theta_d &= Rsin\theta + R_T sin\theta_T
\end{aligned}
\tag{3}
$$

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

Thank you for the comment. Figure 2 shows the radar beam elevations of 0.5° and 0.9° versus the range. The distance between the KAMX radar and the TC center of the four radar volumes corresponding to the four flight passes are highlighted by the vertical lines. Our analysis focuses on the altitude of 4 km, and the four radar volumes at this altitude is constructed by the radar beam below 1° elevation angle. Therefore, the assumption of Vd $\sim$ Vd/cos(ϕ) is applicable.

Plugging Eq. 4 into Eq. 3 and approximating $\hat{V_d}/cos\phi$ with V_d (only valid when the elevation angle is low):

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.

Thank you for the comment. We have added the conditions to the assumption.

This closure assumption may not be applicable within the boundary layer or outflow layer where the radial wind asymmetries can be substantial.

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.

Thank you for the comment. We would like to present the GVTD simplex algorithm which outperforms the GBVTD-simplex algorithm as a reference for future work. The spline fit of

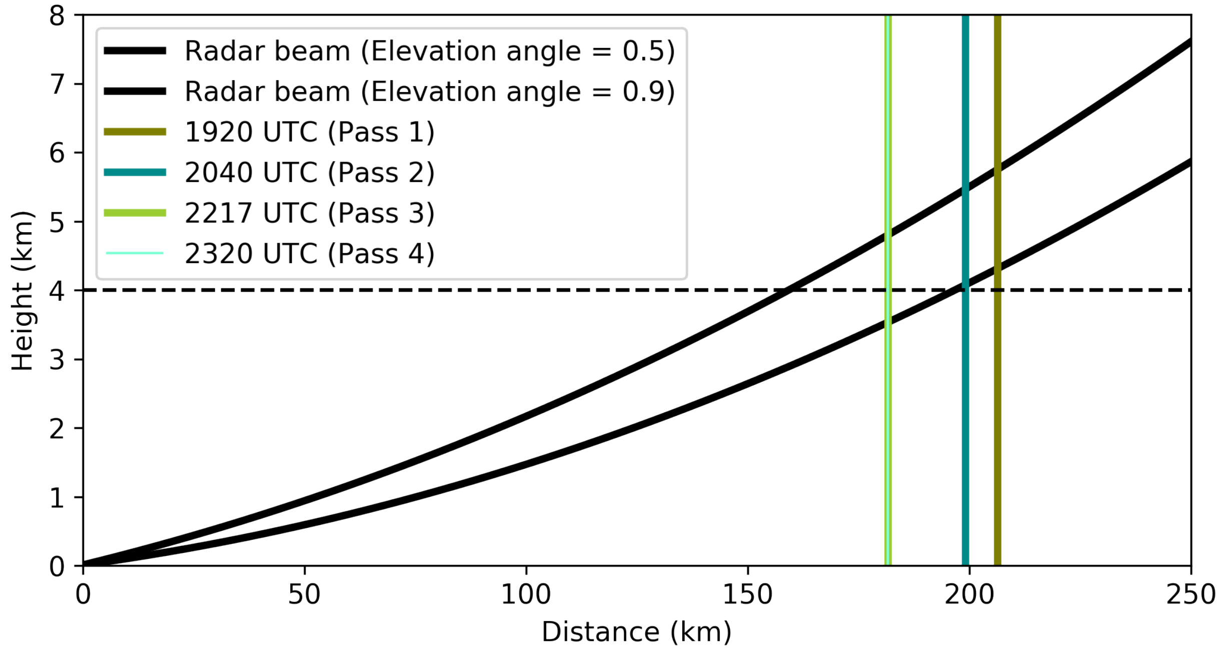

Figure 2: The radar beam height of 0.5° and 0.9° elevation angles (black solid line). The distance between the KAMX radar and the TC center of the four radar volumes corresponding to the four flight passes are highlighted by the colored vertical lines. The dashed black line represents the altitude of 4 km.

the dynamic centers is sometimes quite variable, so we would want to provide the GVTD-simplex centers as another option to derive the TC centers with high temporal resolution in this manuscript.

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.

Thank you for the comment. Since the four solutions are quite similar with only a few m/s apart, we have calculated the RMS difference results quantitatively in Table 3 to quantitatively demonstrate that the "optimal solution" outperforms other methods, and the RMS difference of the improved GVTD solution (red) is smaller than the original GVTD solution (orange). However, similar to the response to the major comment above, we have changed the tone in the manuscript to not overly state the results.

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.

Thank you for the comment. The altitude of the analyses are added in the manuscript.

The subsequent analyses use the observations at the altitude of 4 km, so the ground-based 0.5° radar elevation beam can detect the TC inner core.

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?

Eq. (2) in Lee et al. 1999 shows the vertical velocity and terminal velocity term ((w-v_t)sin(ϕ)) in the construction of the Doppler velocity. They removed the contribution from w and v_t, and divided cos(ϕ) to get Eq. (3). Therefore, the vertical velocity and terminal velocity contribution becomes (w-v_t)sin(ϕ)/cos(ϕ).
sin(ϕ)/cos(ϕ) is approximate to 0 if we use the data mainly derived from the lowest scan (ϕ=0.5, sin(ϕ)/cos($\phi = 8.72 \times 10^{-3}$, Fig. 2). Therefore, the vertical velocity and terminal velocity contribution can be neglected in this case. We have added the discussion to the manuscript:

Note that $\hat{V_d}$ neglects the contribution from the terminal velocity (v_t) and vertical velocity (w) (Eq. 2 in [Lee et al., 1999]). The contribution from w and v_t is small if the elevation angle of the radar beam is low (<1°).

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.

Thank you for the comment. A nonparametric Wilcoxon signed-rank test has now been conducted to test the null hypothesis that two paired sets of the RMS differences derived from the original and improved GVTD algorithms are drawn from the same distribution. Results show that the RMS difference between the original and improved GVTD algorithm is statistically significant in both single and dual-Doppler analyses with 99% confidence.

A nonparametric Wilcoxon signed-rank test is conducted to test the null hypothesis that two paired sets of the RMS differences derived from the original and improved GVTD algorithms are drawn from the same distribution. The RMS difference between the original and improved GVTD algorithms is statistically significant with a p value < 0.001 using both the projected dual-Doppler winds and single-Doppler velocities, indicating that we can reject the null hypothesis at the 1% significance level (99% confidence). The statistics suggest that the RMS differences distribution of wavenumber 0 tangential wind retrieved from the original GVTD algorithm are likely to be larger than those from the improved GVTD method.

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.

Thank you for the comment. We have revised the sentence.

The deviations of A_0, A_1 and B_1 coefficients between the two analyses within the eyewall region (15 - 25 km) are less than 2 m s^{-1}.

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

Thank you for the comment. Please refer to the response to 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).

Thank you for the comment. Please refer to the response to major comment (2) and comment "Lines 265 – 268", where we have changed the tone of the manuscript and conducted a statistical significane test.

Table 4, I don't understand the wavenumber magnitudes listed here. Why is the magnitude of WV0 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.

Thank you for the comment. We have corrected the table caption to be more specific.

$V_d D/R_T$ harmonics coefficients amplitude (harmonics 0 to 3) retrieved from the single Doppler and dual Doppler analyses.

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

Thank you for the comment. The physical wind components are added.

(a) A_0 (to obtain $V_R C_0$ and $V_M cos(\theta_T - \theta_M)$) (b) A_1 (to obtain $V_R C_0$) (c) B_1 (to obtain $V_T C_0$) (d) A_2 (to obtain $V_R C_0$ and $V_T S_1$) (e) B_2 (to obtain $V_T C_1$) (f) A_3 (to obtain $V_R C_0$ and $V_T S_2$) (g) B_3 (to obtain $V_T C_0$ and $V_T C_2$)

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