We thank this reviewer for their inciteful and valuable comments. We have modified the manuscript for the most part in accordance with their suggestions. Their comments have resulted in improvements to our paper.

Our responses (in red) along with reference to the line numbers of any associated substantial changes to the paper follow.

Reviewer #2 Comments

"Wind comparisons between meteor radar and Doppler shifts in airglow emissions using field widened Michelson interferometers" by Kristoffersen et al. deals with a comparison between two completely different wind measurement techniques in the mesosphere and lower thermosphere region. Their approach is more advanced than other existing comparison studies between meteor and optical methods by introducing the effect of airglow brightness weighting, which I have long been interested in but have had no opportunity to test myself. I enjoyed very much reading the manuscript and do not have a major concern about what have been done in the present study.

However, the obtained results are still not conclusive and there could be more to be done. I would like the authors to even step forward and to think about the following approach additionally, which will simplify the comparison setting and provide further insight into the difference of the two techniques with less restriction. A major obstacle that makes the comparison difficult is the difference in temporal and spatial resolution/averaging between the two techniques. Although the authors use 90 min averaged and some hundred-km horizontally averaged winds in this study, I would directly use the 5 min ERWIN winds and instantaneous meteor echo radial velocity without conducting such temporal and spatial averaging in order to avoid/minimize unwanted effects caused by small scale gravity waves described in the discussion and conclusion.

The number of meteors detected within the FOV of the ERWIN may be small, but by using meteors within an area such as 10 degree diameter centered around the ERWIN FOV the number will be enough to make a direct comparison with only a minimal spatial averaging because you have two month long data set. Some minor correction of radial wind values for meteor echoes will still be necessary considering the elevation and azimuthal angles of meteor echoes. Further, a frequency spectral analysis of ERWIN radial velocities for individual four FOVs will also be what should be tried to see how the spectral values behave, especially in the high frequency range. Because of their large off-vertical angles most contribution to the radial winds are from horizontal winds and the effect of vertical winds to the spectra will be negligible.

I strongly recommend that the authors try such approach and make the present study even more attractive to potential readers.

We appreciate the suggestion. This is a good idea.

The suggestion of comparing individual meteor Doppler data with single (one FOV) ERWIN measurements has been tried but was not included in the paper. It shows statistically similar results (e.g. speed bias) to the current fit comparisons. But there are caveats to be considered:

Since the emission layer heights are nominal rather than measured, the effective sample range (and height) for individual N, E, S, and W ERWIN values is unclear, so even if there were a surplus of concurrent meteor echoes in or near an ERWIN field of view, it would not be possible to choose the "appropriate one".

In addition, the meteor echo is a point measurement, whereas the optical Doppler is a sort of average over a long slant path through an unknown emission layer, which may also include significant height variations in tidal winds.

We felt that these results did not augment the results reported on in this paper. We may try to identify a time period with enhanced meteor echoes and attempt, a study as suggested, but this would be the topic of a new paper.

We have noted the possibility of undertaking a study such as that suggested in a new paragraph in the conclusions.

“Unfortunately, the results of this study do not fully resolve the reason for the difference between winds measured by the two instruments although it does specify the nature of the difference more precisely than previously possible. Future studies can be directed toward attempting direct correlation of individual meteors collocated with the ERWIN observations for periods with enhanced meteors. The airglow model can be refined further by including more realistic airglow profiles and exploring the scale dependence of wind deviations (including the form of the background profile and gravity wave characteristics) due to gravity wave modulation of the airglow brightness. An effect not considered in this paper, is the possibility that scattering might affect optical wind measurements as suggested by Harding 2017. Since indications of the presence of cloud are available at PEARL, a correlative study examining the wind deviations as a function of cloud can be undertaken in the future.”

MINOR COMMENTS

Abstract

line 3

ERWIN is not defined.

A definition of the ERWIN acronym was added to the abstract.

line 10

0.3/s > 0.3 m/s

Corrected

lines 85-86

"They consist of ..... cross section"

The meaning is not clear. Grammatical correct?

We have replaced this somewhat condensed sentence with the following to clarify the observation process.

“They are determined from Doppler shifts in airglow emissions integrated along each line-of-sight. The contribution from each point in the field of view is weighted by the airglow volume emission rate at that point.”

line 138

"Kristoffersen et a. (2021)" should be

[Kristoffersen et al., 2013]

Citation changed to correct citation.

line 143

one "]" is missing.

Added “]”

line 147

one "in" is redundant.

Removed second “in”.

lines 166-167

"[ ]" is missing.

Added parentheses around citations.

Figure 2

Information on direction is missing.

Added the following to the caption to clarify the wind directions shown in the plot: “such that northward is the positive y-direction, and eastward is the positive x-direction”.

Figure 8

Red and magenta are confusing.

White can be a better choice for one of them.

Thank you for the suggestion, the figure was updated to help make the symbols clearer. The colour map was changed to a grey scale. The blue x’s were changed to blue circles, the red +’s were changed the red triangles, and the magenta +’s were changed to green triangles.

lines 342-343

"2-D fits to many meteors to set the zero"

The meaning is not clear.

The line-of-sight Doppler shift for the meteor radar is determined directly from the time shift between the returning signals and the transmitted signal. Zonal and meridional winds are then determined in a least mean squares sense from the echoes in each time/height bin (for details see Hocking et al., 2001). We have changed the text to:

"MRW winds are determined from Doppler shifts and echo directions by least squares fits to horizontal components of radial velocities (see Hocking et al., 2001 for details)"

Figure 12

It is hard to tell apart the lines in the left panels.

We agree but are not sure what to do about this. We will think about how to resolve this question before the next submission of the paper.

Equation 2

delta_z is not defined.

We have added the definition in the text, "(vertical thickness, Δz) " (line 425).

delta_z/lambda_z in front of sin is reversed if my calculation is correct.

Yes, thanks for catching this. We have corrected this expression in the text.

Equation 4

dz is missing in the numerator.

One ")" is missing in the denominator.

Thank you for catching these typos, the dz was added to the numerator and the ")" was added to the denominator.

lines 447-448

A conjunction is probably missing.

To clarify this sentence, we have added "since" to link the clauses.

"The background profile is unlikely to have significant small-scale variations since the vertical gradient is small …"

line 456

I believe that "sigma approaches zero" should be "sigma approaches infinity" or "lambda_z approaches zero". If not, the description in this line is not immediately evident to me.

Thanks for catching this. You are correct. We have changed this to “sigma approaches infinity”.

Figure 13

What alpha and delta_u are used for the evaluation?

In the figure, we are showing normalized amplitudes, i.e. $\delta u = 1$. This is stated in the figure and text. We have noted that the value of α used in this figure is 0.2 in the text and in the figure. Thanks for pointing out that this was missing.

The letters "a,b,c,d,e and f" are too small and hard to recognize.

The letters have been changed to a larger font to make them visible.

Figure caption: "pi" should be in Greek.

Two instances of “pi” changed to “π”

line 482

Lots of expressions such as "small vertical scale gravity wave horizontal variability" are seen in the manuscript. They are not so easy for a non-native English speaker like me to follow, being vague in what is the subject/object or adjective. Expressions with some moderate use of prepositions would be preferable.

We have started going through the paper to identify and modify these. We will continue to do this and hopefully the copy editor will also help with this.

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