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Interactive comment on "Study of second-order wind statistics in the mesosphere and lower thermosphere region from multistatic specular meteor radar observations during the SIMONe 2018 campaign" *by* Harikrishnan Charuvil Asokan et al.

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Although the etiquette suggests the authors to express the gratitude towards any reviewer comment, unfortunately, I am not able to see any constructive criticism from this review; hence I am refraining myself from expressing my thanks to the reviewer. However, I choose to respond to the important comments made by the reviewer for the benefit of future readers of the paper.

(a) One of the first argument by the reviewer is the statement about the current manuscript being the 'clone' of Vierinen et al. [2019]. I have to disagree on this point. As I respond to the RC1, Vierinen et al. [2019] described and introduced the WCFI method on their paper as a novel method for estimating the mesospheric wind correlation from multistatic specular meteor radar observations. They have utilized one day of data to demonstrate the capabilities of the method. However, in our current manuscript, we did not reintroduce the method, but we used it to study a specific application, namely the large-scale wave dominance during our campaign period. Here we conducted a spectrum analysis from second-order statistics independent of the functional form of the wind. Besides, in the manuscript, we have used gravity wave simulation efforts to validate both methods and understand the observed scales. Therefore, our work is not a clone of Vierinen et al. [2019], instead is a study of non-expected geophysics that were dominant during the seven days reported. We choose to include some of the mathematical equations of WCFI in the current manuscript for the straightforward reading of the paper without going back to Vierinen et al. [2019], for the mathematical parts alone. Nevertheless, this did not refrain us from acknowledging the work by Vierinen et al. [2019], and we have referenced this paper properly in every aspect. Also, in the current manuscript, we did not present any result about momentum flux or momentum flux spectra. The reviewer has mentioned it multiple times, and I do not know how to respond to it.

(b) Vertical winds: I share a similar concern regarding the unexpected values of mean vertical wind plotted in Figure 2. Other colleagues and I are still pursuing to understand these estimates on this subject and working on a manuscript specifically addressing this very issue by combining SIMONe geometry on the ICON-Upper Atmosphere (ICON-UA) model. Our intention is to share what we have obtained with the reader since other groups prefer not to fit for the vertical velocity or just ignore them. At this point, if needed, we can remove the vertical velocity estimates from the current paper or indicate that those estimates represent "apparent" values and their understanding require further studies. After all, the vertical winds are not the main thrust of the paper. However, the vertical velocity estimates do not taint any other estimated wind parameters from our analysis. To validate this point, I estimated the mean wind velocities by the least-square fitting of radial winds to only u and v (not fitting for vertical velocity) at a given altitude gate during a time bin, assuming the horizontal wind is homogeneous inside the observed volume. Also, I did the same on our gradient approach (also not fitting for vertical velocity), and the reviewer can observe that zonal and meridional components are not affected by the vertical velocity estimates (see Figure 1 here). So the argument raised by the reviewer about the validity of the horizontal winds is not valid any more. Hence, it does not evaporates the possibility to obtain meaningful observations in a geophysical sense.

(c) CW meteor radar observations: I do not expect that our CW transmission to impact the meteor trail scattering as to change dynamics we obtain from them. Although I have not made the calculations requested by the reviewer, which by the way are not trivial, based on the following arguments, I do not think they are relevant: (a) Our continuous transmitted power at 32.55 MHz is 2000 W and using five single Yagi antennas on transmission the maximum effective radiated power (ERP) is less than 4kW in comparison the Tromso Heater's maximum ERP is 300 MW (Rietveld et al., 1993), i.e., more than five orders of magnitude larger and at frequencies at least five times smaller!; (b) AM and FM radios, as well as TV transmitters, present a larger ERP and if there are any influences, they would be "heating" the meteor trails all the time, (c) existing pulsed specular meteor radar systems present similar ERPs, e.g., SAAMER in Argentina, ALO Multistatic in Chile. In addition, there have been studies previously involving CW as well as pulsed links that show that the obtained horizontal winds are consistent (e.g., Vierinen et al., 2016, Stober et al., 2018, Chau et al., 2019).

(d) Mean wind estimation: We choose this term since Vierinen et al. [2019] used it. We wanted to follow the same nomenclature not to create confusion. It is not our intention not to acknowledge VVP and VAD methods. The gradient method is explained in Chau et al. [2017], and I believe that this paper has already cleared the differences

between these methods. The so-called Volume Velocity Processing method mentioned by the reviewer is a VVP method using a first-order Taylor expansion approximation to a monostatic geometry and not able to determine all gradient terms independently. Therefore it is not THE VVP. Other VVP methods applied to multitstatic configurations can be found in Stober et al. (2018), Chau et al., (2020). The VAD method is applied to a pre-defined radar beam pointing direction; in our case, the pointing direction is given by the meteor occurrence, without any particular organization. Nevertheless, I will add the references corresponds to VVP and VAD in our modified version of the manuscript.

(e) Temporal and vertical resolution: we choose our resolution based on the number of meteor counts that we were able to get from the Campaign. We have more than a hundred thousand meteors per day in our Campaign, and it is sufficient enough to reach the resolution that we used in the paper. However, our estimates represent averages over the horizontal coverage, i.e., up to 500 km diameter approximately.

(f) 4h-4km winds: we choose this simple filtering to reduce the effects of tides and other large-scale waves with periods higher than 4 hours. We wanted to see the signatures of 3-4 hour gravity waves. It was the reason for the selected parameters. Moreover, this filtering has been used in our horizontal correlation analysis (see section 4.4 and discussion).

(g) Windfield Correlation Function Inversion: There have been allegations from the reviewer's point of view regarding the validity of several peer-reviewed papers, including Vierinen et al. [2019]. Those statements by the reviewer seem like an unnecessary challenge towards the peer-reviewing system of other journals, and I do not encourage this discussion here. The next few paragraphs address to reviewers' disagreement towards the WCFI method, and I do not see any merit in discussing as the reviewer choose to disbelieve the method itself by discrediting Vierinen et al. [2019].

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Attached Figure1 description:

The figures (a) and (d) shows the same zonal (u) and meridional (v) mean winds obtained from the gradient approach, as showed in Figure 2 of the manuscript. Figures (b) and (e) corresponds to u and v obtained by least-square fitting the radial winds to only u and v (not fitting for vertical velocity). Figures (c) and (f) shows u and v obtained using gradient approach but not fitting for vertical velocity.

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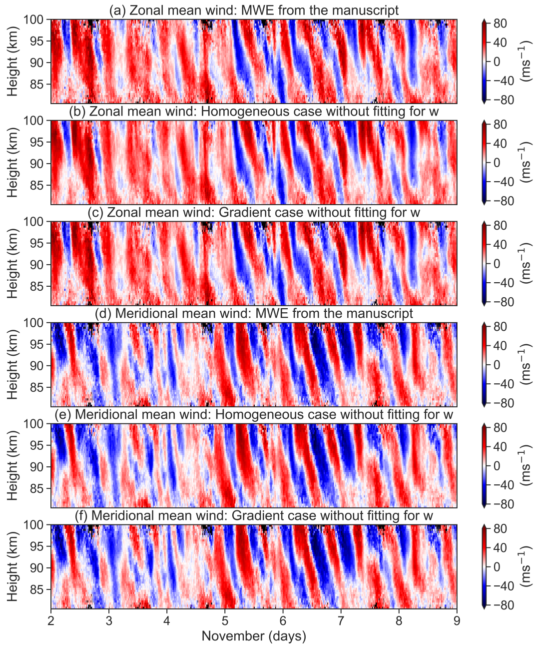

Fig. 1. Mean wind estimation