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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #2

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Review 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, Jorge L. Chau, Raffaele Marino, Juha Vierinen, Fabio Vargas, Juan Miguel Urco, Matthias Clahsen, and Christoph Jacobi

General Comment:

The manuscript presents multistatic observations in Northern Germany. The measurements were obtained during a 7-day campaign in 2018 combining monostatic and forward scatter meteor radars. The submitted paper is in parts a clone of Vierinen et al., 2019. The authors tried to analyze the data concerning mean winds, momentum fluxes and wind variances, which are termed in the manuscript second order statistics. They claim to have developed a generalization of the more elaborate and known Hocking (2005) method. Mean winds are obtained applying the volume velocity processing (Waldteufel and Corbin, 1979) and a recently published wind field correlation inversion. The manuscript is written in a rather popular English presenting very simple and known physics in detail, but fails to provide detailed explanations about the assumptions associated to the applied methods. Furthermore, the authors introduce many new acronyms and names to well-known equations and methods instead of an appropriate referencing to more established nomenclatures. The reviewer got the impression that this is supposed to pretend progress, but in fact, it triggered a more careful analysis of the manuscript.

The manuscript lacks a detailed physical presentation and fundamental explanation as well as justification of the applied methods. The reviewer had sometimes the impression to read a computer generated manuscript as some paragraphs and statements seem to have no connection to the context of the manuscript.

After a careful assessment of the manuscript, the reviewer suggest a rejection and no re-submission is encouraged as there are major flaws in all aspects of the presented work including the experiment setup, data analysis and physical interpretation. The manuscript lacks in large parts a clear physical understanding of the observations revealing severe deficiencies on the relevant atmospheric physics. The manuscript is full of mathematical statements, which appear to be sometimes correct, but not the context of the manuscript. Furthermore, there are substantial contradictions to other manuscripts submitted by some of the authors e.g. Conte et al., 2020, Chau et al., 2020 and Urco et al., 2019.

In the following only some of the major issues and flaws are going to be briefly summarized to justify the reviewer suggestion of a rejection.

Major comments:

Vertical winds:

The authors present vertical winds obtained as a mean over the domain area of approximately 500x500 km. These winds are almost two orders of magnitude off what can be found in the literature from other observations and models and are not justified by the current understanding of atmospheric dynamics. These winds are violating more or less the 1st and 2nd law of thermodynamics. The reviewer sees no chance to publish these winds without a detailed review of the physical processes and the current knowledge. Where does the energy come from? Please quantify the energy that is required to obtain such an upwelling. Are there additional momentum sources? The authors need to quantify the required heat and cooling rates, which are in the order of several hundred Kelvin per day. Due to the large domain area, there should be clear signatures of a heat source and cooling region in SABER or MLS or local lidar data visible. Some of the authors actually already used these measurements in Vargas et al., 2020 to analyze the same period as in this manuscript leveraging the wind analysis presented here in. However, looking at the reviews that Vargas et al., 2020 received, there are first of all issues with the data agreement to use SABER, but moreover the temperatures are in line with the current understanding of atmospheric dynamics and provide no evidence for such a heat/cooling region and, thus, the authors are contradicting themselves.

Two orders of magnitude deviation from what can be found in the literature requires

a much more careful analysis. As these winds are present or inherent in all SIMONE observations (e.g. Conte et al., 2020, Chau et al., 2020), it appears that there is a major flaw related to the experimental setup or concept. The vertical wind bias is actually large enough to substantially bias the horizontal winds as well and, thus, evaporates the possibility to obtain meaningful observations in a geophysical sense. The manuscript even uses these winds to obtain spectra and the classical u'w' and v'w' momentum fluxes. Any systematic bias in the vertical winds more or less directly contaminates these results as well, which also brings the results of Conte et al., 2020 into question. However, in that respect Vargas et al., 2020 is also affected and should be tied to the faith of this manuscript.

This deviation alone justifies the rejection of the manuscript, as there was not even an attempt by the authors to discuss these observations in the context of previous results invoking atmospheric physics and the current understanding of the residual circulation. The paper, as it is now, discredit the concept of multistatic transverse scatter meteor radar observations. Furthermore, these results eliminates any confidence in the data analysis of the observations presented here in.

Cw-meteor radar observations:

Can the vertical winds be explained in the context of the scattering? The vertical wind magnitude seems to increase with increasing altitude. A cw-experiment is a heating experiment and can bias or actively modify the plasma. Please quantify the degree of magnetization of the electrons and the corresponding accelerations in the plasma trail and whether they are still coupled to the ions? Similar effects are reported on PMSE with the Tromsoe heating facility. However, they likely transmit less power and have a smaller antenna gain, but on the other side the plasma density in the meteor trail is much higher and thus shows a different collisional coupling.

Referencing:

The referencing is not appropriate and does not comply with the ACP ethic rules. The reviewer forwarded a list of more than 39 papers to the editor that could have been cited (just covering 4 aspects of the manuscript). The reviewer did not expect to find them all, but just by chance or from a simple google search one could have found at least some of these references. Instead, there is a sole preference to self-citations. It appears that the authors did not make an attempt to cite recent or previous publications related to atmospheric dynamics, remote sensing other groups using cw-forward scatter meteor radars, general circulation or meteor radar observations dealing with the topics mentioned in the manuscript.

Mean Wind Estimation:

The authors renamed the known Volume Velocity Processing (VVP) presented in Waldteufel and Corbin, 1979 into Mean Wind Estimation. The technique goes back to Browning and Wexler (1968) and is also known as velocity azimuth display (VAD). This original work was the first publication that introduced spatial gradients. However, the VVP approach is more general and practical in the implementation. The apriori unknown non-linear wind field is expanded into a Taylor series and truncated after the first order. This approximation holds only for small areas around the development point (x-x0). The reviewer doubts that this is applicable to the large domain areas as used in this study due to the polynomial growth towards the domain boundaries a physical meaningful wind field is difficult to be approximated just using the first order. Please comment on physical meaning of the gradient terms and why they have to be estimated on scales of 15 minutes? Are the assumptions still valid?

Temporal and vertical resolution:

The authors claim to observe mean winds with 15 min resolution? Do they expect GW with periods of 30 min and spatial scales of the approximately 4-7 times the domain area? The vertical resolution is 500m. Just considering the measurement errors intrinsic to meteor radars, it is hard to believe that. Considering that the specular point actually refers to the scattering from several Fresnel zones, which are several kilome-

ters in length, this vertical resolution appears to be not physical not realistic. Please justify.

4h-4km winds

Why are the winds filtered with this resolution, which is in the center of the GW spectrum, but mixes essentially or even removes any inertia gravity waves by the vertical smoothing, which are the scales they are interested. Furthermore, this filtering seems to be not used later in the manuscript neither it is discussed.

Wind Field Correlation Function Inversion

This method was introduced in Vierinen et al., 2019. However, also this paper got accepted, the reviewer is not so convinced that the method is actually correct. The authors claim that the method is a generalization of the Hocking (2005) method. However, Hocking (2005) presented already a general solution and is applicable to all multistatic and monostatic systems and solves the equations along the principle axis, which allows to derive mean momentum fluxes and wind variances.

It is obvious that the matrix shown in eq. 5 is identical to the matrix obtained in Hocking (2005) for i=j and this also means that equations 2 and 3 are the standard radial wind equation as shown in Hocking (2005). The method proposed in Hocking (2005) can be theoretically derived from the Reynolds averaged Navier Stokes equation and is based on a Reynolds decomposition, whereas Vierinen et al., 2019 just correlates the sensor response function (eq. 2 and 3) and ignores the spatio-temporal properties of propagating waves as these quantifies are mixed together.

The problem is that equation 2 or 3 do not hold the linear polarization relation for gravity waves for i neq j, viz. the matrix equation 5 does not hold the polarization relation of gravity waves, which is a kind of a problem as the goal of the manuscript is to derive gravity wave momentum fluxes and wind variances due to GW. In fact, the method, as described in the manuscript, is not applicable to a 3D atmosphere like on our Earth at the mesosphere/lower thermosphere. The relation only holds for a 1 dimensional atmosphere or a 2 dimensional isotropic atmosphere (u'=v' and k=l).

In principle, the method, as outlined in the manuscript, is physically only correct for the small subset of observations containing meteors that are simultaneously detected in three different links at same time and location. Considering that the authors use a specular scattering from meteor trails, this small subset is essential zero as it would require to have 3N forward scatter meteor links for N meteor detections or with other words the antennas have to be set up at the right location ahead of time to observe the meteors simultaneously.

Ignoring this aspect, as done in the manuscript, lead to an entire mixing of the geophysical information with the spatial sampling and the sporadic meteor background source dependent flux and geophysical non-sense. This is also confirmed in figure 4. The spectra shown there deviate by almost an order of magnitude from the reference slope omegaˆ2. This expected for the smaller scale waves as the u',v',w' distortions more less lead to random white noise. Only scales larger than the volume are still correlation, which explains the that slope for these scales apparently agrees a bit better to the reference. However, the reviewer has no doubt that mean winds can be fitted from all meteor detections, but this is also achievable with any other standard meteor radar and, hence, not even worth to be published for a 7-days campaign.

The authors also seemed to have noted that there are issues as they claim that they observed so unexpected results (see discussion and conclusion). Could it be that the results were so unexpected, because the applied methods are physically not valid?

This flaw in the concept of the experiment and data analysis is hard to be repaired given the raised issues.

Furthermore, considering that the terms G_uu, G_vv, G_ww and G_uw, G_vw and GW_uv correspond to the wind variances uˆ2,vˆ2 and wˆ2 and u'w', v'w', and u'v', as mentioned in Hocking (2005), shows that one needs to obtain the structure functions for

each quantity u',v' and w' at each location and time. Just using the radial measurement is physically not consistent and explains the wired spectral shapes shown in figure 7 as well, which basically show white noise for time scales below 7 hours and a tailing off of the spectral slopes towards the small scales. Only the tides appear to be somehow more correctly approximated as the semidiurnal tide satisfies the condition of (u'=v' and k=l) at these latitudes of these observations. However, this is surely not helpful.

Increasing the number of observation even more is also not adding any value to solve the mathematical problem as the measurement response per link is not going to increase, e.g. one million observations towards the East do not help to constrain the wind blowing from the North.

Other selected major comments:

Page3, Line 60 – 70

This paragraph about DNS simulations and results seems to belong to a different manuscript. Neither any DNS results are used nor DNS simulations are shown later on in the submitted manuscript. They actually even mention that there is not really a chance to connect the observations with DNS simulations. There is only a loose statement that the MLT is a laboratory for DNS models. However, the questions is why should one spend time on a supercomputer to investigate turbulence at the MLT, if the results are not going to be used and compared to the observations.

Page 8, line 169-ff

The statement is mathematically correct, but does apply to Reynolds averaging and the Reynolds stress terms as these are based on Gaussian random noise zero mean statistics, which is whole idea of a Reynolds decomposition. Following Kaya (2006), it is shown that for Gaussian zero mean random processes even the square can be treated as Gaussian in a WSS sense. Furthermore, the group at IAP Kborn seems to had no problem to claim in Conte et al., 2020 that for i=j the least square can be applied and that this is not a big issue. What should the reviewer believe? Please provide a mathematical prove of the statement.

Page 4, line 93-95

This passage contradicts again Conte et al., 2020. There the authors claim that 28 days are required to obtain momentum fluxes, but why they then conducted a campaign of 7 –days, which is too short according to Conte et al., 2020.

Page 20, line 406-ff

The authors should please provide an explanation about secondary waves or better what are primary waves and how they investigated that they are observing secondary waves. The reviewer is not aware of any significant mountain ridge in northern Germany or the Baltic see, which is actually the main domain area in this paper that could excite mountain waves, which then provide the source of the body forces that are needed to generate secondary or non-primary waves. Nor there were thunderstorms or any other extreme meteorological events during these 7-days. Please provide evidence for this statement.

The reviewer stops here, although there are further major comments, but there is no point to add more criticism. It will not change the suggestion of rejection.

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