

***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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Second 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

C1

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

The reviewer thanks the authors for the quick reply and the comment about etiquette, which was very much appreciated. It might be true that the etiquette is not the main concern of the reviewer. However the mathematical and physical correctness does matter. The reviewer actually tried to be constructive, but also did not want to embarrass the authors too much. My apologies for that. I hope this is acceptable for the authors. Although the reply to the comments was now written in the I-form, which suggests that it was not iterated with the co-authors, the reviewer is going to respond to the authors.

Anyway, the reviewer wants to be constructive and is going to present some of these non-trivial computations mentioned in the replies about the heating. However, the reviewer also appreciates some of the additional information provided in the reply about radio stations. The reviewer was not aware that radio broadcasts are transmitted by vertical pointing antennas, but maybe some astronauts enjoy listening for 5 minutes every 2 hours.

In fact, this review shows that the review process works. Even mistakes that were not found in previous reviews are essentially brought to discussion.

In detail: General comment (Vierinen et al., 2019) Very interesting reply. Equation 5 of the submitted manuscript and Hocking (2005) coincide for  $i=j$  and, thus, the intended correlations are of the wind variances and momentum fluxes. The authors refer to that by citing Sato et al., 2017. As they even claim that they want to present a generalization of Hocking (2005). If the new method is a generalization why should the name of physical quantities now become second order statistics, if they before were termed wind variances and momentum fluxes.

Vertical winds and heating experiment The reviewer asked the authors to estimate whether they could estimate the degree of magnetization or provide any other mean of

C2

estimation to rule out that they actually actively modify the mesosphere/lower thermosphere by their cw-heating experiment. A statement about the effect of magnetization of meteoric plasmas can be found in the literature, but if the authors don't want to search than it is hard to find that. The reviewer did not mention to present scattering simulations. Furthermore, what the authors believe is not relevant. It is more important to estimate and check.

The Tromsø heater consists of 12 100kW cw transmitters (see webpage EISCAT Association). The SIMONE system consists of 5 450W cw-transmitters (Chau et al., 2019). Now we start with the non-trivial computation of the energy, that is transmitted, during one heating cycle. The Tromsø heater conducts typically heating experiments with 4-10min heating and then it is switched off and the ionosphere can relax again to its initial state. The SIMONE cw-experiment conducts a heating cycle of 24/7. Power is defined as energy per time and 1 W corresponds to 1 Joule per second and, thus, the total energy per heating cycle is given by the integral of the transmit power over time. We assume a heating cycle of 6 min for the Tromsø HF heater, which is sufficient to cause huge ionospheric modifications. These modifications are immediately visible after the heater starts to heat.

Tromsø heater (6 min heating cycle)

$$12 * 100 \text{ kW} * 6 * 60 = 432000 \text{ kJ}$$

SIMONE heating (24 hour= 86400s), the effective integration time might be a bit shorter and in the order of 8-14 hours, but this is only producing a factor 2 less, which is negligible in this approximation.

$$5 * 0.450 \text{ kW} * 86400 = 194400 \text{ kJ}$$

Repeating this computation in the ERP domain leads to an effective heating of SIMONE compared to the HF heater of 2-4%, however, as the HF heating is instantaneously visible after starting the heating experiment viz., already much lower powers seem to

C3

modify the ionosphere. Considering the results from above, the transmitted energy from SIMONE is essentially sufficient to actively modify the mesosphere/lower thermosphere, which was the question raised in the review by both reviewers and reaches effectively about 2 GJ energy during 24 hours. However, the heating efficiency is much lower compared to the TROMSØ heater (about 54000GJ per 6 min), but the amount of energy that they deposit in the MLT is still gigantic and a true VHF environmental pollution problem. Although the exact numbers might change a bit, it is clear that such a mode leads to changes in the ionospheric components and presents a reasonable explanation for the observed vertical winds. The reviewer agrees that an exact quantification is more challenging. It should be nearly impossible to get some useful information out of these data in a geophysical sense. Furthermore, considering Figure 2, there are several features providing evidence that support the outlined explanation. The vertical winds strengthen with altitude (higher degree of magnetized electrons) and with campaign duration. The energy is even high enough to modify the background state. Secondly, the maximum upwelling can be found during daytime, where the highest ionization is present. It might be even possible to find an image of their linear polarization radiation diagram in the sky when imaging the winds or other quantities (should look like a dipole).

Thus, neither the vertical nor the horizontal neutral winds are reliable and, the whole campaign is geophysically pointless in that respect. They could not even trust the pulsed systems either as they most likely actively heated the whole environment or network volume and they cannot remove these effects and obtain neutral or unbiased dynamics.

The reviewer feels a bit sorry, but the reply by the authors falls much too short and arguing that things are complicated or non-trivial is, in fact, no excuse to not do the homework and sometimes simple physics is already enough to estimate potential issues. Although, an exact threshold is difficult to define, the reviewer assumes that one has to stay orders of magnitude (4-6) below the energies of the heater, which is not the

C4

case for SIMONE in general. The computation of the magnetization of the electrons is comparable non-trivial as the estimated power deposition of the cw-experiment and the reviewer omits this here.

Pulsed radars vs cw The main difference between a pulsed system and a cw-transmission is the collisional coupling. Between the pulses the Brownian random motion of all atmospheric molecules removes/forces all electrons to remain collisional coupled to the neutrals or ions in the meteor plasma. During the pulse the electrons are going to respond to the e-m-wave, but first have to overcome their inertia due to the random motions. In a cw-case the relaxation time is essentially zero and the electrons are moving as a whole cloud absorbing more and more energy from the radio wave. The magnetized part of electrons is going to be accelerated and their motion is going to be controlled by the e.-m. field of the radar wave plus the Earth magnetic field and currents. The rest can be found in text books.

d) Wind tests The reply contained a circular reasoning and proofs nothing. They cannot remove these vertical winds, as they are an essential quality control and actually point out severe issues in the analysis. Even suggesting that is close to data manipulation or fraud, which is not acceptable. They should really have a look to the guidelines and the rules of scientific publications. Observations should be always presented without manipulation or if changes were made these have to be descript and explained.

The reviewer asked about the meaning of the terms obtained by the VVP applied in Chau et al., 2017 and here. Furthermore, the reviewer questioned the applicability of this method for the 15 min resolution. Obviously, the number of meteors was not the answer to the questions.

e) and f) The authors did not answer these questions.

g) WCFI The reviewer raised serious issues about the mathematical correctness of the method and provided an outline why. Answering that the paper was accepted and they just copy the analysis is not sufficient. The reviewer tested whether the equations 2

C5

and 3 hold the linear polarization relation for gravity waves (not really difficult) and also explained what the problem is. However, the authors did not even provide a single argument, why the method should be correct or how they consider to deal with these arguments. The reply "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". Do the authors have arguments to show that the raised concerns are incomplete or not? If not Vierinen et al., 2019 should be withdrawn, as obviously there is no possibility to prove the mathematical or physical applicability to this type of inverse problem, but it is possible to show that the the underlying equations do not hold the linear polarization relation in a 3D atmosphere. The reviewer takes this statement as an agreement to the points raised concerning the WCFI method and recommends therefore the rejection of the paper. The authors show no arguments to justify a publication, but still want to publish the paper, which is a bit odd. Considering that the key analysis method is not applicable. What is left of the paper to justify an ACP publication? The authors didn't even know/consider that they performing an unintended heating experiment.

Contradictions: The authors did not reply to the contradictions related to other papers of the group namely Vargas et al., 2020 and Conte et al., 2020 and Chau et al., 2020. These contradictions have to be clarified. It is not acceptable to change the conclusions like a candle in the wind. This is scientific non-sense and not enhancing the community knowledge.

Referencing: The referencing is not acceptable and adding more self-citations won't help to improve that.

In general, the reply of the authors covered maybe 1/3 third of the questions raised. The authors might want to use a bit more time for the next round and provide a point-by-point reply to the mentioned concerns, which fits to the etiquette.

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-974>,

C6

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C7