

# ***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 #1**

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The paper presents results from a the SIMONE (Spread-spectrum Interferometric Multistatic meteor radar Observing Network) approach conducted in northern Germany in 2018. Specifically, the manuscript discusses the statistics of mesoscale MLT power spectra determined through observations obtained during this campaign. The SIMONE 2018 campaign comprised of fourteen multistatic SMR links allowing to build a substantial database of specular meteor trail events, collecting more than one hundred thousand detections per day within a geographic area of  $\sim 500$  km X 500 km. The

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manuscript reports power spectra in frequency range obtained using a Wind field Correlation Function Inversion (WCFI), and Mean Wind Estimation (MWE), which determines the MLT winds and gradients from specular meteor observations.

It is important to note that the campaign is not driven by an atmospheric or geophysical event, but this is more a technical demonstration of the use of many radars to obtain the geophysical values of interest reported in this manuscript. Thus, this manuscript is mostly a report of 'new' technical methodology, which in principle, a journal like Atmospheric Measuring Techniques would be a better venue than *Annales Geophysicae*. Unfortunately, in its current form and with such aim, the manuscript is pretty much a repetition of the paper published by Vierinen et al. (Vierinen, J., Chau, J. L., Charuvil, H., Urco, J. M., Clahsen, M., Avsarkisov, V., et al. (2019). Observing mesospheric turbulence with specular meteor radars: A novel method for estimating second-order statistics of wind velocity. *Earth and Space Science*, 6, 1171–1195. <https://doi.org/10.1029/2019EA000570>). Even the title is similar. The main difference that is easily noticed is that the Vierinen et al. paper uses 24 hrs of data, while this manuscript uses 7 days of data. However, there is no significant benefit for the purpose of this manuscript to use more data such that it warrants a new publication. E.g., the technical demonstration using 24 hrs of data or 7 days of data yields the same results. I believe, but not entirely certain since the manuscript makes no effort to describe the differences between the two reports, that this manuscript uses a few more radar stations. However, the methodology to obtain the reported variables are the same, as well as the math, etc.

Finally, the manuscript reports mean vertical winds of 10-20 m/s for several hours, which are physically nonsense. This points to a major flaw either in the data analysis or experimental setup. Such winds correspond to a vertical upwelling of 30-50 km in a few hours on a regional scale. Where does the energy come from? This upwelling would be accompanied by an extreme cooling that is not observed in SABER. Thus, there is a major conceptual issue that cannot be addressed in a major revision.

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In its current form I recommend rejection of the manuscript

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