

Atmos. Meas. Tech. Discuss., community comment CC4
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Reply on AC4

Gunter Stober

Community comment on "Four-dimensional mesospheric and lower thermospheric wind fields using Gaussian process regression on multistatic specular meteor radar observations" by Ryan Volz et al., Atmos. Meas. Tech. Discuss.,
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Dear Ryan,

thanks for clarifying the 2D nature of the presented retrievals. From the reply I take that the winds are computed within a 2D layer with an irregular horizontal grid using a 3 km vertical average. This setup sounds reasonable. The terms 2DVAR or 3DVAR or higher are typically used for data assimilation e.g., Gelaro et al., 2017 for MERRA2 (3DVAR) and Eckerman et al., 2019 for NAVGEM-HA (4DVAR) and references therein. It might be worth describing the algorithm concerning this aspect with some more details.

WGS84 geometry implementation:

The reply about the WGS84 geometry implementation somehow confused me. Chau and Clahsen (2019) cite Stober et al., 2018 as a source of the WGS84 geometry. I am even more confused as one of the co-authors of this paper (Jorge L. Chau) confirmed this in a public comment a few weeks ago (see link: <https://doi.org/10.5194/amt-2021-124-CC2>).

I might also want to recall that we had a good meeting in Kühlungsborn in March 2017. The attendees were Ryan Volz, Philip J. Erickson, Juha Vierinen, Jorge L. Chau and Gunter Stober. We discussed the multi-static projects and presented the status and help the MIT colleagues to catch up with the multi-static developments that were done so far until 2017. The patent is mainly based on the details presented in Stober et al., 2018 including the WGS84 geometry, which was already in parts implemented in Stober and Chau (2015), but only to solve the forward scatter geometry and the altitude of the meteors without angular correction. Due to the embargo time between patent submission and the patent acceptance the submission to AMT had to be delayed. If the editor needs further evidence for the timeline, I can forward the documents in a private e-mail to avoid public embarrassment.

I also tried to google the cited thesis of Clahsen (2018), but I received a match to another AMT paper, which cites Stober et al., 2018.

Vertical winds:

I am also confused about the reply on the vertical winds. They now justify the vertical winds as real by presenting additional citations. This is contradicting another time a public comment by Jorge L. Chau (see again link from above) related to the SIMONE systems. In the public comment, Jorge L. Chau clearly confirms that there is no claim that the winds are physically meaningful or presenting the geophysical truth. I suggest they add a paragraph discussing this obvious contradiction or they present a physical explanation within the laws of thermodynamics that these winds are realistic. Considering that the residual circulation causes a 100 K deviation from the thermal equilibrium at the mesosphere lower thermosphere (Becker, 2012, Smith, 2012) and is associated to mean vertical velocities of a few mm/s to cm/s. So it might be worth estimating the adiabatic cooling of the presented 10 m/s upwelling and estimating the cooling rate per day. If these winds are real, this will be a game-changer of our understanding of the middle atmosphere dynamics at the MLT. Otherwise, they can also discuss the obvious controversy to other observations. I added some references, which show different vertical velocities. It would be good to discuss these things a bit more. It is the one point that doesn't fit, that brings science forward.

I really enjoyed reading the paper and I am looking forward to future scientific results.

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Vertical wind observations:

Most of them contradict the wind speeds cited in the manuscript (certainly not complete).

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