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Comment on amt-2021-40

Anonymous Referee #1

Referee 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., https://doi.org/10.5194/amt-2021-40-RC1, 2021

This is an important, clearly-written article which describes the application of Gaussian process regression (GPR) to MLT meteor radar winds data acquired with multistatic MIMO meteor networks which produce angle of departure and angle of arrival observables in addition to Doppler shifts. Thanks to wavevector diversity, data from the network can be used to characterize the three-dimensional wind field without the ambiguity inherent in individual monostatic systems which provide no information about horizontal vorticity. The GPR method is tested here as a framework for resolving fine-scale structure in the flow field and for performing error propagation. The GPR formalism is developed and tested using synthetic and actual data. The results are compared with those from the more conventional homogeneous and gradient methods.

Ovearll, the significance of the paper is excellent in view of the importance of the MLT winds (which are crucial for understanding ionosphere/atmosphere coupling) but difficult to measure using other ground-based methods. Also noteworthy is the novelty of the multistatic method which is revitalizing the meteor-radar field. The potential of the method can be realized through incisive analysis such as is provided by GPR. The scientific method is also excellent in view of the appropriateness of the methodology and the clarity of the fomalism and explanations. The presentation quality is good and could be improved by clarifying the meaning of some of the figures.

The paper is acceptible for publication but could benefit from addressing a few minor questions:

1. Throughout the paper, the GPR method is described as being

non-parametric. However, the prior model for the winds is necessarily parametrized by the means and variances of the wind components (theta). In what sense is this method non-parametric?

2. The paper distinguishes the GPR method from Tikhonov regularization which is viewed as a competing method. However, 2nd order Tikhonov regularization can be interpreted as the adoption of a Gaussian prior for the state vector. How is this fundamentally different from the method presented here?

3. The paper describes the assumption of a Gaussian random process for the winds as "convenient" because of the tractable computations that result. Could not the authors provide a more satisfying rationale by considering whether the central limit theorem applies to the MLT wind components?

4. The authors of the paper note the absence of other kinds of MLT wind measurements that could be used to validate the wind estimates produced by the GPR method. Have the authors considered the use of generalized cross validation?

5. The authors note that the region of validity and the spatial resolution of their method depends on the geometrical configuration of the multistatic meteor array. Have they considered developing a geometrical dilution of precision (GDOP) estimator?

6. Finally, interpreting figures 2--4 and 7 is very difficult in view of the fact that color gradations are being used to represent multiple quantities simultaneously (i.e. horizontal winds, vertical winds, and data quality). The authors should attempt to clarify these figures.