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Comment on amt-2021-40

Anonymous Referee #2

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-RC2, 2021

The present manuscript describes a new algorithm to estimate mesospheric and lower thermospheric (MLT) winds applying the Gaussian process regression (GPR) technique using meteor radar data collected during the SIMONe 2018 campaign in Germany. The algorithm is effectively a Bayesian approach, thus it requires prior information that is provided in the form of a mean and covariance matrices for the prior wind components. The covariance matrix is modeled as a Matern-5/2 covariance function that has amplitude and scaling parameters that can be estimated as part of this technique or that can be provided directly based on some previous analysis of the observations. The document is well written, the description of the algorithm is clear, and the analysis of the results is profuse. Given the novelty of this approach and the need of measuring MLT winds with higher spatial and temporal resolutions to study the dynamics of this region, I would like to recomend the present manuscript to be published after some minor revision. Below, I present a few questions or comments that should be addressed by the authors.

* Based on the description of the GPR algorithm to estimate winds at any particular point in space and time, it is necessary to provide the mean and covariance matrix of the a priori wind distribution. While there is a nice description about how the covariance matrix can be modeled as a Matern-5/2 covariance function, there is no much discussion about how to determine the prior mean wind. In the manuscript, it is mentioned that it is obtained from applying a tensor product cubic spline to the dataset. However, it is not clear, at least to me, if the authors are calculating this mean from wind estimates that were obtained applying a different method, for instance with the zero-order method. Please clarify this point. In addition, the role of the prior covariance is analyzed in detailed, but the role of the mean is not. I can imagine that the results have a strong dependence on the a priori mean that is provided. The errors will probably increase depending on the accuracy of the mean. I would also suggest to discuss about the role of the a priori mean wind field in the manuscript.

* The position of the detected meteors are also the result of a fitting procedure, thus there are uncertainties associated to the space and time location of a meteor. How these uncertainties are taking into account in the GPR algorithm? Do they play any role in the accuracy of the estimated winds? I understand that a filter criteria is applied to the data to consider only high quality detections, what is the criteria that is used? Do the high quality detections have negligible uncertainties for the meteor locations? I would also suggest to discuss this question in the manuscript.

* The detection of meteors in time can be modeled as a Poisson process, in the sense that given a detection the probability of detecting the next meteor increases as function of time. Given this, the time location of the Doppler samples is also a Poisson process. However, for the GPR approach, we are assuming that the Doppler samples can be modeled as a Gaussian process in space and also in time. What is the impact of this difference in the estimation of the MLT winds?

* While the role of the covariance amplitudes for the a-priori wind distribution are discussed in detailed, there is no much discussion about the role of the space and time scales. In principle, these parameters are also obtained from minimizing the negative log-likelihood, however, I can imagine that their values will strongly depend on the distribution of data considered. For instance given some particular data set where data samples are more concentrated around 90 km but more sparse around 80 or 100 km altitude, it is reasonable to expect that the space scale values will also vary as function of height, they will be probably shorter round 90 km but longer at higher or lower altitudes. Is this something that is considered in the algorithm? How the wind estimations will be affected by the precision of the space scales used in the algorithm?

* In section 5.2, in the case of the horizontal wind, it is not clear whether the "mean bias error" or the "mean absolute error" were calculated. The labels in Figure 3 indicates "u+v", does this mean that the values of the zonal and meridional winds were added before calculating the error? Please, clarify this issue, the authors may consider to include the actual formulas used to estimate the errors. Also, in the same figure, the titles of the plots on the right side indicate "variance of horizontal wind", however, I think the authors

are referring to the variance of the horizontal wind error. Please, fix the labels of the figure to clarify their meaning. Similar comment with respect to Figure 4, have the authors plotted the variance of the vertical wind or the variance of the error?

* In section 6, in the implementation of the algorithm, it is mentioned that the data is divided in time intervals of 90 minutes with 30 minutes overlapping, would not this have an impact on the smoothness of the winds that are estimated? Would not it have sense to apply a similar criteria in space (altitude, latitude, and longitude) based on the actual distribution of meteors? The accuracy of the wind estimates in the areas further from the center may improve if covariance parameters are computed particularly for these regions. This comment is related to the role of the space and time scales presented above.

* In Figure 9, Gradiente winds and GPR estimates are compared. In particular, it is mentioned that the GPR estimates show some mesoscale structure. Are the authors implying that the GPR method was capable of estimating these mesoscale structure while the other method could not do it? I would recommend to clarify this comparison. The gradient winds were computed considering wider time intervals, and probably that has an impact in the smoothness of the estimated winds, thus the comparison would not be fair.

* In Figure 2 and Figure 7, the vertical winds are depicted as the color of the horizontal wind lines. In fact in the labels, it is mentioned "vertical wind speed", however, "speed" by definition is a positive value, and the colors go from positive to negative, so I think it is more appropriate to change the label to "vertical wind". In addition, are the values of the estimated vertical winds within the expectation? How do they compare with their corresponding variances? Are the error bounds small enough to have good vertical wind estimates? It would be important to add a discussion in the manuscript about the accuracy of the vertical winds estimated with the GPR method given that previous methods have just assumed that the vertical winds are zero.

* Finally, in the conclusion section, the authors indicate that the GPR method can resolved winds at the finest spatial and temporal scales allowable by the instrument. However, what are these finest scales allowable by the instrument? The geometry and the spate-

time distribution of meteors will definitely set limits to the features that can be resolved both in space and time with this method, however, aren’t other methods within their assumptions also capable of resolving fine structures? In fact, in section 6, it is mentioned that the authors do not have a ground truth to validate the horizontal scales that are resolved by the GPR method. Given this it cannot be claimed that the GPR method resolves the finest scales allowable. I would recommend to change this conclusion.