Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., community comment CC1
https://doi.org/10.5194/amt-2021-124-CC1, 2021

Comment on amt-2021-124: On vertical velocity estimates

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Community comment on "Atmospheric tomography using the Nordic Meteor Radar Cluster and Chilean Observation Network De Meteor Radars: network details and 3D-Var retrieval" by Gunter Stober et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-124-CC1, 2021

The authors present an interesting technique to study gravity waves (GW) scales in the mesosphere and lower thermosphere (MLT) using multistatic meteor radar geometries, either by combining closely located monostatic systems or using additional receivers to existing monostatic systems. Given that the concept is relatively new and has a lot of potential, this and other techniques that resolve wind fields in smaller spatial scales are needed to explore MLT GW scales.

As the authors mention in the paper, usually meteor radars have not fitted for the vertical velocity. They present interesting results using monostatic systems by using a regularization scheme (e.g., spatio-temporal Laplace filter). The results are indeed interesting, since my colleagues and I do obtain larger variability on the residuals (or apparent vertical velocity) when we apply a gradient method (Chau et al., 2021, Conte et al., 2021). Note that the variability without fitting for gradients would be even larger due to horizontal divergence contamination as clearly shown in Chau et al. (2017). I tend to attribute the large difference on the distribution of residuals to the regularization the authors are using.

However, in lines 169-175, it appears that the authors attribute the estimated vertical velocities by Chau et al. (2021) and Conte et al. (2021) to not implementing the WGS84 geometry,

"The implementation of the WGS84 Earth geometry turned out to be important for all meteor wind analysis and essentially reduces the projection and altitude biases of the observed meteors. In particular, at mid- and high latitudes these corrections are rather significant and lead to substantial improvements of the wind as well as the vertical profile of ambipolar diffusion and, thus, has an impact on the temperature estimates (Hocking, 1999; Hocking et al., 2001). Furthermore, the vertical wind mean bias and variances are significantly reduced compared to other studies (Egito et al., 2016; Chau et al., 2021; Conte et al.,2021) without including additional damping terms or regularization

constraints to the fitted vertical wind."

Could you please provide evidence that this is the case? For example, if you do not apply WGS84 the resulting vertical velocities are comparable to those reported by Chau et al. (2021) (at low latitudes) or Conte et al. (2021)? Although we are using WGS84 and the proper geometrical considerations (e.g., Bragg vector with respect to the local geographic coordinates)**, it would be good to know if the distributions of residuals in Figure 2, that are much narrower than reported in other works, is due to implementing WGS84 "without any damping terms or regularization constraints to the fitted vertical wind" or due to the regularization explained in lines 125-140.**

In the case of vertical velocity estimates obtained as part of 3D wind field estimates (e.g., Figure 13), I do expect them to be less contaminated by horizontal velocity spatial inhomogeneities, since these inhomogeneities are resolved (or better resolved). The control simulations of one of my students (paper to be submitted) indicate that these horizontal velocity spatial inhomogeneities are the source of the observed variability on vertical velocity estimates (i.e., provide apparent vertical wind). However, they are not responsible for the relative high values lasting for a few hours above 95 km.

Again, I want to reiterate that this additional technique to resolve wind fields is interesting and needed to attack MLT GW scales.

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