Review of Multistatic meteor radar observations of gravity wave-tidal interaction over South Australia.
by Andrew J. Spargo et al.
(Atmos. Meas. Tech. Discuss. AMT-2019-138)

This clearly written, very detailed paper, reports on a statistical simulation of bistatic meteor radar data including assumed realistic noise, biases, measurement errors, based on actual meteor distributions, and added pertubations from expected gravity wave parameters - the object being to estimate typical biases and uncertainties of e.g., $< u'w' >$, under various filtering and refection criteria.

A quite interesting observation is that the addition of a remote receiving site, which seemingly increases the diversity of meteor echoes does not contribute except by numbers: an expensive way to discover that, but valuable nonetheless.

Sporadic meteor distributions measured by radar are not uniform, e.g. in azimuth - they can vary hour to hour and day to day depending on the orientation of the source regions. This model is based on actual distributions. How much error in GW forcing could be due to non-uniform distributions, which are inherent in the use of meteor radar.

Pg 10 line 5: are the decay times for the remote site expected to be the same as for the main site?

Pg 11 line 21. Was the tidal phase adjusted for each meteor's position or is that correction judged to be overkill?

Pg 12 Line 5: I have brought this up before and been shot down - so I will try again. It appears that a basic assumption of the method is that the atmospheric motion at the meteor is perpendicular to the trail; that is, that the echoing region has a vertical velocity component. This might be true if there is a "hot spot" (point scatter) in the trail , but for a straight line reflector the reflection point would be expected to slide along the trail if necessary to maintain perpendicularity. There would be a very small change in zenith angle, but no vertical velocity is needed.

If there were a large numbers, uniform azimuthal/time meteor distribution at the height of interest, the sliding effect would be expected to have minimal influence. Otherwise significant covariances could be created from horizontal variations alone (no vertical motion).

Another question in the same vein: in the monostatic case, if there were only zonal wind perturbations, then because of the radial measurement, there would also appear to be meridional perturbations. That is, zonal and meridional perturbations “bleed” into each other. Does this affect your results? It seems that bistatic operation would mitigate this concern to some extent.

Pg 12 line 10 - it’s not clear what is meant by a (square?) radial/AOA pair - does that refer to a single meteor ?

Pg 12 Line 17 is there an extra ”i” in this equation?

Figure 5,6 red(-dish) lines (and yellow) are almost invisible (despite the caption, I don’t see any thick lines). Figure 7 is good (probably because all lines are thick - when some should be thin. Solid and dashed thick might look better.

Fig 13 The zero U and peak forcing ‘line’ are very close over the plotted heights. It appears that U leads the forcing. Curious.

Minor:
Pg. 6 line 20 “Spatially distributed...”?

—///—