Referee's report on the paper

Multistatic meteor radar observations of gravity wave-tidal interaction over Southern Australia

by Spargo, Reid and MacKinnon

The paper looks at the accuracies of momentum flux determination using bistatic meteor radars, and some consequences. It is well written grammatically, and has been adequately proof-read. Indeed it was a pleasure for once to read a paper which did not require that I spend large amounts of extra time correcting grammar, so congratulations to the authors for that.

Scientifically, the paper is of interest but contains some significant points of potential confusion or even error. I shall address these in more detail later, but briefly these are:

1. As shown in "Spatial distribution of errors associated with multistatic meteor radar", Earth, Planets and Space, 70:93, doi.org/10.1186/s40623-018-0860-2, 2018, some data need to be excluded from calculations, notably those close to the midpoint between the radar transmitter and receivers. In Fig 1., lower right, (bistatic case) these correspond to the region to the south-east of the transmitter, . While modest in number in this region, the radial velocities measured here relate mostly to the vertical velocities, and are best excluded for calculations of winds and momentum flux. Failing to exclude these points can adversely affect the errors. For a monostatic system, these are all data close to overhead of the transmitter-receiver system (the blue-coloured cluster around the transmitter in the lower left figure). Typically data within 12-15 degrees of vertical are removed for the monostatic case (e.g. see Radio Sci., 32, 833-865, 1997). A similar exclusion process (or at least a weighting which reduces the weight of these meteors) should be applied in this paper, but no mention of it is made. While the numbers of meteors in these regions are low (in the blue colours), their effect can be disproportionate, so some discussion about how these cases are treated is warranted.

2. In the same spirit, Fig. 3 is a bad choice of figure, since it seems to refer to a horizontal meteor trail - which is the one case that should always be avoided, as it produces measurements of the vertical wind only, and consequently division by zero when trying to deduce "horizontal winds". I suggest using the figure, but tilt it at some arbitrary (non-zero!) angle from the horizontal.

3. The paper does not discuss how the Bragg angles are found. The paper by Stober et al. (2018) suggests they always point exactly to the centre-point of the transmitter-receiver line, but this is in error, and only approximately true (as some simple geometric calculations using high-school-level trigonometry will show). I believe the authors of the Stober et al. paper sent a correction to the journal in this regard. Incorrect calculation of the Bragg angle will bias the results.

4. The appendices (software) of Stober et al. contain couple of typos - the authors should confirm that they found these.

5. The authors use a spherical-Earth calculation for the bistatic case, as they should. But it is unclear as to whether they did the same in the monostatic case. In some radars, a flat-Earth approximation is used for the monostatic case - the error in height is typically 0.5 km or so at

zenith angles of 45 degrees. But it is unclear here whether the flat-Earth approximation was used in the monostatic case, or whether a full spherical Earth approximation was used in both cases. If a flat-earth approximation is used in one case, and a spherical-Earth system used in another, it could lead to biases in comparison. This may also impact the upper-right graph in Fig. 1 (please add labels a,b,c,d - its getting annoying referring to "upper left", "lower right" etc.)

The above 5 points could bias the calculation of momentum fluxes. Perhaps the authors have considered these points, and simply not mentioned them, or perhaps they neglected these matters - either way, these matters must be considered and discussed in the paper.

Another point of note, though maybe less important, is the mean Bragg wavelength. The authors note that the bistatic procedure gives rise to a diversity of Bragg scales, but it must be remembered bistatic studies also alter the mean Bragg values. The effective Fourier scale involved in the reflection process is of course $D = \lambda/[2 \cos (\beta/2)]$ (using the notation of Fig 3). If $\beta/2$ is say 30°, $D = 0.58 \lambda$, rather than 0.5λ as for the monostatic case. So it is as if the radar had a frequency of 47 MHz rather than 55 MHz. So it might be of interest to note that the mean effective frequency has been lowered - and meteor detections tend to be more common at frequencies closer to 30 MHz. The improvement in detection rated may be more significant in the case of a 55 MHz radar because 55 MHz is not normally considered optimum for meteor work, so a shift to 47 MHz might be more dramatic (relatively). It would be of interest for the authors to look not just at diversity effects but also to consider the mean effective wavelength due to the bistatic arrangement. My choice of β of 60 degrees is arbitrary - the authors should be able to find a more realistic mean using averaging over real values of β.

Now I will progress to other matters, and work through page-by-page.

Page 6, line 10 - the term "said distributions" seems clumsy - the word "said" is used in this way several times - it does not add anything to the text, and I would recommend removing it.

Page 8, line 4 - the authors talk about "well-known linear GW polarization relations". There is no *single set* of "linear relations" -this is just one of many, and it is the simplest. It all depends on whether you include Coriolis terms, Brunt-Vaisala terms, scale-height terms etc -these cases *still deal with linear waves*, but the detailed formulas change. See Walterscheid et al., JAS, "Stokes diffusion by atmospheric internal gravity waves", *J. Atmos. Sci.*, **48**, 2213-2230, 1991, equation (45) and set α to 0 for a more general linearized case.
    So this misleading statement needs to be clarified, perhaps by saying "we use the simplest polarization relations". Of course it needs to be recognized that the *authors own work will be in error at periods close to the tidal and inertial frequencies if they use this simple polarization relation* - Coriolis terms should be included for a proper treatment, for example. This is pertinent to discussions later in the text, where the authors consider the optimum bandwidth for studies and look at GW periods close to the tides.

Page 9, line 9 --- I would make the "dot product" between $\mathbf{v}_{ecef}$ and $\mathbf{e}$ clearer - maybe use a bold dot - it is important that readers do not miss this, and plays into the discussions introduced earlier regarding overhead meteors. The equation emphasizes that when the meteors are close to overhead, the radial velocity is dominated by the vertical wind component (also Hocking, Earth,

Planets and Space, 70:93, doi.org/10.1186/s40623-018-0860-2, 2018 (+ see item 1 on page 1 of this review). As noted, it would be a good idea to change Fig. 3 to have a generalized tilt for the meteor.

Incidentally, the authors used bold font to represent both vectors and matrices. While not wrong, since a vector is indeed a type of matrix, they may want to consider distinguishing these cases e.g. by using an arrow over the vectors and keeping matrices bold.

Page 10, line 18 - how "rare" is "rare"?

Page 10, lines 19 to 21, and page 11, lines 1 - 14.
   These lines are very disturbing. First, the authors seem to have the wrong Holdsworth et al. (2004) paper in the References. The authors give Holdsworth, D. A., Tsutsumi, M., Reid, I. M., Nakamura, T., and Tsuda, T.: Interferometric meteor radar phase calibration using meteor echoes, Radio Sci., 39, 2004. Yet line 19 on page 10 discusses an "error code 3". I cannot see an "error code 3" in this paper. I CAN find one in the paper "Holdsworth, D. A., I. M. Reid, and M. A. Cervera (2004), Buckland Park all-sky interferometric meteor radar, Radio Sci., 39, RS5009, doi:10.1029/2003RS003014". It appears that the authors have referenced the wrong paper, and I believe this is true for (almost?) all references to Holdsworth et al., (2004). Please check. I should not need to have to double-check your own references!
But it gets worse. On page 11, line 3, the authors give Holdsworth (2004) as a representative example of "standard analysis", and then on line 11, make the statement "... we follow the iterative scheme PROPOSED by Holdsworth et al., (2004).". Sorry ,but this is very misleading. This iterative scheme was proposed fully *7 years earlier* (Hocking and Thayaparan, Radio Sci., 32, 833-865, 1997, section 4.1) and again discussed in Hocking et al., *J. Atmos. Solar-Terr. Physics*, **63**, 155-169, 2001.). Holdsworth may have made minor adjustments, but he did not "propose" the idea - it was already proposed 7 years earlier. Indeed much of Table 2 in Holdsworth et al (2004) was also proposed 3 years before Holdsworth (2004) - in Hocking et al., (2001). Repeated reference to Holdsworth throughout the text in regard to ideas which were proposed many years earlier by others is unprofessional and misleading. Perhaps the first author (who, I believe seems to have written much of the paper - and written it quite well, I will add) was simply unaware of these much earlier works, but a proper historical perspective must be presented by rightfully referencing these earlier works in a suitable manner.

In regard to sections 3.7 and on - I again ask - were cases close to the zenith, where the radial velocity is dominated by vertical winds, removed, or given lower weighting? Or how were such cases dealt with? Nasty near-infinity terms can occur when such terms are include, which could bias the resultant mean winds and momentum fluxes.

Page 12, lines 1 to 9. This is another example of poor citations, perhaps due to the first author's inexperience. Line 1 says "The approach we apply is identical to that presented by Thorsen et al., (1997) and subsequently Hocking (2005)." It gives the impression that the ideas were developed by Thorsen and simply re-applied by Hocking. This is patently untrue. The current authors use the matrix **A'** as discussed in lines 7 to 8. Thorsen et al. NEVER used this matrix. The matrix Thorsen et al. used is given in their equation (18), section 3.3, page 714. It is a MUCH simplified version of the true **A'**. The matrix that the current authors use comes straight out of Hocking

(2004) and was never used by Thorsen et al. It makes a major improvement to the analysis. So please replace "subsequently" in line 1 of page 1 with "subsequently improved", and give a specific reference to Hocking (2004) in regard to lines 7 and 8.

Page 12, line 26 - "we have opted to compute the covariances at the origin" - do the authors mean "at the origin at the ground" or "at a point in the atmosphere at meteor heights immediately above the ground". I can see that either might work, but it needs to be clarified.

Page 13, line 1. The statement that "This estimate represents what one would measure with an "anemometer" at some fixed location in the vicinity of the radar", is a little misleading. I understand what the authors are trying to say, but it isn't quite right, and needs some caveats added. First, "in the vicinity of the radar" is misleading. Do the authors mean "in the vicinity of the meteor region immediately above the radar"? As it reads, it could be at ground-level. In addition, in the bistatic case, which radar? the transmitter? or the receivers? True, in this case they are moderately close, but since the report is supposed to somehow represent general multistatic radars, where the transmitter and receiver can be hundreds of km apart, the question is no longer pedantic. Furthermore, a suitable anenometer might truly measure wind components, where with the radar we have this problem that we deal with radial components. So while I get what the author is trying to say, I suggest it be mentioned with some more caution than has been applied..

Page 14, lines 2-3 - good point.

Page 14, lines 9-10 - OK, but as discussed earlier, could some of the increase in count rates be due to a new effective mean frequency? And I also ask again - did the monostatic radar use the same "spherical Earth" geometry as the bistatic case?

Pages 15-21 seem quite good. However, the term "gap" is used in Figs. 10, 11, in line 10, page 21. "Gap" gives the impression of a "hole" - I would say the term "temporal shift" might be more appropriate.

Page 21, line 1 - the authors say ".. aside from considering that the GWs may have propagated from a region with weak eastward mesospheric winds." Do they really mean this? Or do they mean "... GWs may have propagated through a region with weak eastward..."? It's the filtering that matters - so either the statement needs some expansion, or the authors really mean "through".

Page 23 line 2 - give a reference to the forcing formula.

Page 23, line 28 - suggest changing "Also like.." to "As for..."

Page 24, lines 11-12 - seems a bit unclear - please try to reword what you mean.

Pages 26 to 28 are a nice summary of the literature, and really emphasize current uncertainties and confusion in this regard.

Page 28, conclusions. Some of these statements may need revisiting following other comments discussed here-in.

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