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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "A long-term comparison of wind and tide measurements in the upper mesosphere recorded with an imaging Doppler interferometer and SuperDARN radar at Halley, Antarctica" *by* R. E. Hibbins and M. J. Jarvis

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We thank both referees for their constructive comments on this manuscript. To summarise, in light of their comments we have restructured the paper into clearer sections on the instruments and data analysis, the comparison between the derived winds and tides and discussion of the results. In addition, we have included new figures showing the approximate sampling volume of the two radars, a meridional wind climatology divided into calendar month and hour of day, and modified several plots for clarity, including significance tests on all reported correlations. We have removed reference to comparisons of the zonal winds, and hence scaled down the discussion on long term trends. Finally the abstract has been extended to emphasise the purpose of his paper as a comparative study rather than a climatology.

Detailed responses to the referees' comments (in italics) follow:

Referee 1 Comments received and published: 13 July 2007

*The authors should clearly specify the main aim of this paper. One of them is to take care applying and combining different techniques.*

Following this, and comments from referee 2 (see below) we have modified the abstract to emphasise that this paper is meant as a comparative paper, as a guide to future studies on the validity of mixing data from these two different radars.

*The second question that comes up after reading the paper and must be addressed by the authors: How reliable are the winds in the MLT region derived from SuperDARN radar measurements which are primarily used to record coherent scattering from F-region plasma irregularities.*

No radar technique offers a definitive "true" measurement of winds and tides and we have been careful in this study not to assume one technique is more correct than the other. Only by repeated intercomparison with different techniques can the reliability of any one method of determining winds and tides be assessed. This study is one such comparison.

*2) The description of the radars, especially of the SuperDARN radar is too short. Some parts are written twice, in the introduction (p. 6575, line 23-25) as well as in Sect 2.1 (page 6576, line 17-20), but more details on the resolution, meteor rates, averaged times, especially on the measuring volumes of both radars and their separation are necessary. I recommend to add a map of the observing geometry comparable to Fig.2 in Arnold et al., (2003). Without additional literature like the above cited article by Arnold et al., it is hard to evaluate the data used in this paper. This problem concerns also to explain more precisely a) why the zonal component of the SuperDARN radar is*

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*noisier than the meridional component; and b) the possible effect due to the back lobe of the SuperDARN, which is mentioned at first in Sect. 4 on page 2582, line 1-2.*

A map showing the approximate sampling volumes of the two radars has been added as suggested here. The sounding frequency statistics of the SuperDARN radar have been added and the absolute echo count rates of the two radars are now included in the figures. The duplication has been removed from the text.

*3) I recommend urgently to consider the following paper by Thayaparan and Hocking, "A long term comparison of winds and tides measured at London, Canada (43N, 81W) by collocated MF and meteor radars during 1994 -1999" , JASTP, 64, p. 931 - 946, 2002. Here the authors are using a relatively large data set of two different radars and received conclusions partly similar to this submitted paper, however with larger differences in the meridional winds compared to the zonal component, vice versa to the results presented here.*

Thayaparan and Hocking (2002) compares data from a MF radar with a VHF meteor radar. As neither of these techniques is the same as the two radars discussed in this paper it is difficult to assess what a comparison of the comparisons would add to this paper. Previous attempts to calibrate each of the techniques used in this paper against other radar techniques are discussed in the discussion section of this paper.

*Another reason to cite this paper is caused by the applied statistical methods for the comparison of two observational data sets with differing accuracies (for details see Hocking et al., Adv. Space Res., 27, 6-7, pp. 1089-1098, 2001) which may be superior to the here applied RMA method by Sokal and Rohlf (1981) assuming the errors in each technique are similar, which is obviously not the case here.*

We carefully considered using the statistical technique reported in Hocking et al. (2001) in this study. This technique allows accurate determination of the relationship between the winds and tides derived from two radars as a function of the variance due to the system noise of each technique. However, we have no way of determining the system noise of the two measurement techniques (or even their ratios) so this technique cannot accurately answer the question "does one radar systematically report stronger winds than the other?". The RMA technique of Sokal and Rohlf (1981) estimates a "true relationship" between the two parameters provided the system noise in one variable is less than 3 times that of the other. Although this is an assumption in our comparisons, the RMA regression technique allows an estimation of the relationship between the two variables over a spread of system variances. A note to this effect has been added to the appropriate section.

*4) The comparison of hourly meridional winds results in a poor correlation in contrast to the daily mean meridional winds. Unfortunately, the authors speculate only that one possible reason for that poor agreement is caused by the different sensitivity of both radars to the gravity wave spectrum. Some more explanations for this statement are necessary. On the first view, as stated by the authors, the maximum of the correlation coincides with the maximum of the annual and diurnal variation of the IDI-echo occurrence, are there any reasons for that? How sensible are the correlation of hourly averages to the applied selection criteria, especially to variations of the necessary minimum number of echoes and of the thresholds of the echo's signal strength used for the averages and fits? 5) In general, the comparison of hourly values as well as of daily values of wind, semidiurnal and diurnal tides leads to the basic necessity to estimate statistical and systematical errors. The discussion of the poor agreement in the diurnal tides due to the diurnal variation of the number of echoes is a step in this direction. On the other side if the amplitudes of the tidal components (especially of the diurnal components at high latitudes) are small, than the comparison of amplitudes and phases will automatically result in weaker correlations.*

95% significance estimates have been added to Figures 3 and 4 (now Figures 5 and 6) as a guide to the probability that the correlations are spurious. This shows that during nearly all of the year the hourly mean data is poorly correlated (i.e. worse than a 1:20 chance of random association). Despite this, Figure 7 shows good agreement between the two techniques when the same data are averaged over time. This is not the case for the daily mean data which correlate significantly all year (Fig. 6). Therefore there must be a process by which the data derived by the two radars varies to a much greater degree on timescales of 1 hour compared to 1 day. This is most likely due to the fact that the two radars are simultaneously observing perturbations in the wind field due to different gravity waves. Whether this is due to a bias for one technique to observe different gravity waves (e.g. the SuperDARN radar only detecting perturbations due to long vertical wavelength gravity waves) or whether the gravity waves vary on the scale of the difference in the sampling volumes of the two radars is undetermined. However, Arnold et al (2003) note a poor statistical variance between hourly wind data derived from different quadrants of a single meteor radar, and Fraser et al (2006) even see large MLT wind inhomogeneities with scales of the order of 10km and 1 hour from common volume meteor wind measurements. Therefore it is reasonable to assume that there is considerable variation in the gravity wave fields over distances smaller than the separation of the two radars' sampling volume. The references to “sensitivity” of the radars to a spectrum of gravity waves have been removed from the text and additional discussion of the Arnold and Fraser results have been added to the discussion section.

In generating the SuperDARN meteor winds we have used the commonly-used selection criteria for observed echoes and signal strength (Yukimatu and Tsutsumi, 2002). An attempt to re-optimise these criteria based on the observed IDI winds is beyond the scope of this paper.

*6) Comparisons of hourly and daily values are made only for the meridional winds because the derived zonal wind components of the SuperDARN radar are noisier than their meridional component (see 2). But starting with the climatology and its discussion, the behavior of the zonal winds (6b and 7) is included without any additional explanations.*

The paper now only discusses the meridional winds.



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*7) The discussion of differences of mean winds derived with both radars for years with high and low solar activity (Fig. 9) is very instructive. It is only questionable; whether Table 1 and Fig. 8 are still necessary, considering the author's opinion on page 6584, line 5, that detailed discussions of the long term trends are beyond the scope of this paper.*

We have modified this sentence to read "detailed discussion of the causes of the long term trends are beyond the scope of this paper". Table 1 has been removed as without the zonal wind data (see above) it is less instructive. The long term change section now only discusses the meridional winds.

*8) If there are only uniformly distributed statistical errors, than the long term measurements of both measurements should not differ of each other. If there are differences like those in the meridional wind climatology during the SH-summer months (Fig.9) then there are systematic errors and possible reasons should be discussed. One direct question arises here: Do the mean heights of the meteors depend from the solar activity? Other possibilities for the differences are the well known, but not shown here, stronger gradients of the winds during the summer months in connection with the uncertainties of the mean height of winds derived with the SuperDARN radar.*

Clemesha and Batista (2006) estimate a change of 540 m between the mean meteor ablation altitude from solar max to solar min. A typical summer time meridional wind shear above Halley at 95km of around -1ms-1/km (Hibbins et al 2006) would only account for a change of  0.5 ms-1 in the mean SuperDARN meteor winds over a solar cycle, so this is unlikely to be a significant effect here. As mentioned in Section 3 we see no change in the altitude of the peak of the correlation coefficient with season, suggesting that seasonal effects due to the change in the wind shears are not significant at the 5 km vertical resolution of the IDI.

Minor remarks:

*1. The statement in the abstract that climatology of the 12-h tidal amplitude and phase*



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*in both the zonal and meridional components derived from both techniques are in "excellent agreement" seems to be slightly overdrawn because figure 4 and 5 show only correlations coefficients up to about 0.4 for the daily amplitudes of the semidiurnal meridional component.*

"Excellent" has been replaced by "good". The significance of the correlations are discussed elsewhere in the text.

*2. In Fig 6a) no HWM 93 meridional wind results are shown. If they are overlap (as mentioned in the Figure caption), then the authors must specify the corresponding component*

The figure has been redrawn with colour coding. The HWM-93 meridional winds (overlapping) now appear as a red and black dashed line to emphasise that they overlap.

Referee 2 Comments received and published: 23 June 2007

General comments

*1) This paper compares wind estimates for an altitude of 90-95 km made using two instruments that have operated at Halley Bay in Antarctica since 1996. Parameters derived from these wind estimates (mean winds, tidal amplitudes and phases) are also analysed. Care is taken to ensure the consistency of the comparison and advanced regression analysis techniques are used. However, it becomes apparent that the agreement between the two wind estimates is often very poor. This limits the utility of such a study unless some significant insight into the cause of the disagreement can be presented. Comments suggesting reasons for the disagreement are scattered throughout the paper but the focus remains on presenting comparisons. The paper would be greatly improved if the cause of the disagreement between the instruments became more of a focus.*

This paper is not intended as a climatology per se, nor is it a technique paper. Numerous articles have been published on the IDI and SuperDARN meteor wind tech-



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nique previously. Rather it is intended to highlight situations where commonly used techniques to derive winds and tides, when applied to these two radars, can be used to draw reliable conclusions from comparative studies based on geographically distributed pairs of instruments. Hence the disagreements between the instruments are as important, if not more so, than the situations where the two radars are in good agreement (e.g. the derived semidiurnal tides). To emphasise this the abstract has been amended to clarify the purpose of this paper.

Specific comments

*1) As figures 3, 4 and 5 show, the correlation between the measurements made by the two radars is often very poor. But surprisingly (and possibly instructively) there are some parameters for which the correlation is better than others. It is argued that the differences in the correlations of the hourly data and the tides are due to different gravity wave fields being present in the two (separated) radar sampling volumes. But this does not explain the difference in the correlation for the mean winds and the two tides. Nor is the variation of the correlation through the year explained in this context. (Perhaps the gravity wave field differences at the two sampling values vary through the year?) The meaning of the word 'sensitive' in the context of P6574 L8, P6578 L17 and P6585 L13 needs to be expanded (at least). Alternately a model to explain the observations more fully should be developed. (A suggested model has been included below for discussion.)*

See response to comments 4 and 5 from referee 1.

*The authors should make a clear note about the separation between the sampling volumes of the two instruments when the data properties are presented so that the reader can interpret the observed differences with this in mind.*

See response to comment 2 from referee 1

*Aspects of the measurement technique that draw the SuperDARN meteor radar re-*



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*sults into question (i.e. the influence of meteor detections in the back lobe of the radar) should be noted when the data technique is presented along with arguments that suggest the data is still worth considering.*

We have included a reference to Yukimatu and Tsutsumi (2002) in the instruments and data section when describing the derivation of neutral winds from this radar. Detailed discussion of the influence of the back lobes on the derived meridional winds is included in the discussions of the RMA regression results.

*The description of the mean and tide fitting technique should be expanded (P6577 L21 on). Something like "SVD was used to fit a wind field described by two horizontal components to the spatial distribution of radial velocities". This will help the reader to interpret the effect of radial velocity estimates from the back lobe.*

This section has been expanded as suggested.

*In describing the assumed meteor echo height (or prior to that point), the frequency at which meteors are being detected should be included.*

Between 1996 and 2006 the SuperDARN radar at Halley was sounding above 12 MHz for 85% of the time and between 10 and 12 MHz for 15% of the time. No soundings were made below 10 MHz. These statistics have been included in the instruments and data section.

*There could be more comparison of the mean winds and tides with results from other studies. Two papers already included in the reference list are candidates for this. Baumgaertner et al., (2006) contains results for the same latitude that could be included in many of the papers diagrams. Murphy et al., (2006) also contains tidal amplitudes*

The Baumgaertner and Murphy papers present data from MF radars (a different technique from those presented here). In addition, both papers show that tides determined from different sites vary due to the interaction between migrating and non-migrating



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components. Hence there is no value, within the remit of this paper, to compare the results from Halley with those of other sites. We therefore chose to compare our data with HWM-93 which provides a first order model for winds and tides to emphasise the degree to which the small separation of the two radars should affect the derived data.

*Given the poor quality of the correlations and the resultant doubt about the quality of the data, it is questionable whether the section on long-term trends contributes much to the field in its present form. With better knowledge of the causes of the poor correlations, it may be possible to apply data rejection schemes that strengthen the long-term trend analysis.*

This section is included as it provides and additional timescale on which the two radars are seen to vary, though see also the reply to comment 7 from referee 1.

Technical corrections

*P6577 L9 Insert "southern" before "spring"*

Amended.

*P6578 L16 onwards - As noted above, the poor correlation cannot be attributed to different gravity wave fields alone.*

Amended in the light of comments 4 and 5 from referee 1 and comment 1 from referee 2.

*P6579 L9 Suggest replace 'Though' with 'However" and add an 's' to 'cover' in the next line.*

Amended.

*P6581 L16 Suggest start new sentence at 'A similar'*

See comment 6, referee 1.

*P6581 L27 The January zonal component also differs.*



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See comment 6, referee 1.

*P6582 L14 on. The text from 'linear regression' onwards should be given a new sentence (or two).*

Amended.

*P6585 L22 Does 'meteor winds' refer to mean winds or tidal amplitudes?*

Added "tides derived from" for clarity.

*P6586 L17 Change 'This' to 'The present'*

Amended.

*P6587 L10 Insert 'approximately' before 'co-located'*

Inserted.

*Legends describing each line in Figures 4, 5, 6 and 9 would be desirable.*

The different lines have now been colour coded for clarity and are described fully in the figure captions.

*Fig 6a caption Delete 'equivalent' in L2 and insert 'for equivalent sampling volumes' before 'generated' in L3.*

Amended.

References

Arnold et al. (2003) - Ann. Geo. 21, 2073-2082
Baumgaertner et al. (2006) - JASTP 68, 1195-1214
Clemesha and Batista (2006) - JASTP 68, 1934-1939
Fraser et al. (2006) - JASTP 68, 317-322
Hibbins et al. (2006) - JASTP 68, 436-444
Hocking et al. (2001) - ASR 27, 1089-1098



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Murphy et al. (2006) - JGR 111(D23), D23104
Sokal and Rohlf (1981) - Biometry, 2nd ed. Freeman, NY
Thayaparan and Hocking (2002) - JASTP 64, 931-946
Yukimatu and Tsutsumi (2002) - GRL 29(20), 1981

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Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6573, 2007.