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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Seasonal variations in the horizontal wind structurefrom 0–100 km above Rothera station, Antarctica (67° S, 68° W)" *by* R. E. Hibbins et al.

R. E. Hibbins et al.

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The authors thank both referees for their constructive comments on this paper. In the light of these comments the major change to the manuscript is that sections 4 and 5 have been combined and, following a summary of the main features in the observed mean winds, the discussion has been subdivided into three subsections: 4.1 Comparison with other Southern Hemisphere sites, 4.2 Comparison with Andoya (69N,16E) and 4.3 Comparison with empirical models.

Specific comments by the referees are addressed below:

Referee #1 (Y. Portnyagin)

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1. “When comparing the Rothera MLT wind data with the data, obtained at the another longitudinally distinct Antarctic radar stations (Mawson, Molodezhnaya, Davis), the authors did not take into account that the measurements at these stations were carried out during the periods, which are not coincided with the period of the discussed measurements at the Rothera station. As a result the possible effects of the earlier detected strong year-to-year and long-term Antarctic MLT wind variations on the differences between the climatic monthly mean wind values would be discussed.”

A sentence highlighting this has been added to section 4.1 (“Although Portnyagin et al. (1993) have shown that significant year on year variability exists in the mean winds in the mesosphere and lower thermosphere the Rothera climatology presented here allows comparison with several longitudinally distinct sites”) and dates of observations and summaries of techniques used are added to section 4.1 and 4.2 to clarify the differences.

2. “Along with the presented comparison of the obtained experimental data with the HWM-93 model, which is longitudinally depended one, a comparison of the Rothera MLT wind data with the climatic zonally averaged updated prevailing wind model GEWM-I by Portnyagin et al. (Mesosphere/lower thermosphere prevailing model,Adv.Space Res., v.34,pp. 1755-1762, 2004) would be very useful.”

HWM-93 was chosen as the model against which to compare the data as it covered the whole altitude range (0-100km) in the spirit of this paper. However, a useful comparison can be made with the GEWM-I zonally averaged model and the height of the zero crossing of the summertime zonal wind in the MLT, and Section 4.3 now includes this and the reference.

3. “In the figs. 2, 6 and 7 the absolute values of the wind speed in relation to the different isolines would be shown.”

The Figures are now modified.

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4. “The discussion about possible role of an atypical gravity wave field around Rothera, which appeared in the Summary, deserves more words and have to be moved in the main part of the paper.”

A detailed discussion about the work of Ern et al (2004) is beyond the scope of this paper, the purpose of which is to present a climatology, and therefore this section has been removed from the paper for discussion elsewhere. However the height and extent of the summertime westward jet is a function of the gravity wave activity (Lindzen (1981), Garcia and Soloman (1985)), and at Rothera the jet is seen to differ significantly from other southern hemisphere sites and current empirical models. Thus, a sentence is added at the end of the discussion section highlighting this.

5. “There is the Section 6 (Zonal wind climatology). Where is the Section " Meridional wind climatology"? (cf. Fig.2).”

Both referees questioned this. The reason we did not include a whole atmosphere meridional wind climatology in this paper is that there was not a published meridional wind climatology from the PORTA rocket campaign to include with the balloon and radar data. This is because Lübken et al. (2004) have shown that the meridional component of their rocket winds was significantly biased by tidal influences, which are less strong in the zonal component. Without these rocket data the resulting climatology becomes more dependent on the HWM-93 model (in particular during the significant summer period) and was therefore omitted from the manuscript. The following sentence as been added to Section 2:”Lübken et al. (2004) do not publish a meridional wind climatology from the Rothera falling sphere data, since they showed that meridional winds (unlike the zonal winds) are significantly biased by tidal effects (see Müllemann & Lübken, 2005).”

Referee #2 (anonymous)

1. ”Comparisons with other Antarctic radars. The Rothera MF-radar results are compared to the observations made over Mawson (MF radar) and Molodezhnaya (meteor

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radar) described by Portnyagin et al. (1993) - but these are from 1984 - 1986 and 1967 - 1986, respectively (not mentioned in the text). There is a very real possibility that at least some of the differences identified are due to interannual variability. In this context it should be noted that Portnyagin et al. reported a significant year-to-year changes in annual mean wind over Molodezhnaya and other stations."

As discussed in the reply to comment 1 from referee 1 we have now expanded on section 4 and the references to clarify this.

2." Comparisons are made between meteor-radar and MF-radar observations and the MF-radar observations are taken to 99 km. It is now well known that there can be significant differences between observations made by the two techniques and that these differences vary with height and season (see for example Manson et al., Mesopause dynamics from the scandinavian triangle of radars within the PSMOS-DATAR Project, Ann. Geophysicae, 22 (2): 367-386, 2004; Hocking W. K. and T. Thayaparan, Simultaneous and colocated observation of winds and tides by MF and meteor radars over London, Canada (43 degrees N, 81 degrees W), during 1994-1996, Radio Sci., 32 (2): 833-865, 1997). In particular, MF radars appear to record weaker winds than other techniques at heights above Ÿ 90 km. This effect may well account for the structure of the summertime zonal winds of Figure 2, where the strong wind shear evident at lower heights declines above Ÿ 90 km. Here we should note that Arctic observations made by a number of meteor radars suggest that - at least in the Northern hemisphere - the summertime shear continues upwards and that the winds are therefore stronger as one approaches Ÿ 100 km. The reasons for such differences are poorly understood, but the authors should mention that such problems exist and carefully qualify their observations as a result."

The following has been added to Section 2: "Several authors have investigated inconsistencies between winds derived from MF radars and other techniques (e.g. Manson et al., 2004; Hocking and Thayaparan, 1997; Thayaparan and Hocking, 2002). Although MF radars are believed to underestimate wind speeds above ˜90 km (especially

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in winter) relative to meteor radars, and to suffer from group retardation (Namboothiri et al., 1993) and E-region echo contamination (Hocking, 1997) in summertime above ~95 km we have chosen to include data from the full altitude range returned by the radar". In addition the discussion now includes the following sentence: "although the apparent reduction in the magnitude of the summertime zonal wind shear observed above ~90km could be an artefact of the MF radar techniques as discussed previously", and the references updated.

3. "Comparisons are made with the Molodezhnaya results, I believe this system was a meteor radar operating without height finding. The winds recorded are therefore representative of the vertical distribution of meteors. Although this will be reasonably close to what would be observed with a height gate a few km deep centred near 95 km, there will inevitably be some differences. This should be mentioned."

A comment explaining this has now been added to section 4.1: "recorded between 1967 and 1987 with a meteor radar approximating to 95 km altitude".

4." Overall, this Section needs an expanded and more critical discussion."

Sections 4 and 5 reorganised as discussed above.

5."A zonal wind climatology is given in Section 6. Why is a meridional wind climatology not presented?"

See reply to comment 5 from referee 1.

6."In Figures 3 and 4, are the error bars the standard deviation or the error on the mean? For making comparisons with the model, the error on the mean would be more appropriate."

The standard deviation has been used, but we agree the standard error of the mean is more appropriate for model comparison and Figures 3 and 4 and their captions are modified accordingly.

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7. “The thin and sometimes overlapping lines of Figure 5 are rather hard to distinguish.”

Figure 5 has been reproduced with data from the different sources coloured differently, and the plot lines thickened.

8. “The final note in Section 7 about gravity-wave fluxes over the Antarctic peninsula needs to be expanded or removed.”

This, and a similar comment from referee 1, has prompted a rewrite of the discussion section as outlined in the reply to comment 4 from referee 1.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 4291, 2005.