Reply to Anonymous Referee #1

We would like to thank Referee #1 for his comments and the suggestions for improving the manuscript. Below we provide point by point answers to the referee's comments and questions.
The new version of the figures, including a new one (Fig 2) are provided in a supplement file. Note that a mistake has been corrected in Fig 7 (now Fig 8). Larger biases between the ECMWF and SMILES winds in the near meridional direction are seen above 0.1 hPa at the latitude 20N. The difference is consistent with the results in Fig 6 (now Fig 7).

Main comments are:
- more information about the zero wind correction should be given
- more information about the used ECMWF model versions should be given

Our answers to these points are given when these issues are addressed in the minor comments below.

1. p.32475, l.3: this statement is too general, wind observations from radiosondes cover altitudes up to about 30km suggest to add: "..., in particular at altitudes above 30km."

Done

2. p.32476, l.19: It is not true that all equatorial waves are "non-geostrophic motions"! There are also Rossby-type equatorial wave modes, and even the wind field of mixed Rossby-gravity waves and Kelvin waves is partly geostrophic, in particular for the low zonal wavenumbers (see Matsuno, JMSJ, 1966). It is however true that it is difficult to derive the larger scale wind fields using the geostrophic assumption in the tropics because the Coriolis parameter vanishes at the equator and the solutions become numerically unstable. This has also an effect on wind estimates and wind climatologies that are using the geostrophic assumption. See, for example, Fleming et al., ASR, 1990 (CIRA-86).

We changed the sentence *"However, ..."* as follow:

However it is difficult to derive the larger scale wind fields using the geostrophic assumption in the tropics because the Coriolis parameter vanishes at the equator and the solutions become numerically unstable (Hamilton et al., 1998; Zagar et al., 2004, Polavarapu, et a., 2005). For instance, significant differences are seen between wind climatologies derived from mass balance (Fleming et al., 1990, Randel et al., 2004).

Randel et al., The SPARC Intercomparison of Middle-Atmosphere Climatologies, J. Climate, 2004

Fleming et al., Zonal mean temperature, pressure, zonal wind and geopotential height as functions of latitude, Advances in Space Research, 1990

3. p.32477, l.11: LOS winds from MLS are derived not only at altitudes 80-92km. Results are shown for the altitude range 70-92km (see Wu et al., 2008). However the precision at altitudes below 80km is much worse.

We changed the altitude range to 70-92 km both in the text and in Figure 1.

4. p.32478, l.7: What do you mean by ±10◦ ? The explanation comes later in the manuscript, but here this causes confusion and could be mistaken as geographic latitude range. Maybe just omit. Or briefly explain here.

At this stage of the manuscript, we believe that a short indication that SMILES measured near the zonal and meridional directions is enough and we prefer to remove the information about the deviation angle range. In the next section the observation characteristics are given with more detailed (see following comment). We changed the sentence as follows:

Because of the ISS rotation during an orbit, components near the meridional and the zonal directions are retrieved between 30◦S-55◦N.

5. p.32480, l.5: Again, what do you mean by ±10◦ ? Please state more clearly that only orientations of the LOS are used that do not deviate by more than ±10◦ from the exact zonal and exact meridional direction, respectively. Because of the geometry of the instrument field of view in relation to the ISS orbit the LOS winds during the ascending (descending) portion of an orbit are almost in meridional (zonal) direction.

We changed the sentence with the one proposed by the referee:

"Because of the geometry of the instrument field of view in relation to the ISS orbit, the line-of-sight winds during the ascending (descending) portion of an orbit are almost in meridional (zonal) direction."

In line 9 we replaced the sentence *"Winds are retrieved near the meridional or the zonal directions between 30S and 55N"* by the new sentence proposed by the referee:

"Between 30◦S and 55◦N, the line-of-sight direction does not deviate by more than 10◦ from the exact zonal or meridional direction."

6. p.32480, ll.5–11: How large are the wind errors that result from not exactly meridionally or zonally oriented LOS? Is this error contained in the error budget of Fig.3 (discussion on p.32481)? Perhaps refer also to the discussion in Sect. 2.4.

In the manuscript we only show line-of-sight winds and when the observed winds are compared with the ECMWF winds, the latter are projected on the line-of-sight direction. This information is indicated in section 2.3 and to avoid any confusion it has been added in the captions of the relevant figures. Note that the error due to the line-of-sight angle is small enough to be neglected and it is not included in the error budget.

7. p.32480, ll.9–11: Would it be possible to derive zonal and meridional winds for the whole latitude range by introducing corrections for the rotation of the LOS direction? This would be an important information for future instrument design.

It might be difficult to have a LOS azimuth adjustable instrument. Especially on the ISS where the field-of-view is limited by the solar panels. The easiest way (more reliable ?) would be to use two perpendiculars line of sights, one looking forward and the other one looking backward. The two LOS can measure the same air mass with a small time delay (few seconds) and the meridional and the zonal winds can then be derived for any satellite orientation. As indicated in the last sentence of the conclusion, the optimal specification for a microwave wind instrument are under study. However such discussion are out of the scope of this paper.

8. p.32481: It should be mentioned that ECMWF operational analyses change from time to time, and that this has important consequences for the quality of ECMWF analysis winds. Please include the following information: There are two important changes in the ECMWF setup that are relevant for the SMILES data measured from October 2009 to April 2010:

(a) on 8-Sep-2009 ECMWF cy35r3 was introduced: In particular, the mesospheric winds were significantly improved by using a nonorographic gravity wave scheme (Orr et al., J. Climate, 2010). Maybe this is the reason why ECMWF winds are already in gross agreement with SMILES, even in the mesosphere. Comparison with previous model versions probably would have been somewhat worse. This shows the importance of SMILES-like observations as a reference for model data.

The following sentence line 26 has been added following the Referee information:
".. 3 September 2009". In particular the mesospheric winds were significantly improved by using a nonorographic gravity wave scheme (Orr et al., 2010)."

(b) on 26-Jan-2010 ECMWF cy36r1 was introduced: The main change is a general increase of the horizontal model resolution. In particular, the resolution of the deterministic forecast and analysis model was improved from T799 to T1279. Do you see any effects of this change when you compare to SMILES observations?

It is difficult to identify any improvements in cy36r1 with the current analysis. The differences between SMILES and ECMWF winds change from month to month due to changes in the atmospheric conditions (see Fig 14 in the supplementary file). For instance, the standard deviation and the bias look better in March-April than in the previous months but this effect is likely due to the fact that the velocity of the winds in the Northern hemisphere decreased in that period (Arctic vortex is disappearing).

A proper analysis would be to compare both versions of ECMWF with SMILES data over the same period. Such analysis is out of the scope of this paper.

9. p.32482, l.15: Does the zero-wind correction derived from equatorial observations also hold at higher latitudes? Is this zero wind correction also valid for zonal winds?

The zero wind corresponds to a daily averaged tropical "meridional" wind and it is subtracted for all LOS winds retrieved on the same day. We assumed that the zero-wind correction holds at all latitudes, which is not fully correct because we know that calibration errors are latitude dependent. However this analysis shows (Sections 3.1 and 3.2) that above 30-35 km the tropical zero-wind allows a good correction even a high latitudes. Below 30 km, the amplitude of the calibration errors and their latitude dependence are not well corrected (see in the large increase of the mean bias in Fig 8).

10. p.32483, l.10ff: For the zero wind correction using ECMWF winds may currently be the best way to do, and ECMWF winds in the mesosphere may have improved since the inclusion of a nonorographic gravity wave scheme. However the meridional winds in the mesosphere will still not be fully realistic. As far as I know they are not even validated so far. Therefore I wonder whether it would be possible to use zero winds derived at lower altitudes also for higher altitudes to avoid ECMWF winds above 50km. Please note that wind observations entering the ECMWF assimilation system are limited to altitudes of about 30km and below. Above this altitude winds are more or less the dynamics of a free-running model, in particular at low latitudes, as already stated in your introduction.

We do agree with the referee that the use of ECMWF meridional winds in the zero-wind correction is not fully satisfactory above 50 km. In the current version, the zero winds can not be extrapolated from lower altitudes by a simple linear function. The retrieval has to be improved (especially band A data). However, as indicated in the conclusion, new retrievals will be performed using measurements with improved calibration schemes (both for radiance and frequency). Improvements might be significant enough to try a new strategy for computing the zero-winds without using mesospheric ECMWF meridional winds.

11. p.32483, l.25: ERA-40 is based on a rather outdated version of the ECMWF model (the IFS version cy23r4 used from June 2001 until January 2002 with some modifications). Therefore ECMWF winds may have improved meanwhile. Nevertheless, the main findings by Baldwin and Gray (2005) may still be valid.

This information has been added in the line 28:
"... (correlation of 0.3 at 0.1 hPa with observations). Although this analysis is based on an outdated version of the ECMWF (version cy23r4), these results may still be valid and highlight the difficulties for reproducing winds in the tropics. Hence, the comparison … "

12. p.32484, ll.7–9: What is the averaging time period for the wind altitude profiles shown in Fig.5?

All the data when band A and B were observed simultaneously, have been averaged. The configuration A+B represents 1/3 of the total observations. This information has been added in Section 3.1 (p 32484 line 10):

"... where bands A and B were measured simultaneously are used. This corresponds to one third of the full dataset."

13. p.32485/6: the zero wind profile is derived how often? daily?

The zero wind profile is derived for every day. This information is given in Section 2.4 (p32482/15).

14. p.32486: What about tides and other waves that might not be represented correctly in ECMWF data, in particular in the mesosphere? These waves could also contribute to the standard deviation of the SMILES-ECMWF differences.

Tides and waves contribute to the differences between SMILES and ECMWF. The oscillations at 0.2 hPa seen in Fig. 6 (3rd panel) and 12 (Fig. 7 and 13 in the new version) are likely due to waves and tides but are not seen in the ECMWF winds (see text in p 32493/4). However we did not discuss the waves and tides in this paper since the current analysis is not suited for their study since longitudinal and local-time variations are not considered.

The text has been changed as follows:
"The mean differences in the mesosphere vary between +/-10 ms^{-1}. Mesospheric tides and waves may contribute to such differences. For instance Morton et al. (1993) has reported migrating-tides induced meridional wind oscillations above 60 km during the winter solstice and near the latitude of 20° in both hemisphere. The vertical wavelength was about 20 km and the amplitude 40 km below 80 km."

Morton, et al.: Global mesospheric tidal winds observed by the high resolution Doppler imager on *board the Upper Atmosphere Research Satellite, Geophysical Research Letters, 20, 1263–1266, 1993.*

15. p.32487: Does it make sense to calculate mean differences between ECMWF and SMILES averaged over November-April? There may be a seasonally dependent bias in ECMWF that should be larger when winds are stronger (DJF). This may also be important for meridional winds.

We agree with the referee that differences change with the months. However the first objective to this comparison was to infer the bias and the precisions of the measurements, and to do so, we wanted to cancel out as much as possible errors due to ECMWF itself. However as we can see in the additional Figure provided at the end of the supplement file, the main difference are seen all over the SMILES periods. For instance, the difference between SMILES and ECMWF zonal component show bias > 20 m/s at about 0.1 hPa, negative bias in the mesosphere at latitude >40N and bias ~5 m/s in the tropical mid-stratosphere.

16. p.32488, ll.4/5: "improve ECMWF winds... " Please be more specific! How do you think SMILES could contribute? Perhaps tune the model dynamics to match SMILES for the period of SMILES observations and then assume that this tuning also holds for other periods?

Or are you thinking of SMILES-like measurements that might become operational in the future and their winds could be directly assimilated into ECMWF?
Further, I think that not only in the tropics, but also in the mesosphere in the extratropics SMILES might be useful to improve ECMWF winds. There are already applications for data assimilation in the mesosphere, for example, Polavarapu et al., QJRMS, 2005 or Eckermann et al., JASTP, 2009.
By comparing to SMILES observations one could also think of a further improvement of the gravity wave scheme used in ECMWF.

The sentence has been changed in the text as follows:

"The large differences between the ECMWF and SMILES zonal-winds found in the tropics and in the mesosphere indicate that a future instrument similar to SMILES could significantly improve ECMWF analysis into these regions by assimilating the observations in the model. Although, the current SMILES observations cover a short period, some persistent biases in the analysis during the whole winter are clearly seen and SMILES could help to understand the origin of such biases. In particular, the observations can provide new constraint on the parametrisation of unresolved waves for the analysed winter and might be applied to other years. However such investigations are out of the scope of this analysis."

17. p.32491, l.16: At the stratopause (1mbar) the zonal wind in November is only 20m/s in Fig.11, not 40m/s. Same in April: only 20...30m/s, and not 40m/s.
Please check!

We agree with the referee and changed the text.

18. p.32492, l.25: The statement in this line is not generally true! The SAO amplitude at 60km is about

the same as in the NH stratosphere; "much smaller" is only valid for the SH stratosphere.
Please check!

We agree and changed the text:

“In the lower mesosphere (0.2 hPa), the measured wind SAO is in phase with the stratospheric oscillation and the amplitudes in both hemispheres are similar (~20 m/s).”

19. p.32493, l.4ff: The diurnal tide is aliased with a period of two months... This means that the substructures seen in the SAO at the equator (Fig.12) might be due to diurnal tides. For example, the diurnal westward tide has notable amplitudes also in the stratosphere (Mukhtarov et al., JGR, 2009).

We agree with the referee that tides should leave their signatures in the measurements. However it is out the scope of this paper to look for them. For this reason we have preferred to simply provide the information without showing them (see answer to comment 14).

Technical Comments:
1. p.32502ff: please use larger fonts for all figures, where possible: Done

2. p.32477: zonal-winds → zonal-wind: Done (line 1)
C120083. p.32477, l.25: at at → at : Done

4. p.32478, l.14: winds analysis → wind analysis: Done

5. p.32479, l.7/8: capitalization in "superconductor-Insulator-superconductor" looks strange

Capitalization as been changed as: “Superconductor-Insulator-Superconductor”

6. p.32479, l.23: three winds profiles → three wind profiles: Done

7. p.32480, l.13: semi → semidiurnal ?? : Done

8. p.32486, bottom: O3 lower and HCl upper panels ???

The senstence has been changed: “... from the O3 and the HCl lines (lower and upper panels, respectively) ...”

9. p.32487, l.5: ...as in the previous...: Done

10. p.32488: in November and February → from November to February ??? the latitude range is extended southward in Fig.9 over the whole period, please check! (coverage is always 30S-60N, only in Oct. and Apr. reduced to 20S-60N)

The sentence is unclear and as been rephrased by “In November and February, the observed latitude range is displaced to about 60S – 30N because of the maneuver of ...”
Line 19: “focus” is replaced by “show”

11. p.32489, ll.10/11: sounds odd, please check!

The sentence has been rephrased: “... are high westerlies during winter (e.g. in December and January) revealing the edge of the Arctic polar vortex.”

12. p.32492, l.3, suggestion: Southern (left column) and northern tropics (right column) are shown

We included the Referee suggestion.

13. p.32492, l.13: this statement is somewhat confusing, suggestion: "the variation range within time intervals as short as about 1 week"

We followed the Referee suggestions and changed the text.

14. p.32493, l.10: winds products → wind products ? Done

15. p.32494, ll.3/4: What do you mean by "...using the O3 line signal enhanced during night time."?

We changed the sentence as:
“using the nighttime O3 line signal enhancement due to the large chemical diurnal variation of O3 at altitudes between 0.01—0.005 hPa.

16. p.32496, l.21: doppler → Doppler: Done