Reply to comments from referee #1

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- blue: referee's comments
- green: author's replies

• **Comment on cross-referencing:** references to figures in the manuscript are named "Fig." whereas reference to figures attached to this document are named "Figure". The figure numbering is usually based on the original manuscript and Supplement, except in the case of citations from the reviewed version and new figures included during the review process.

The paper describes the oscillating modes of the middle atmospheric flow as measured with the ground based radiometer WIRA at different latitudes. Such observations are important since the instrument is sensitive to the altitude range 5 hPa to 0.03 hPa where few observations exist and none on a routinely basis. To my knowledge, WIRA is the first ground-based microwave radiometer designed for wind measurements and provide good quality data described in previous papers. It is the complement to the upper atmospheric observations derived from radar systems. The analysis presented in this manuscript shows the detection of well known atmospheric oscillation modes with periods near 5, 10, 16, and 25-50 days. The quasi-2day oscillations and the tidal oscillations are not detected because of the data daily sampling used in the spectral analysis. The results are a good demonstration of what can be done with this new system.

The manuscript presents important results that should be published but I think the discussion is too short to match ACP journal requirements. Therefore, I would recommend minor changes before publication. My main concerns are:

1) There are extensive theoretical and experimental studies related to the observed oscillations and their connections with atmospheric waves or atmospheric states. The reference to previous works is not enough. The main oscillation characteristics derived from the WIRA observations (latitudinal and seasonal variations, period variability, life time) should be compared with those from previous studies. I can not really figure out if WIRA observations are in agreement with what it is supposed to be known. Also they are some features in the plots that are not mentioned.
We agree that our discussion was a bit short and substantially extended it. We also added 10 new references. Moreover, many more comparisons to the outcomes of the studies in already cited or new references were incorporated to the

manuscript. As WIRA is the first instrument which is continuously measuring wind at these altitudes we had to rely on studies based on model data, on observations of wind at other altitudes (mainly mesopause region) or observations of middle-atmospheric temperature, geopotential height or the concentration of trace gases. Further theoretical literature about atmospheric dynamics has been added. For more details, please refer to the "Specific Comments" and the new version of the manuscript with marked-up differences to the discussion paper.

2) The impacts of the characteristics of the measurements and of the periodogram (spectral features broadening, time resolution of the periods, measurement vertical resolution, possible spectral artifacts) are not sufficiently taken into account in the discussion. - The spectral features seen in the periodogram are broadened by the analysis because of the limited lifetime of the oscillations ($\sim$ 30 days?) and of the spectral window (3T). For instance, the spectral broadening for a long-period oscillation ($T > 20$ days) should be large ($\Delta_T > 10$ days, FWHM). - The vertical wavelengths of the waves associated with some of the stratospheric oscillations are similar to the retrieval vertical resolution. The latter may have a significant impact on the results. - I believe that some spectral features discussed in the manuscript can be artifacts. If I am right, their interpretation has to be presented with more cautions.
The interpretation of the results has been revisited. Along with this task a more thorough assessment of the properties of the spectral method (peak broadening, artefacts, etc.) was carried out and comments added in the manuscript. Details are given in the "Specific Comments" section.

I think these comments are minor since they are simply a demand for more information and do not require any modifications of the data analysis presented in the manuscript. The details are given in the specific comments section herebelow.
Thank you very much for all the very interesting and constructive comments.

Specific Comments:

P35038, L11: Is the vertical resolution derived from the FWHM of the averaging kernels as explained in Rüfenacht2014? If yes I would expect such estimation to underestimate the actual vertical resolution because of the strong asymmetric shape of the averaging kernels and the presence of negative lobs. Should a better estimation of the retrieval vertical resolution be used?
Yes, we used the definition given in Rüfenacht et al. 2014. This is the most common definition of altitude resolution in microwave radiometry and is broadly accepted (e.g. see the textbook by Rodgers 2000 and numerous papers such as Forkman et al. 2003, Straub et al. 2010, Palm et al. 2010 and many more). From our point of view the averaging kernels are not particularly asymmetric in the altitude range judged as trustworthy (see definition in the manuscript). The slight overshooting to negative values is a common feature of averaging kernels from midcrowave radiometry (e.g. see previously cited references). Its influence on the width of the averaging kernel is judged to be small, therefore we do not see a need for defining another estimator for the altitude resolution.

The wind averaging kernels depends on the O3 abundance and tropospheric conditions. I am wondering if the change of the averaging kernels due to the seasonal change of these parameters may have enough impact on the retrieved wind profiles to make a spectral signature in the results?
It is correct that ozone abundance and tropospheric opacity have some effect on the wind averaging kernels. It can especially decrease the measurement response (i.e. the area under the averaging kernel) at high and low altitudes. Therefore data at these altitudes may reach the threshold to be judged untrustworthy and will therefore be removed from the spectral analysis. In this way, a temporal oscillation of O3 or the tropospheric water content may introduce time dependent data gaps at very high or low altitudes. However, as the spectral method can handle such data gaps this would not lead to oscillation artefacts in the retrieved wind field.
The effect of ozone or tropospheric conditions is not responsible for a biasing of the wind field as demonstrated in Rüfenacht et al. 2014. The impact on the vertical width of the averaging kernels (i.e. the vertical resolution of the measurement) is minimal so that additional contribution of the wind field of other altitudes can be neglected. Thus oscillations in O3 abundance or tropospheric conditions do not propagate to the wind field as an effect of changing averaging kernels.

P35039, L15: Why the authors use percentage to express the differences? I think it is better to express the differences between ECMWF and WIRA in term of velocity (m/s). For instance, the differences between the observations and ECMWF are expected to be larger in the Tropics (Reunion) than at higher latitudes (other stations) though the stratospheric wind mean velocity is smaller above la Reunion. Also it is interesting to compare the differences with the measurement errors which do not depend on the wind velocity.
AND: Is the statement "mesospheric zonal wind overestimated by the model ... " derived from Fig18 in Rüfenacht et al., 2014? If yes, it should be indicated that it is applicable to only mid/high latitudes sites and not for La Reunion (not given in Fig18). Note that wind measurements with JEM/SMILES (Baron et al., 2013, cited in the Supplements) clearly show a large underestimation of ECMWF forecast in the Tropical mesosphere. The data also shows the overestimation at higher latitudes such as that reported in Rüfenacht et al., 2014.
We have modified this part of the manuscript by omitting percentage indications and relating the agreement to the measurement error. We also restrict the statement of higher ECMWF wind speeds to mid and high latitudes: "A previous study revealed agreement within the measurement error between ECMWF's Operational Analysis and WIRA's wind measurements in the stratosphere, but demonstrated that the mesospheric zonal wind speed is generally significantly larger in the model for mid and high latitude stations (Rüfenacht et al. 2014)."

P35040, L1: Why a width of 3T? How does the width of the window compare to the expected lifetime of the oscillations? The spectral broadening of the spectral features induced by to the size of the window and of the oscillation life time should be discussed. I think it is relatively large (periods of 30 days are spread over a 10-20 days period-range depending on the oscillation lifetime) and should be taken into account in the discussion of the results. (see the supplement file uploaded with my report)

Most oscillations are short-lived and do rarely persist for more than 3 periods as can be inferred from Fig. 4 in the discussion paper. We have also added some numeric values for the longest lifetimes of the observed oscillations to the manuscript in reply to one of your comments below (for the extra long period oscillation 80 days in zonal wind over Bern, 50 days over La Réunion, and 30 days in meridional wind over La Réunion; for the quasi-16-day oscillation over La Réunion 16 days in zonal wind and 16 days in meridional wind). Therefore when observing one specific occurrence of an oscillation a longer time window is not adequate as it would yield lower oscillation amplitudes than the actual oscillation has because periods where the oscillation is not present would contribute to the average. On the other hand a short window increases the frequency uncertainty of the oscillation peaks (broadening). In our eyes a window of three periods is a good tradeoff between accurate estimation of the amplitudes of short-lived oscillations and acceptable broadening of the peaks.

P35040, L10: add "s" to "more detail"
Modified in this sense

P35042, L09: The Figures S2 and S3 should be added to the main manuscript. They should be used to discussed the impacts of the retrieval vertical resolution and missing data in the periodogram. For instance, the 5 days oscillation in the Meridional wind above Provence (Fig 3, mid-stratosphere) is strongly reduced in the unaltered ECMWF data (S2). Is the Fig3 spectral feature an artifact due to missing data? If yes, this should also be the case of the measured one (Fig2)? The mesospheric 10day oscillation in the Provence meridional wind (S2) vanishes in Fig3. Is it due to the measurement vertical resolution? I am surprised to see that in general upper stratosphere and mesospheric oscillations are much stronger in Fig3 than in S2. Altering the data should decrease the oscillation amplitude?
Thank you for pointing this out. We agree and have included figures S2 and S3 to the main article and provided some comments for the interpretations of the figures for "WIRA", "ECMWF at WIRA" and "ECMWF": "The analysis for the scenario ECMWF at WIRA shown in Fig. 3 should yield identical results as presented in Fig. 2 if the measruements are error-free and the atmosphere is realistically represented by the model. In this case WIRA and ECMWF would agree that the periodograms of the real atmosphere correspond to Fig. 4." (p. 3043, l.23). We also provided a new figure showing the temporal evolution of the oscillations based on unaltered ECMWF data in the mesosphere at 0.05 hPa (Fig. S12 in the Supplement) which can also help in the interpretation of the differences between Figure 3 and S2 of the original submission.
For example, a strong 10-day oscillation features appears in the meridional wind at 0.03 hPa over Provence in December 2012, right before the onset of the major SSW in January 2013. It is clear that this oscillation causes a relatively large 10-day signal in the temporal average (of the unaltered time series). However, from Fig. 1 one can infer that meridional WIRA data are very rare at this altitude in December. Consequently also "ECMWF at WIRA" contains data gaps for this time where the strong oscillation is present. Therefore, in contrast to the unaltered ECMWF data, one cannot see a strong 10-day oscillation in the upper mesosphere in the temporal average of "ECMWF at WIRA". We have clarified the properties of the scenario "ECMWF at WIRA" in the manuscript

in reply to your next comment and a request by referee #2 (please see below). Vertical resolution does not play a role here. The limited vertical resolution of WIRA can, however, reduce the amplitude of oscillations with a vertical extension in the range of or smaller than the vertical resolution.
The 5-day oscillation in stratospheric meridional wind for WIRA might be an artefact triggered by the small data gap at the beginning of January 2013 as it is visible for the altered ECMWF time series whereas it is not present for the original. The limited altitude resolution cannot be made responsible for the appearance of this feature as there is no oscillation signal at other altitudes which could contribute to the weighted average described by the averaging kernels. We are sorry that we did not comment on this fact in the interpretation. We would like to point out that the 5-day oscillation visible in Fig. 2 (WIRA) in the other data sets, except meridional wind in Provence, does not seem to be an artefact of the measurement setup as it is absent in both Fig. S2 and Fig. 3. Therefore these occurrences seem to be real differences between WIRA observations and ECMWF model data. We added information about the 5-day wave to the manuscript at P35043, L13-15 (see below)
The fact that mesospheric oscillations are generally stronger in Fig. 3 when compared with Fig S2 is due to the seasonality in the tropospheric opacity (due to the water content). The consequence is that during the phases of low opacity (in winter) the better measurement conditions cause the altitude range of trustworthy data to extend to higher altitudes. Therefore the temporal averages at high altitudes is strongly influenced by winter data and only very weakly by summer data (because data gaps are introduced at times and altitudes where the measurements are judged untrustworthy). As in the scenario "ECMWF at WIRA" data gaps are added to ECMWF at the same altitude/time points the same is true for Fig. 3. Winter is also the season with enhanced oscillation activity (also in the mesosphere as seen from the new Fig. S12 in the supplement). Therefore, an average that runs mainly over winter data (Fig. 3) yields a higher amplitude than an average which also considers summer data (Fig. S2). The statement at p.35042, l.12 on the seasonality of WIRA's altitude range has been clarified (see reply to your next comment).
Please note that introducing data gaps will not necessarily reduce the oscillation signal as our method does not interpolate the gaps but rather treats them as missing values. Moreover, the limited altitude resolution might indeed increase mesospheric oscillation amplitudes rather than weakening them because the usually stronger stratospheric oscillation signal might be averaged into the mesosphere.

P35042,L16: "seasonal averages" means that all seasons are averaged which is not the case since the mean periodogram is more representative of winter conditions.
This part of the manuscript has been adapted in order to also satisfy the request for more detail by referee #2. The term seasonal average is not used anymore: "From Fig. 1 one can identify levels where trustworthy measurement data are predominantly present during winter, because the generally wetter summer troposphere alters the signal-to-noise ratio of the observation setup as a consequence of a stronger attenuation of the middle-atmospheric radiation. At these altitudes the oscillation amplitudes should thus not be interpreted as averages over the entire duration of the campaign."

P35042,L21: The 50day period is also a systematic feature in the results. I would expand the period range to 20–50 days and indicate that 50 days is the upper limit of the period estimation.
We have modified the manuscript in this sense.

P35042,L23: The limitations due to the spectral analysis and measurement characteristics should be taken into account in the discussion of the quasi 30day oscillations. For instance separate modes such as 30day and 50day periods may overlap because of the spectral broadening and be seen as single "blob" with period ranging from 20 to above 50 days (except for the zonal wind above Provence).
We have tested the spectral broadening of the oscillation peaks by our method. Results for monochromatic input signals are shown in Figure 1. 30 and 50-day oscillations could indeed overlap with this method as seen in the left panel of Figure 3. At the risk of generating stronger artefacts due to the sharp cutoff at the window edges we have also used our method with a rectangular window instead of a hamming window. Rectangular windowing has the effect to reduce spectral broadening as shown in Figure 2 because data points away from the centre of the window are considered with full instead of reduced weight. Rectangular windowing allows to separate peaks at 30 and 50 days as shown in the right panel of Figure 3. We also applied the rectangular window method to the data set of wind observations. However, this did not lead to a separation of oscillation peaks at 30 and 50 days or other peaks. We therefore conclude that the extra-long period oscillation is not generally originating from two different oscillation peaks with periods differing by more than 20 days.
We also added a comment on the limited vertical resolution and its effect on the altitude-dependent periodograms to section 4.1 of the manuscript (where the new discussion about the altitude dependence of the periodogram was introduced, see reply to your comment concerning P35043, L16): "In the interpretation of Fig. 2 we should keep in mind that the limited vertical resolution of WIRA, which lies around 12 km (i.e. 0.75 pressure decades) at these altitudes, may vertically smear out the oscillation peaks."

P35043,L5: The discussion about the long oscillations is too short. There are clear features in Fig2 that are not mentioned. Mid-latitude oscillations between 20-35 days seems to expand from the mid-stratosphere to the top of the retrieval range (mid/upper mesosphere) while the oscillations larger than 35/40 m/s are blocked at 0.02 hPa. At high latitudes, the oscillation is predominant in the lower mesosphere with a period very close to 27 days but it is not seen in the stratosphere. Are these behaviors compatible with what it is expected? More references about studies on 27day oscillation and more generally those describing periods between 20 and 50 days should be provided (the one provided in the manuscript is not enough). (e.g, Huang et al., observational evidence of quasi-27-day oscillation propagating from the lower atmosphere to the mesosphere over 20N, Ann. Geophys., 33, 1321-1330, 20, 2015, Fedulina et al., Seasonal, interannual and short-term variability of planetary waves in Met Office stratospheric assimilated fields, Q. J. R. Meteorol. Soc., 2004).
Please note that the aim of this publication was not to give a comprehensive analysis of the sources and mechanisms of any individual wave and oscillation

in the period range between 5 and 50 days. It is rather a first paper showing what kind of studies can be made with data obtained from the novel technology of wind radiometry and how the observations compare to ECMWF to get a first idea of the quality of the middle-atmospheric wind field in ECMWF (and of the wind measurements). It would be well beyond the scope of this study to investigate details of the forcing of the extra-long period oscillation because, as mentioned in the manuscript and confirmed by other studies, the influence of the solar rotation on extra-long period oscillations is not direct and might depend on many other factors.
Thank you for the hints towards additional literature (the paper by Huang et al. was published (30 October 2015) only after our manuscript had been submitted to ACPD (6 August 2015)). We have now integrated these two studies and extended the discussion of extra-long periods: "... are often discussed in the context of the modulation of the solar forcing with the rotational period of the sun (e.g. Fedulina et al., 2004; Huang et al., 2015)" and further "Huang et al. (2015) indicate that their observed extra-long period oscillation might be an atmospheric normal mode and that it may be indirectly introduced by the modulation of tropospheric convective activity with the solar rotation period. Fedulina et al. (2004) report a modulation of the 5-day wave amplitude with a period of 25 to 35 days but point out that a correlation with solar activity might appear by coincidence regarding the considered time scales."
We have to be careful not to conclude from one special case to generality. For example, when considering Supplementary Figs. S5 or S6 one can see that also periodicities larger than 35 days can extend over a large range of altitudes. From Figs. S4 and S6 it is obvious that also at high latitudes the oscillation period is not always close to 27 days. We included one more link to the manuscript (p. 35043, l.20) to these supplementary figures to prevent the reader from drawing conclusions too rapidly: " This hypothesis is supported by Figs. S4 to S7 showing ECMWF data for more extended time intervals at the campaign sites."

P35043, L13-15: The 5-day oscillation of the meridional wind above la Reunion is more significant (alpha near 0.01, white contour) than above Provence (alpha $>$ 0.1, grey contour). Is alpha $>$ 0.1 a reliable value? Can we trust a large peak but with low significance?
There is a confusion between Provence and La Réunion in our manuscript. We are extremely sorry for that and thank you very much for pointing it out! We intended to write that the 5-day oscillation was present in La Réunion meridional wind and that it might be present also for Provence but with low significance. We are convinced that α is the better indication for a reliable oscillation detection than the amplitude. In Provence the 5-day oscillation occurs close to the sudden stratospheric major warming event, when the variability in the atmosphere is high. A strong oscillation peak overlaid to a "noisy" background can be less significant than a weaker oscillation overlaid to a almost constant background wind field. Therefore the peak on La Réunion is more significant. We have more trust in a peak with a lower alpha value and rather than in a peak with high amplitude but higher alpha. As discussed in reply to a previous comment the 5-day oscillation in the Provence meridional wind is believed to be an artefact from data gaps at an unfortunate moment.
The manuscirpt has been modified to: "A quasi 5-day wave is observed in WIRA's zonal wind measurements for Bern and Sodankylä, and for the zonal

and meridional winds on La Réunion. The 5-day signal in the meridional wind in Provence has lower significance and seems to be an artifact of the measurement situation as it is also present in Fig. 3 showing "ECMWF at WIRA" data but not in the periodogram of the unaltered ECMWF data in Fig. 4. It might originate from the small data gap at the beginning of January 2013 (see Figs. 5 and 6) at a time of high variability due to a major sudden stratospheric warming."

P35043, L16: Over la Reunion, the zonal wind oscillations with periods larger than 10 days vanished in the mesosphere. Is it expected based on other radar and satellite measurements or is it a lack of measurement sensitivity/resolution that could explain the oscillations decrease?
Lower oscillation amplitudes in the mesosphere can indeed be expected from theory and observations. We added the following sentences to the manuscript at p. 35043, l. 20: "The reduced wave activity in the mesosphere, particularly above 0.1 hPa, may be explained by planetary wave breaking in the stratosphere (e.g. McIntyre and Palmer, 1983; Brasseur and Solomon, 2005). Interestingly this consideration also applies to the extra-long period oscillations what is in line with the periodograms of geopotential heights from MLS at mid-latitudes presented by Studer et al. (2012). In the interpretation of Fig. 2 we should keep in mind that the limited vertical resolution of WIRA, which is around 12 km (i.e. 0.75 pressure decades) at these altitudes, may vertically smear out the oscillation peaks.
The only major exception to the quiet mesosphere in Fig. 2 is the 27-day peak around 0.1 hPa in the periodogram for Sodankylä. This oscillation can probably be regarded as a special case as it occurs in the vicinity of the major sudden stratospheric warming event of January 2012 as seen from supplementary Fig. S12 which displays the oscillation activity at 0.05 hPa"

Similarly, Day et al. 2012 (cited in the manuscript) clearly shows a 16-day signal in winter mid-latitude at high altitudes. In Fig.2, above Bern and Provence, the 16day oscillation signal strongly decreases at 0.1 hPa and increase slightly again at the top of the retrieval range. I have the same questions as previously for La Reunion site.
After the adaptations to the previous comment the following statement has been added to the manuscript: "Although based on very few data points, the slight increase near the 16-day periodicity at the very top of the retrieval range might be understood as an influence of the strengthening of this signal in the MLT region reported by other observational studies (e.g. Williams and Avery, 1992; Day et al., 2012)." (The abbreviation MLT is explained in the introduction)

My general feeling on this section is that the behaviors of the 5/10/16day periods should be described in more detailed and, their main characteristics should be compared with previous studies in the middle and upper atmosphere.
We extended the discussions in the subsections 4.1 and 4.2 and added comparisons to previous studies (altitude dependence, duration of the oscillations, seasonality, SSW's ...) at various places in the text. Please see the version of the manuscript with marked up modifications uploaded along with this document.

P35043, L24: This result is compatible with other measurements and theoretical studies. Previous works should be cited. (e.g., Fedulina et al., Seasonal, interannual and short-term variability of planetary waves in Met Office stratospheric assimilated fields, Q. J. R. Meteorol. Soc., 2004, ...).
The following sentence has been added: "It (the hypothesis of seasonality) also confirms previous studies based on observations or assimilated model data (Hirota and Hirooka, 1984; Hirooka and Hirota, 1985; Day et al., 2011; Fedulina et al., 2004)"

P35044, L6-10: As already mentioned, the comparison with results from other observations should be improved.
We extended the discussion in subsection 4.1 (and 4.2) and added comparisons to several other studies. Please refer to the new version of the manuscript with marked-up differences to the discussion paper.

P35045, L20: The interpretation of the spectral features is too fast (I don't say wrong). The period variation (35-25 days) is in the same order that the spectral broadening (period resolution (FWHM) is $\sim 10 - 20$ days for a period of 30 days). The effect has to be taken into account in the discussion.
We have investigated the broadening of oscillation peaks in reply to your comment to P35042, L23 (please see above for details). The period variation is indeed in the range of the spectral broadening. The period variation, however, does not seem to be a random feature as it monotonically decreases over three oscillation cycles. A similar decrease in the oscillation period could also be seen in the analysis with rectangular windowing (please also refer to the comment on P35042, L23). We have modified the manuscript to: "... between the different campaigns. It can even vary within a single occurrence of the oscillation as seen in the example of Bern where the period decreases from 35 to 25 days between December 2010 and March 2011. A 10 days period change is at the limit of the spectral resolution of our analysis method for this long periodicities. Nevertheless it may be interpreted as a real signal, not only due to the monotony of the decrease, but also in accordance with an additional check using our spectral method with rectangular instead of Hamming windowing in order to improve the spectral resolution (not shown)."

P35045, L24: The 16day period is too quickly attributed to atmospheric wave. The period resolution has to be taken into account (as stated in my previous comment). Also the 16day oscillation signature can be reproduced as an artifact at the beginning and the end of a long-period monochromatic oscillation event. The authors should check if such artifacts can explain the spectral signature seen in their observations. (see the supplement file I uploaded with my report).
We assumed that the long period oscillation builds up and vanishes smoothly. In this case no strong 16-day artefact should be produced. In your simulation the sharp edges at the start of the oscillation (around day 70 in column c) or at the termination of the oscillation (around day 100 in column d) trigger a strong 16-day feature but the smooth termination in column c and the smooth initiation in column d do cause not cause significant artefacts. That an increase of randomly distributed data gaps generally increases noise at short periodicities is clear to us. But this should happen independently of the presence of a 30-day oscillation. Moreover the assumption of 20% randomly distributed data gaps

does not correspond to the reality at stratopause level as seen in Fig. 1 of the manuscript.
If, however, the atmospheric 30 day oscillation is abruptly initiated or terminated an artefactic feature at 16-days cannot be excluded. We commented on this possibility in the manuscript: "... the strongest 16-day amplitudes are observed near the initiation and the termination of the extra-long period oscillation. However, it should be noted that if the extra-long period oscillation is abruptly initiated or terminated, the 16-day signal could be produced as an artifact of the used spectral method as simulations showed. Whether a real 16-day wave is present and whether the two oscillations are linked in some way will have to be verified in further studies." Thank you for drawing our attention to this possibility.

P35046, L1: Note that if the 16day spectral features are artifacts, they are still a good indication of the beginning and termination of the long-period oscillation event. A value of the measured oscillation lifetime should be provided for Bern and La Reunion (it is difficult to infer it from the plots) and compared with other studies.
The first part of your comment has been treated in the previous reply. For the second part, we added lifetimes for the 30 and 16 day periodicity: "In the Bern and the La Réunion time series the strongest 16-day amplitudes (lasting for about 1 period) are observed near the initiation and the termination of the persistent extra-long period oscillation with a duration of 80 and 50 days, respectively. The duration of the presence of these oscillations is comparable to the results for mesopause wind presented by Luo et al. (2001)."

P35046, L21-26: "... extra long period (20-40 days)" → (20-50 days)
Modified.

P35046, L26: The 16day spectral feature might be described with cautions if the authors agree with my comment in the previous section.
We have modified our statement: "Enhanced quasi 16-day oscillation activity has sometimes been detected in the vicinity of strong extra-long period oscillations. A more extended study would however be needed to establish the origin of this signal and to uncover a potential link between the quasi 16-day wave and the extra-long periodicities. In addition to the extra-long period oscillations, normal modes with periods near 5, 10 and 16 days are present in our observations."

Supplement TextS1, second paragraph: HRDI has also measured wind in the stratosphere over a long period (∼ 10 years). The observations started from ∼ 30 km (e.g., Ortland D. A, Rossby wave propagation into the tropical stratosphere observed by the High Resolution Doppler Image, GRL, 24, 16, 1997)
Thank you for pointing this out. We modified the sentence on HRDI: "Previously, mesospheric observations down to 65 km and stratospheric daylight wind observations up to 40 km had been performed by HRDI on UARS (Hays et al., 1993; Ortland et al., 1996)." We used another reference here, focussing more on the instrument.

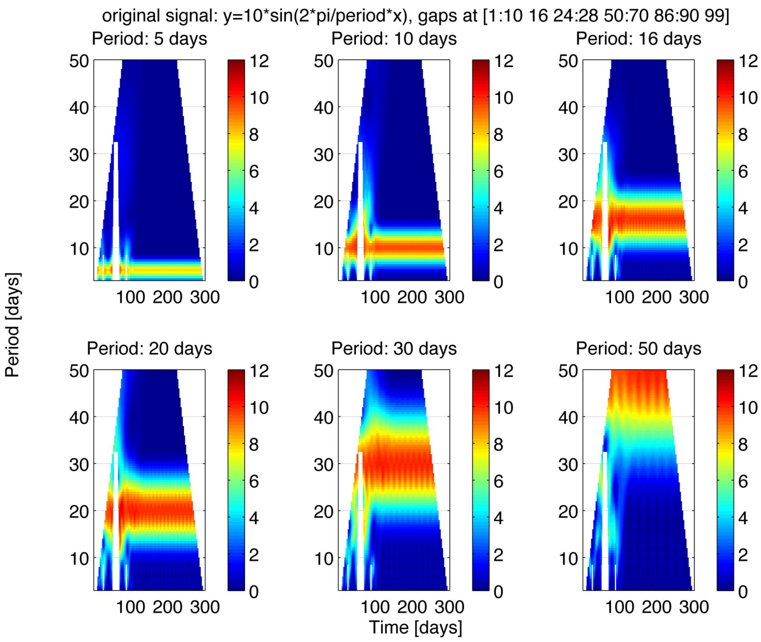

Figure 1: Reconstruction of synthetic monochromatic oscillation signals with the spectral method used for the analysis published in the manuscript (Lomb-Scargle with Hamming windowing, window width 3 periods).

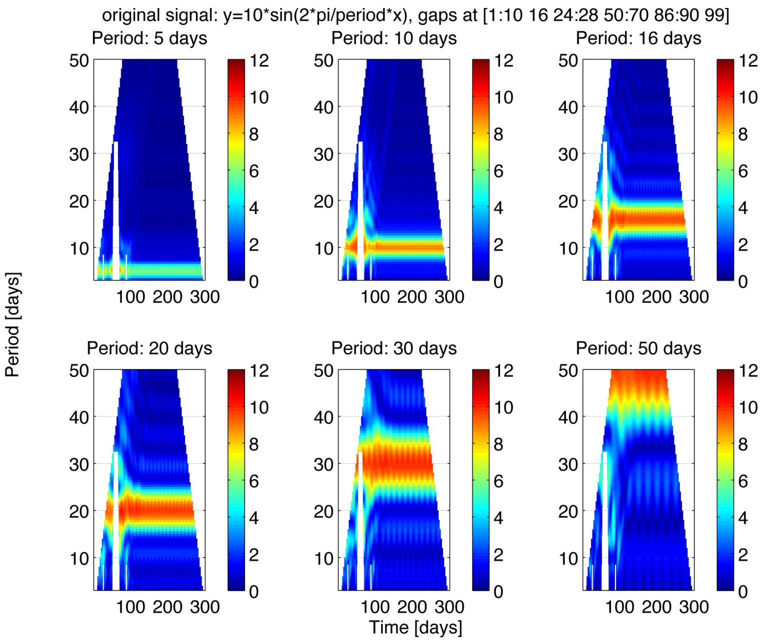

Figure 2: As Figure 1 but with rectangular windowing instead of Hamming windowing.

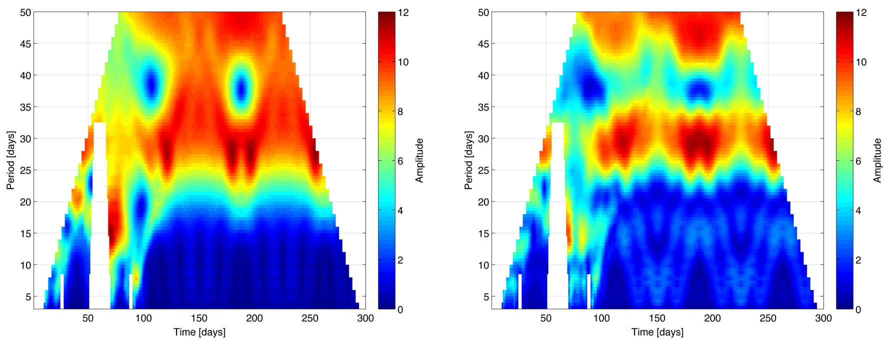

Figure 3: As Figures 1 and 2 but for the reconstruction of a superposition of a 30 and 50-day oscillation both with oscillation amplitude 10. Left panel: reconstruction with the method as used in the paper (Hamming windowing); right panel: reconstruction using a rectangular window function.