Response to reviewer #1

We thank the reviewer for the useful suggestions that significantly helped to improve the paper. We have modified the manuscript following most of the suggestions. Specific response to the respective comments is given below. The reviewer´s comments are repeated in italics.

The manuscript will be mostly acceptable as it is except for some parts where the results do not seem statistically significant to me as shown below. I hope that the authors modify the manuscript according to these comments.

P9635 lines 26-28 Using the meridional component for the period determination is reasonable. But aren't there any cases where estimated periods are different between the two components?
Yes, there are differences at times, but most cases with strongly varying zonal and meridional amplitudes are only found if amplitudes are small. We added a further description (lines 106-110): "This is also justified, because for large amplitudes the period difference is generally small, and < 2 h in about 75% of all cases with large amplitudes. Hence, the underestimation of zonal amplitudes is small and, as will be described in Section 3.1, the zonal amplitude is smaller than the meridional one even when using QTDW periods that are based on a periodogram analysis for the zonal component."

P9638 line 28- The three maxima do not seem statistically significant except the one around 47-48hrs. The number of samples at 42-43hrs is about 60 indicating the error bar is 7-8 counts (=sqrt(60)) and the peak is within the error. The same thing applies to the maximum at 50-51 hrs where the error bar is about 9 (=sqrt(70)).
You're right. We inserted a comment about significance on line 195: " Furthermore, we find a clear maximum at 47 − 48 h and two secondary maxima at 42 − 43 h and 50 − 51 h. The latter two maxima are not significant in a statistical sense, but they, together with the primary maximum, correspond to the three groups presented by Malinga and Ruohoniemi (2007). "

P9642 Section 3.3 The error bars (SDs, presumably) in Figure 10 are significantly large compared to year-to-year variations, except F10.7. I suspect that the correlations shown in this section are even less statistically significant than the authors describe. On the other hand, I think that confidence intervals defined as SDs/sqrt(number of independent samples) would be more suitable for the purpose instead of SDs. As the number of independent samples is about 10-11 (4 months/11days), the confidence intervals are estimated to be roughly one third of the corresponding SDs. They are, however, still large and the year-to-year variations are mostly within the confidence intervals. If the four heights (I guess they are independent) are further averaged, the confidence intervals will become about a half, then the following correlation analysis can be statistically meaningful and can appeal more to the readers. Although this comments will not affect the last part of the abstract anyway, I hope that the authors modify the manuscript considering more about the statistical significance.
In order to improve the statistical meaning we now use the standard error, given by the standard deviation devided by the square root of independent samples (123 days/11 days=11). However, averaging all altitudes is not helpful; they are not independent because the vertical wavelength of the QTDW is larger than the vertical range of observations. The description is added in Fig. 10.

P9636 line 18 Eq 1 seems wrong. Shouldn't it be sqrt(Az^2+Am^2) ?
Thanks for this hint.However, acccording to the comment of Referee #2, we deleted this formula as it is trivial. (l. 130)

P9639 Eq. 2 When the difference between Vz and Vm is small, this expression will be suitable to express the relative difference in %. But since the actual difference is a lot more than a few tens of %, values obtained using Eq 2 can be misleading. For example, when Vz is a third of Vm, delta_V = -50%. When Vz=0, delta_V=-200% Some additional explanation will be necessary for the Eq 2.

Actually, if Vz is 1/3 of Vm, delta_V = -100%. The value gives the difference relative to the mean amplitude. The same has been applied by Jacobi (2012), reference is inserted in line 200. A further explanation is added to improve the understanding but we would like to keep this quantity as it is.

line 26 This is the first time that the expression 'SD' appears in the manuscript. This abbreviation should be used after it is defined here.

Thank you for that hint. We removed the acronym because standard deviation is not used too often. (ll. 221,222 and Figures)

P9640 lines 16-18 It is not clear why a Weibull distribution is assumed.

We were looking for a probabilty density function (PDF) that is skewed and which starts at value 0 in order to present the mode. Thus, Weibull seemed to be good. Now, we quantified that and took a PDF that is best fitting according to several statistical hypothesis tests (Kolmogorov-Smirnov, Anderson-Darling and Chi-squared) and exchanged the Weibull fit by a Lognormal function. Explanation is given in lines 238-240.

P9641 lines 6 and 9 What kind of 'low pass filter' is applied? No expression as 'low pass filter' is seen before this paragraph. Is it the 11 day fitting?

Thank you for the hint.We used a Lanczos low pass filter with 30 weights and a cut-off period of 20 days. A respective sentence is added. (ll. 253-254)

line 13 Is 'the maximum of the low pass filter was ...' meant to be 'the maximum of the low pass filtered values was ...'?

Corrected, thanks. (l. 261)

P9642 lines 19-20 'gravity interactions reach to ...' This sentence does not make sense.

Grammar is corrected, "to" was removed and "gravity *wave* interactions" was inserted. (l. 289)

Figure 7 In the top of the right panel, the blue and green symbols are misplaced.

Thank you, Fig. 7 is corrected.