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Comment

## ***Interactive comment on “Meteor radar quasi two-day wave observations over 10 years at Collm (51.3 N, 13.0 E)” by F. Lilienthal and C. Jacobi***

**Anonymous Referee #1**

Received and published: 6 May 2015

————— General comments —————

‘Meteor radar quasi two-day wave observations over 10years at Collm (51.3N, 13.0E)’ by F. Lilienthal and C. Jacobi is a compact and well-written paper on the so-called quasi two-day wave in the northern high latitude, and will be suitable for the publication in ACP. The results shown in Figure 9 are especially interesting, where a more quantitative investigation will be necessary in the future though. 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.

————— Specific comments —————

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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?

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 ( $=\sqrt{70}$ ).

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  $SDs/\sqrt{\text{number of independent samples}}$  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.

————— Technical corrections —————

P9636 line 18 Eq 1 seems wrong. Shouldn't it be  $\sqrt{A_z^2 + A_m^2}$  ?

P9639 Eq. 2 When the difference between  $V_z$  and  $V_m$  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  $V_z$  is a third of  $V_m$ ,  $\Delta V = -50\%$ . When  $V_z=0$ ,  $\Delta V=-200\%$  Some

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additional explanation will be necessary for the Eq 2.

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.

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

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?

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

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

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

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 9631, 2015.

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