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Interactive comment on "Solar 27-day signatures in standard phase height measurements above central Europe" *by* Christian von Savigny et al.

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Reply to comments by reviewer #1

Note: our replies are boldfaced

Reviewer comment: This is an interesting and well-designed study of 27-day solar UV-induced variations of "standard phase height" (SPH) measurements in the ionospheric D region. While the observed inverse correlation of SPH with 27-day variations of solar EUV flux and the agreement of derived sensitivities with those obtained on the 11-year time scale are consistent with a purely photochemical mechanism (enhanced photo-ionisation of NO leading to lower effective reflection heights), a number of characteristics of the response are not consistent with such a mechanism. This leads the

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authors to further analyses identifying “non-trivial dynamical effects” and “clear evidence that planetary waves are associated with quasi 27-day periods”. After revisions in response to comments by the other two reviewers and by Jan Lastovicka as well as my comments, publication is recommended in ACP.

Reply: We thank the reviewer for his/her encouraging comments.

(1) One major disagreement with expectations for a purely photochemical origin is the fact (lines 358-360) that “the minimum in SPH precedes the maximum in solar forcing by a few days”. This statement agrees with the phase shift listing in Table 1, which shows shifts ranging from -2 to -4 days with no apparent dependence on solar activity level (F10.7 flux threshold). The negative phase lag of a few days can also be discerned in Figure 4, which shows the superposed epoch analysis (SEA) results for all available data (584 epochs). However, this is not so clear from the SEA results for low solar activity periods. Looking at the smooth red curve in the lower panel of Figure 10, the minimum in the SPH anomaly shows no significant negative lag when epochs with F10.7 less than 130 sfu are considered. Please explain.

Reply: The reviewer is right and the phase lag for solar minimum is smaller, and there may be no phase lag at all. We currently don’t have a plausible explanation for this behavior and we do not claim to have one. We can’t either say with certainty, why the minimum in SPH occurs a few days before the maximum in solar activity, if the entire time series is analyzed. We believe that non-trivial dynamical effects are the cause of these phase lags. We added a brief statement to section 4.2 of the manuscript. We would like to point out that the phase lags listed in Table 1 are not inconsistent with the lower panel of Figure 10. Table 1 shows the analysis results for epochs with solar activity exceeding a certain F10.7 cm flux threshold, i.e. the case that solar activity is lower than a certain value is not shown in the Table.

(2) A second major disagreement is that (lines 361-362) “not only the SPH sensitivity

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to solar forcing is larger for periods of low solar activity, even the amplitude of the potential solar 27-day signature is larger during solar minimum". The authors go on to note that a model study by Gruzdev et al. (2009) did find increased sensitivities with decreased forcing but did not find an amplitude increase with decreasing forcing. The main evidence for an amplitude increase seems to come from a combination of Figure 10 (showing the SEA results for high and low solar activity epochs) and Table 1, which lists the results as a function of the threshold value of F10.7 used to select the epochs that are analyzed. The lower panel of Figure 10 (F10.7<130 sfu) does indeed show a larger amplitude of the 27-day SPH variation than the upper panel (F10.7>130 sfu). However, this difference is not so clear from Table 1. The top entry corresponds to all data (F10.7 threshold of at least 60 units, 584 epochs) and gives an amplitude of 47.8 meters. The eighth entry corresponds to all data when F10.7 was at least 130 sfu (303 epochs) and gives an amplitude of only 31.5 meters, which is roughly consistent with the upper panel of Figure 10. However, the next entry in the table corresponds to data when F10.7 was at least 140 sfu (267 epochs) and gives an amplitude of 40.5 meters, considerably larger than that when 130 sfu is used as the threshold and maybe not significantly less than the 47.8 meter value obtained when all data are considered. So, I would ask the authors to re-examine their conclusion that the amplitude is larger under solar minimum conditions.

Reply: Our manuscript is indeed a bit misleading in this context, we apologize. The lower panel of Fig. 10 cannot be directly compared to Table 1. Table 1 lists the analysis results for solar activity exceeding a certain F10.7 cm flux threshold, while the bottom panel of Fig. 10 shows the SEA results for epochs with F10.7 cm values less than 130 sfu. In other words, for the two analyses, different sets of epochs are used. We now added some text to section 4.2 to address this point explicitly.

(3) The listings in Table 2 (summarizing the results for different times of the year) are hard to understand. The mean 27-day sensitivities are larger for the summer (May to

Sept.) and winter (Oct. to Feb.) seasons (-.45 and -.49, respectively) than they are for the whole year (-.37). Is this because the sensitivities in March and April are much lower than for the rest of the year? Please explain. Also, the amplitude of the SPH change is about a factor of 2 larger in the winter season than in the summer season even though the mean sensitivities in the two seasons are about the same. The discussion in section 5 (lines 375-389) gives some explanation for why the amplitude should be larger in winter, involving larger downward transport of NO from the thermosphere, increased 27-day NO production in the Auroral zone, etc. But shouldn't the sensitivities be larger in winter also?

Reply: We cannot fully explain some of the obtained results, as already mentioned several times in the manuscript. This is certainly unsatisfactory, but we hope that some of these aspects will be better understood in the future, when the understanding of the relative contributions of dynamics (potentially solar driven) and solar variability has been improved. We now also cite and briefly discuss the study by Pancheva et al. (1991) reporting on quasi 27-day variations in radio wave absorption measurements. The reported results are quite consistent with many of our puzzling results, e.g. larger amplitudes during winter and solar minimum.

In addition, we would like to point out that the sensitivity values for summer, winter and all-year agree within a 2-sigma range. It is difficult to state, whether the sensitivities are much lower in March and April, because an analysis of these months would be based on a small number of epochs.

The relatively large fraction of random ensemble members with amplitudes larger than the actual analysis for summer (11.2%) may be a reason for the unexpected behavior of amplitudes vs. sensitivities between summer and winter. This is now explicitly stated in the manuscript.

(4) One standard deviation error limits are shown in the figures (e.g., Figure 4). If I

remember correctly, the probability that the true value lies within these limits is only around 68

Reply: The thin lines in Figure 4 (both for standard phase height and the F10.7 cm flux) do not correspond to one standard deviation, but are the standard errors of the mean, i.e. the standard deviation divided by the number of epochs. The standard deviations themselves are much larger than the values shown. The top panel of Figure 2 also shows the standard deviations of the SPH anomaly, which is on the order of 600 m. The standard errors of the mean shown, e.g., in Figure 4 are determined the following way: For each day relative to local solar maximum the corresponding SPH anomaly values from the individual epochs are averaged and their standard deviation is determined. The standard error of the mean is the standard deviation divided by the square root of the number of epochs. This is standard procedure and this is the correct estimate of the uncertainly of a mean value.

Note: point (5) was missing in the review

Minor Points:

(6) This is probably just personal preference but I would use “inversely correlated” rather than anti-correlated, which could be misinterpreted to mean no correlation.

Reply: OK, changed throughout the manuscript

(7) It is necessary to read the details of section 2 to understand what standard phase height means. Since this manuscript is apparently submitted to a special issue on layered phenomena in the mesopause region, it may be obvious to most readers that this term refers to an effect in the upper atmosphere. However, to save many casual readers the trouble of reading section 2 to understand what the manuscript is about, it would be better to indicate in the abstract (preferred) or introduction what is being investigated. For example, in the abstract, you could change line 2 to read: “standard

phase height measurements in the ionospheric D region carried out in central Europe."

Reply: OK, changed. We also added some additional pieces of information to this sentence that hopefully make the concept of phase heights easier to understand.

(8) Line 86: ... such as the atmospheric processes ...

Reply: changed (assuming the comment refers to line 88 in the ACPD version)

(9) Line 130: ... by checking how many of the ...

Reply: the suggested change does not fit to the rest of the sentence. We changed the sentence to

"by checking for how many of the 1000 random cases the amplitude of the fitted sinusoidal function equals or exceeds the amplitude of the sinusoidal fit to the actual epoch-averaged standard phase height anomaly."

(10) Lines 329-330. Please re-write this sentence.

Reply: we revised the entire paragraph, also following the suggestions by reviewer #3 and hope this is now easier to understand. The new paragraph is:

"In the upper mesosphere, the negative regression pattern between the GH anomaly and the 24 – 31 day-band-pass filtered SPH time series over central Europe in about 80 km altitude for lag zero may be explained by horizontal planetary wave transport. An increase (decrease) of NO density is caused by southward (northward) transport of NO by ultra-long waves for an observed mean positive latitudinal NO gradient in a region between a high and low (low and high) pressure system. Vertical transport of NO by lifting or subsidence is assumed to be weak, diffusion too. The consequence is an increase (decrease) of the free electron number density due to photo-ionization as discussed by von Cossart and Entzian (1976) with a lower (higher) SPH. That implies that SPH shows a negative

(positive) regression with the GH anomaly on the easterly (westerly) side of the high-pressure center."

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