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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #3

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This is an interesting paper that is potentially appropriate for ACP. The authors present a thorough uncertainty analysis for their SEA approach and I think their basic results are credible. They also find some interesting deviations from simple expectations- which are somewhat credibly presented; however, while I appreciate their difficulties in trying to explain all this, I confess I got lost in trying to understand much of their hypothesizing. I had difficulties with Figures 11 and 12 to the extent that I do not see where they can state on lines 429-430 that "for the first time . . .. associated with quasi 27 day periods". For that to be valid, I need to see a Fourier spectrum of geopotential height with significant power at that period. In other words, Figure 3 needs to be repeated for GPH (preferably as a function of altitude, as per some of my comments below). And even if they do that- did not they just say that this was first shown by Ebel

et al, 1981. So what justifies the phrase "for the first time"?

Other Major comments

A. Writing/presentation: I recommend breaking up Section 4. It's a jumble of analyses that comes off confusingly. Tellingly, they have to subdivide their Section 3 times (4.2.2) which is hard to follow. They should have a section on "Results" which present their 4 basic results (i.e. SPH basically correlates plus the three puzzles as listed in Section 5). Then Section 4.3 is really (I think) an attempt to find some interpretation- this should be separated.

B. Figure 11 confused me. First, (line 284), there is no "middle panel". Only top and bottom. Second, where is SPH in all this- why can't they correlate the CMAM .01 hPA temperature with SPH? Third, and related, what is the altitude variation of the variability in this band-pass? Or altitude variation of the correlation/regression with SPH? This would relate to whether the forcing is in-situ (i.e. planetary wave mixing at .01 hpa) or due to integrated height changes.

C. The issue of the poorly understood negative lag. First, where do they show this? Which figure has the correlation plotted vs. phase shown a peak at a specific phase? In the absence of this, where am I supposed to find the phase lag? All I see is some words on line 168-169. Does the phase lag change in winter vs. summer? Their arguments in 4.3.2 would seem to be relevant for winter (i.e. requires a mesospheric vortex). Are they saying that the effect is so small in summer that they are ignoring it? That may be OK, but if so, say so more explicitly.

D. Note, there is literature on this question dating back to ozone studies in the 1980s. See for example, Brasseur et al., JGR, 1987, page 903 or Eckman, JGR, 1986, page 6705. Mathematically, from Fourier analysis, if there is damping or negative feedback, it will manifest itself as a negative lag (i.e. response precedes forcing). I confess I do not know if this shows up in wavelets, but it's worth considering.

E. Why are they choosing a phase lag of 12 days for Figure 12? Shouldn't they use the phase lag for which the correlation maximizes? Earlier in the text they say 1-3 days.

F. I looked at the CMAM30 web site they give. There is nitric oxide data. I suggest they use this data to compare with geopotential height, solar changes etc.

Minor comments

1. They need to specify where they got the Lyman alpha time series. Is this a proxy they developed? Is it from satellite data?

2. It would be helpful to provide more context to the standard height technique. I realize this technique is mature, but there are also VLF measurements which, at first glance, are pretty similar in approach. In Peters and Entzian, they mention a reflection height of 500 cm-3. Does the shape of the profile matter? It does for VLF.

3. The VLF technique provides an altitude profile- see any number of papers by N.R. Thomson. Apparently the SPH technique does not? But this makes it hard to interpret height changes. What does a 1 km height change really mean in terms of the electron density? Is this a local increase, or descent of a layer?

4. Lines 322-332. Very confusing. What is "it follows" (line 327)? Sentence needs a verb. Then I don't understand the argument on lines 331. Why should electron density go up if air pressure is higher? Perhaps it would mean more recombination and thus the opposite. And this sounds like a different mechanism than on 324- southward transport.

5. Lines 324-354: There are references worth citing on mesospheric nitric oxide transport and planetary waves, for example: Siskind et al., JGR, 1997, p.3527. Mesospheric transport due to breaking planetary waves is also covered in Sassi et al., JGR, 2002, 4380. More recently, work by Lynn Harvey has discussed the mesospheric polar vortex. She uses CO as a diagnostic. I don't suggest they redo her work, but certainly consider it and cite it, at minimum.

6. Figure 12 needs color bars. None of the labels are readable. The caption should explicitly state what the red/pink and blue colors are. My evaluation of this figure and associated text will likely change once I can actually make out what this is a plot of.

7. Why do they choose 12 days for $\frac{1}{2}$ a solar cycle (lines 315-316)? Should be 13 or 14. (but also consider comment E. above).

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