

Interactive comment on “NLC and the background atmosphere above ALOMAR” by J. Fiedler et al.

Anonymous Referee #2

Received and published: 25 March 2011

General Comments:

This paper describes a comprehensive set of ground-based noctilucent cloud observations near 69 N from 1997 to 2010 and supports the analysis with ground-based wind measurements and model results of winds and temperatures. The data set is a long enough time series to draw conclusions on solar cycle effects of cloud properties but the authors go well beyond this and report results on inter-annual tidal variability as well as its potential effect on the long-term record of these properties.

This is a very interesting paper with important new results on horizontal wind measurements in the presence or absence of NLC observations as well as on inter-annual variability of phases in cloud properties over the diurnal cycle. The authors furthermore provide a satisfying discussion of potential sampling concerns in Sections 3 and 6 and the introduction to the topic (Section 1) is particularly well organized and comprehen-

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sive.

The reviewer nonetheless has some important concerns on the presentation of their results, which must be addressed in the revised version of the manuscript (#1, #2 and #4 below). They can be addressed with some modification of figures and some additional supporting text. There is one major specific comment below (#3) which requires some significant additional analysis. In addition there are many places in the manuscript where the text reads awkwardly or where there are words missing and so the reviewer has many technical comments also included below.

Specific Comments:

1) Figure 6. Variations in panels 3 and 5 are difficult to discern in this important figure. In particular, in panel 3 the reviewer cannot discern the lower limit of ΔT . The reviewer suggests drawing contours at 1 or 2 K intervals in panel 3 and 10 m/s intervals in panel 5. A better selection of colors would also help here to bring out the variations within these two panels. In the text, please state the minimum-to-maximum temperature and zonal wind variation over this period at 83 km altitude as indicated in panels 3 and 5. If this is a representative variation within all the model results please say so. Thank you.

2) Figure 8. The temperature amplitude here looks to be about ± 1.5 K. This appears to be smaller than what is shown in Figure 6 (3rd panel), although it is hard to tell (see Comment #1). Perhaps phase shifts in temperature combined with the heavily averaged result in Figure 8 reduces the amplitude. If this is true then the authors should say so. In the caption of Figure 8, “errors” are associated with measurements rather than models. The reviewer is not sure what the authors mean by “errors” in the model temperatures here. Please rephrase and explain.

In addition, why do the authors not show brightness (B_{tot}) as well in Figure 8? The total amount of ice observed seasonally is proportional to the vertically integrated ice mass density times the observation frequency and the reviewer cannot think of a reason why B_{tot} would not be directly related to this. Can the authors? If not, please include B_{tot}

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as well as $B_{tot} \cdot OF$ in this figure, perhaps as another panel. The reviewer feels most strongly about this addition to the figure. Since B_{tot} is nearly in phase with OF (for $B_{max} > 1$), the reviewer expects that this proxy for ice water content ($B_{tot} \cdot OF$) would show a larger variation over the diurnal cycle than just OF . On a related note, Kirkwood et al. (*Annales Geophysicae* 28, 1333, 2010) recently compared PMSE observations in the southern hemisphere with SOFIE ice water content observations on AIM and it would seem to be a good opportunity for the authors to do the same with their NLC data in the northern hemisphere. This would nicely combine the global capabilities of AIM with the local time coverage from ALOMAR.

3) The reviewer has studied Figure 10 for quite a while and even after re-reading the supporting text multiple times still does not understand the significance of the results. The general point about the changing phases aliasing to a long-term trend is clear and potentially important. The demonstration of the effect in Figure 10 is rather hard to follow. Is the black line with the symbols (the “mean”) the same in both right hand panels? Since Shettle et al. averaged the satellite data together at discrete local times, the same approach should be used here to find an average. That is, the red, green and blue curves in the upper panel should be averaged together for a mean and likewise for the bottom panel. The reviewer is not sure the authors did it this way since they state “black curves are the mean over all local times.” Note also that SBUV is only sensitive to bright clouds so the OF should only use the brightest ground-based data, which is probably what the authors did but they should say this.

Most importantly, the authors must put the results into the context of the Shettle et al. work. In their Table 1, Shettle et al. report a trend of 6.7%/decade for this latitude region. Can the authors report an effect from changing phases that competes with this OF trend? If so, in what direction would the trend go? Clearly, local time effects in any given year are much larger than reported OF trends but this has been reported in many places before, even by Shettle et al. (their Figure 1). The effect of frequency on trends due to phase shifting is new but not useful unless it is put in quantitative context

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of previous work on long-term trends.

If there is little difference in a trend from fixed or variable phases, could the authors speculate on how frequencies might trend due exclusively to tidally induced variations all the way back to 1979 (when the SBUV database began) using Figure 2 of DeLand et al. [2007]? The reviewer suspects that OF is biased high in the early portion of that dataset when the observations were limited to morning local times.

4) P. 5659, lines 17-20. The reviewer is confused by these two sentences. Up until this point and in Figure 12 the authors try to separate the Lyman alpha effects from temperature effects. In this first sentence they indicate that there is a relationship between Lyman alpha and temperature. Please elaborate. And please add a sentence or two explaining the relationship of the solar cycle signal in the NLCs to the assimilated ECMWF data – the second sentence implies that the solar cycle variability originates from altitudes 30 km and below since that is where the ECMWF data is available.

Technical Comments:

1) P. 5642, lines 7-8. This should read “. . .during NLC observations compared to when NLC are absent.”

2) P. 5642, line 8. “On seasonal mean. . .” should perhaps read “On average. . .”. Or instead of “On seasonal mean it is colder. . .” it should read “The seasonally averaged temperature is lower. . .”

3) P. 5642, line 10. “model” should read “model results”.

4) P. 5642, line 11. “14-years” should be “14-year”.

5) P. 5642, lines 18-19. The reviewer does not understand the phrase “partly pronounced”. Please reword. Do they mean “some”?

6) P. 5642, line 24. The Leslie (1885) reference should appear first as it was published before the Backhouse letter. Also on line 26, the word “became” is not appropriate

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because Leslie recognized that they were at high altitude immediately. The reviewer suggests replacing this word with “was”.

7) P. 5643, line 9. “of few” should be “of a few”.

8) P. 5643, line 14. The reviewer has not heard of the term “data basis” and suggests “dataset”.

9) P. 5643, line 19. There should be a reference after PMC [Thomas, G. E., Solar Mesospheric Explorer measurements of polar mesospheric clouds (noctilucent clouds), J. Atmos. Terr. Phys., 46, 819-824, 1984].

10) P. 5643, line 23. “on regular” should be “on a regular”. Also, the term “Earth albedo” suggests that the light is coming from the surface, whereas in fact it is scattered by the atmosphere. The reviewer suggests instead simply “albedo”.

11) P. 5643, line 29. There should be a reference at the end of this sentence such as G.E. Thomas, “Is the polar mesosphere the miner’s canary of global change?”, Adv. Space Res., 18, 149-158, 1996.

12) P. 5644, line 10. This sentence is awkward. The reviewer suggests replacing “is” with “has been” and “since” with “for”.

13) P. 5644, lines 19-20. This sentence is awkward and incoherent. Instead of “during simultaneously. . .Earth.” the reviewer suggests “with synoptic global coverage.”

14) P. 5644, line 26. In order to avoid any confusion between spatial and temporal sampling, instead of “a complete range at all local times” the reviewer suggests “a diurnal cycle”.

15) P. 5644, lines 27-28. The reviewer does not understand this sentence at all and asks that it be deleted. Then in the next sentence rewrite: “In the following we will investigate the state of the atmosphere using lidar observations at. . .” Also please delete “already” in line 29.

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16) P. 5645, line 5. “A further. . .” should be “An additional. . .” and “are” should be “is”.

17) P. 5645, line 15. “of spectral” should be “of a spectral”

18) P. 5645, line 18. “as function” should be “as a function”.

19) P. 5645, line 22. “as measure” should be “as a measure”.

20) p. 5646, line 6. “a human observer would experience” is awkward and the reviewer suggests “one would observe”.

21) P. 5646, line 9. “i.e.” means “in other words” so “like i.e.” does not make sense. The reviewer instead suggests “such as”.

22) P. 5646, line 10. “subject of” should be “subject to”.

23) P. 5646, line 24. “appropriated” should be “appropriate”.

24) P. 5647, line 22. Do the authors mean “with” instead of “relative to”?

25) P. 4649, line 18. “of same type” should be “of the same type”.

26) P. 5651, line 13. “adequate to each other” does not make sense. The reviewer suggests something like “in good agreement”.

27) P. 5651, line 20. Please add here that the data are 3-year running means. Otherwise “the corresponding 3 years range” does not make sense.

28) P. 5652, line 12. “other” should be “others”.

29) P. 5652, line 15. “sensitive” should be “sensitively”.

30) P. 5652, line 17. The reviewer suggests deleting “of different kind” and instead adding references.

31) P. 5652, line 17. “Water vapor provided” does not make sense and the reviewer does not understand what the authors are trying to say. Do they mean “For a known vertical distribution of water vapor, temperature turns out. . .”?

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32) P. 5653, line 5 and below. The inclusion of the numbers in parentheses is confusing to the reviewer. Here they are defined as a range but below as a standard deviation. It would be clearer to include the half-range in parentheses with a +/- sign and then with the standard deviation later also include a +/- sign. Thank you.

33) P. 5653, line 9. "nearly solely" is awkward and the reviewer instead suggests "almost exclusively" to avoid consecutive adverbs.

34) P. 5653, line 13. The reviewer does not know what a "data basis" is. Perhaps the authors mean "database"?

35) P. 5654, line 2. "determines" should be "has observed".

36) P. 5654, lines 5-6. Does this standard deviation represent the geophysical variability or the measurement uncertainty or both? Please be explicit and add +/- in the parentheses.

37) P. 5654, line 10. "in" should be "on".

38) P. 5654, line 23. "the ice particles" should read "an ice particle's" or "the ice particles".

39) P. 5655, line 2. "on hourly basis" should be "on an hourly basis".

40) P. 5655, lines 14-15. The reviewer does not understand "assuming the zonal wind variations covering also stratosphere/lower mesosphere..." Do the authors mean "If the zonal wind variations are also present in the stratosphere/lower mesosphere..."? And if so, incidentally, could they not check this in the model? For that matter, the reviewer also suspects that the speculation on enhanced vertical winds coupled to enhanced zonal winds could also be checked in the model. This would at least rule in or rule out some processes suggested by the authors and does not seem to the reviewer to be so far outside the scope of the work.

41) P. 5655, line 20. "considerably contribute" should be "contribute considerably".

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42) P. 5658, line 25-26. Couldn't the temperature be driven by solar activity? Would it be more accurate to say "...no significant anti-correlation between occurrence and solar Lyman-alpha radiation"?

43) P. 5659, line 25. The reviewer does not understand the notation "+0.25..+0.53". Please be more clear. Thank you.

44) P. 5660, line 16. "have been" should be "were".

45) P. 5660, line 19. The reviewer does not understand the notation "-9...-10 m/s" and "-2...-5 m/s".

46) P. 5661, line 5-6. This sentence is confusing. As noted by the reviewer before, is it not possible that solar activity could be affecting the temperature? If not please explain. If so, then perhaps this sentence should read "...no statistically significant relation between NLC occurrence and solar Lyman-alpha irradiance..."

47) P. 5661, line 22. "monotonic" should be "monotonically" since it is modifying "increases".

48) P. 5661, lines 25-27. "like obtained" should be "like those obtained". Also, please indicate here whether the changing phases can make a statistically significant impact on reported OF trends, as discussed earlier by the reviewer.

49) Figure 10, caption. "tick" should be "thick".

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5641, 2011.

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