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Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

Interactive comment on "Mesospheric Anomalous Diffusion During Noctilucent Clouds" *by* Fazlul I. Laskar et al.

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We thank the reviewer for the thoughtful comments and suggestions. Below we answer them individually. The authors' response start with "Response".

The authors reported the difference for Da measured by the meteor radars during the existence of NLCs and considered the possible mechanism related with the observations. However, the deduced conclusions from the analysis seemed to be more clarified before publication. My main concerns are listed as follows:

1. The paper used daily Da, which is proportion to the T and P, and can be obtained from satellite observations (such as SABER or MLS). Using the Da from satellite measurements during the same period, i.e., 2012-2016 should be better than WACCM-

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Discussion paper

DART data during 2007.

Response: We thank the reviewer for pointing at the satellite observations. However, because of the following issues we refrain ourselves in their use for the current study:

(i) The reviewer might be aware that MLS being an A-train (afternoon at equator) satellite has just one fixed local time over a location, which also does not change much over couple of days for SABER's case. Thus they are not necessarily coincident with the lidar NLC observations. Also because of their low (for SABER) and no (for MLS) local time shift they will be highly modulated by tides.

(ii) Further one has to consider how both satellites retrieve the parameters of T and p. In the case of MLS the observed irradiances are used to derive temperature and geopotential height. The state vector uses 47 fixed pressure levels and a geopoetential reference height at 100 hPa (Schwartz et al., 2008). Due to the coarse vertical resolution of MLS at the MLT region a precise observation of T and p is not achievable considering the required accuracy.

In the case of SABER the retrieval of T and rho or p are not independent. The primary observed quantity is irradiance where density of a certain Molecule, CO2 in the mesosphere is converted into a neutral density assuming a volume mixing ratio. Considering the statistical errors of this conversion (Remsberg et al., 2008, Rezac et al., 2015), unfortunately does not hold the required accuracy. The second issue is that we want to study a polar effect, which is even more challenging with SABER due to the Yaw cycle, which would further deplete our measurement statistics.

However, as pointed out by the reviewer it is might be worth to collect more data and compare just the times of the satellite overpasses. Considering the present statistical database it is not sufficient to do that. Such comparisons seem to be more beneficial for systems at lower latitudes, with a much better instrumental coverage, in particular, from the SABER instrument.

References: Schwartz, M. J., et al. (2008), Validation of the Aura Microwave Limb Sounder temperature and geopotential height measurements, J. Geophys. Res., 113, D15S11, doi:10.1029/2007JD008783.

Remsberg, E. E., et al. (2008), Assessment of the quality of the Version 1.07 temperature-versus-pressure profiles of the middle atmosphere from TIMED/SABER, J. Geophys. Res., 113, D17101, doi:10.1029/2008JD010013.

Rezac, L., Y. Jian, J. Yue, J. M. Russell III, A. Kutepov, R. Garcia, K. Walker, and P. Bernath (2015), Validation of the global distribution of CO 2 volume mixing ratio in the mesosphere and lower thermosphere from SABER, J. Geophys. Res. Atmos., 120, 12,067–12,081, doi:10.1002/2015JD023955.

2. Figure 3, the authors claim the obvious difference of Da during yNLC/nNLC for high-,middle- and low-latitude stations. Is the result statistically significant? If using a random sampling during the lidar observation period to re-group the yNLC and nNLC, how about the response of Da at different latitudes?

Response: We thank the reviewer for this suggestion. While carrying out this test we have come across a bug in our program, which removed many of the meteor trails having extreme values of diffusion from only NLC case and not from no-NLC case of analysis. After making this correction, we see that the difference between NLC and no-NLC profiles are only significant at high-latitudes. At mid- and low-latitudes they are either not-systematic or within the 95% significance levels. As per suggestions of the reviewer, we did a random sampling during the lidar observation period and also during whole summer (June-July-August). In both cases there were no such difference between the profiles at over any of the stations/latitudes. Results from the first test using lidar observation interval are shown in illustration figure 1 (attached) or in supporting info Figure S2 of the revised manuscript. Some discussions on this are added in P.6 L.17-22 in the revised MS.

3. The Da in Figure 3 is separated according to the lidar measurements. The lidar

has time resolution of 15 min. Are the Da measured by radar at different location fully covered the lidar sampling period? For example, are the Da at Andenes, Juliusruch and Biak all available for 107/89 hrs of yNLC/nNLC period during 2012 (the first row of Figure 3)? If not, what's the proportion of the data coverage?

Response: Since lidar observations needs clear sky (no cloud) condition and radar do not have such restrictions the Da measurements durations available are much higher than lidar. However, as there are some datagaps in the radar observations and also we have not considered any intervals where the geo-magnetic activity was high (AE>400 nT) the Da measured at different location do not fully cover the lidar observation windows. Now, based on the common availability of NLC and Da data we have revised Figure 3 and Figure 5. Now in the revised Figure 3 all the common available durations are depicted, where one can see that the common windows are different at different latitudes. Also, in the revised Figure 5 we have not considered the NLC observations where we have no Da measurements. About the proportion, the numbers are now depicted in Figure 3. Some discussions on Da availabiltiy over different stations are mentioned in P6 L.8-15.

4. The authors indicate the global tide are responsible for the observed difference at different latitudes. However, (1) the dominant tidal model also depends on the latitudes. For example, the semi-diurnal tide is dominant at high latitude, while, diurnal tide is dominant at low latitude. This situation can also be found in Figure 4. (2) is the local time response of NLC and tide correlated? In Figure 5, except for the year of 2013, the NLC did not show significant local time dependence. According to the comments above, I am a little confused, since the NLC did not show significant local time dependence, how to explain the observed difference at middle and low latitude region?Especially under the scenario of the different dominant tidal modes and different local time dependence for different latitude?

Response: We thank the reviewer for this thoughtful suggestion in (1). We have now added couple of sentence on this in P.5 L.30-32. On (2), yes, we agree that even in the revised Figure 5 NLC does not have very strong diurnal variations, except in the year 2013. As mentioned above there was a bug in the processing program, which gave rise to systematic difference between Da profiles during NLC and no-NLC at all latitudes. Now with the correction such differences exist only at high-latitudes. Thus in the revised manuscript we re-interpret out corrected results and state that: As the NLC data used here does not show significant daily variations we interpret the observed differences at high-latitudes are primarily due to NLCs. We hope that the reviewer finds our revised results more convincing.

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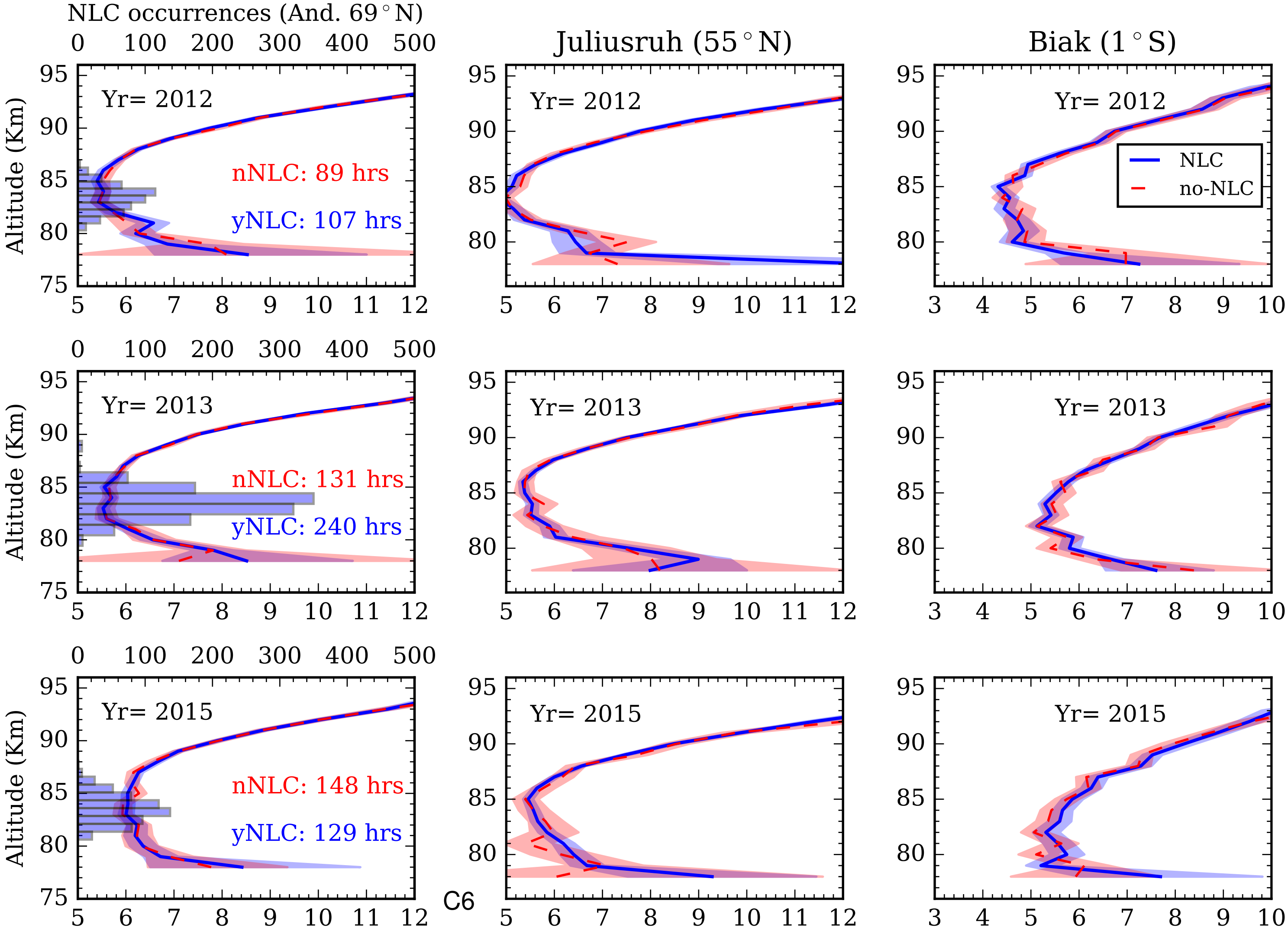
NLC occurrences (And. 69 ° N)
0 100 200 300 400 500
Juliusruh (55 ° N)
Biak (1 ° S)
Altitude (Km)
95 90 85 80 75
5 6 7 8 9 10 11 12
3 4 5 6 7 8 9 10
Yr= 2012
nNLC: 89 hrs
yNLC: 107 hrs
NLC
no-NLC
Yr= 2013
nNLC: 131 hrs
yNLC: 240 hrs
Yr= 2015
nNLC: 148 hrs
yNLC: 129 hrs

C6