Comment on *Springtime nitrogen oxides and tropospheric ozone in Svalbard: results from the measurement station network* by Dekhtyareva et al.,

The paper is based on a novel set of NOx and ozone measurements from three settlements on Svalbard during spring 2017. The main objective (page 4 lines 89-90) is to: i*dentify specific factors affecting the concentration of measured compounds and define conditions that promote accumulation of local and long-range transported pollution in all three settlements.*.

The paper do address relevant scientific questions within the scope of ACP, and it does present novel data from the measurements on Svalbard and applies a semi-novel concept linking atmospheric circulation regimes and pollution levels.

Although several possible environmental impacts are mentioned in the introduction, objective/science questions on page 4 and the discussion/conclusion sections it becomes clear that the primary objective is to study the air quality (wrt NOx and ozone) in the three settlements and how this is related to atmospheric conditions.

The main issue with the paper is the inconsistency of the data. This relates to the lack of co-location for the NOx and ozone measurements, no ozone measeuements in Adventdalen, and the issues with calibration of the instruments in Barentsburg (as also pointed out the authors at the end of the discussion section). This has effects on the conclusions that can be drawn, in that these can not be very strong. The manuscript includes in several places statements like "This *may be explained* by the fact that the measurement station in Ny-Ålesund was located much closer to the diesel power plant, ….. ". Also, the discussion linking circulation regimes and air quality lacks clarity and could benefit from including more years (using ozone data from Zeppelin) to get a more robust result.

I find that the description and discussion in many places could have been more precise (cf detailed comments below).

I suggest that the manuscript can be published after major revisions.

Specific Comments:

Page 2, line 43. There is a very appropriate reference to Beine et al., 1997 who found that PAN decomposition was an important source of NOx at the Zeppelin station. However, this point has not been taken up again during the discussions.

Page 3 line 68: Is it really true that there is no diurnal cycle in the demand for electricity in Longyearbyen? What about the use for cooking and light with a peak in the afternoon?

Page 7 lines 160-170. It is unclear what this "no regime" is. The "for example" wording on line 160 is very confusing.

Page 7 lines 167-171. The sentence starts with "Secondly". Does this means that the identification of the flow regimes for the sub-periods was done on another dataset than the ERA5?

Page 10 line 248: The amplitudes are not very high (+/- 2ppb or so), but they are very short term fluctuations.

Page 11 line 268: Define ozone titration efficiency

Page 11 line 267-271: First, it is stated that there is a strong negative correlation, and then later (line 270) it is stated that it is not statistically significant. This seems contradictory.

Page 11 line 281. The word “However” seems misplaced here. Since the measurements are so close to the source this kind of extreme values can be expected there.

Page 10 line 247. It is concluded that the synoptic conditions have minor effects on NOx due to the low correlation between Ny-Ålesund and Barentsbug. However, from the maps (figure 6) it is clear that in Ny-Ålesund the source is North of the station, while it is the opposite in Barentsburg. Thus, I would expect that the wind direction component of the synoptic conditions could give negative correlation.

On the NO/NOx relation

Generally there will be a local steady state given by the reactions

$NO + O_3 \rightarrow NO_2 + O_2$ R1

$NO_2 + h\nu\ (+O_2) \rightarrow NO + O_3$ R2

At steady state

[NO] = [NO2]* k2/(k1*[O3]

The paper seems to neglect the effect of reaction R2 for the NO2/NOx ratio. With appropriate values for k1, [O3] and k2 (the photolysis rate) one can derive the steady state NO2/NOx ratio. It would be of interest to see how the ratios observed in Adventdalen during daytime is affected by the photolysis.

Figure 4. The diurnal cycle of the means of ozone at BB and Zeppelin are shown. These are the means over the springtime period (about 55 days) I presume. Please add to this figure the standard error of the mean for each hour, so that we can see if these diurnal cycles are really statistically significant.

Page 12, line 305. I don’t understand the argument that enhanced photolysis is compensated by convection. Photolysis in itself would not reduce ozone significantly as the reaction $O + O_2 + M \rightarrow O_3 + M$ would very rapidly reform ozone. In addition, I would expect that for Zeppelin convection would mix in PBL air with lower ozone. At very low NOx levels there could be enhanced loss of ozone during daytime through $O_3 + OH \rightarrow O_2 + HO_2$ followed by $O_3 + HO_2 \rightarrow OH + 2\ O_2$

Page 12, line 307: If the diurnal cycle is not statistically significant, there is no need to discuss possible physical/chemical explanations!

Page 12 line 312: Wind speed 4.1 m/s. This must be the average wind speed. Please also give the variance.

Page 13 line 317: You have written: normally ventilation is sufficient to remove NO_x emitted by the usual amount of motorized traffic. I don't understand this statement: is NOx completely removed?

Page 15 line 342: Unclear sentence. Is the 46% referring to the whole period or sub-period I? Is 0.95 °C the median or is it the deviation from the median during sub-period I.

Figure 7. It would be useful to have the sub-periods indicated over the individual plots.

Page 14, Section 3.2: I find this whole section quite unorganized. The whole section seems to focus on PBL high and how it affects NOx, and possible transport patterns for ozone that allow ozone depletion events. There is a lack of motivation for selecting these regimes. E.g. the regimes depicted in fig 7a and 7h looks very similar to me, and without a rational for splitting this in two different regimes. A factor that is completely missing is the possibility of tropopause folding events with intrusion of ozone rich air, presumable related to the circulation regimes.

On the transport of pollutants to Svalbard the work by Hirdman et al. should be referenced (Hirdman et al., Atmos. Chem. Phys., 10, 9351–9368, 2010 www.atmos-chem-phys.net/10/9351/2010/ doi:10.5194/acp-10-9351-2010)

Page 16, line 366-380. Elevated NOx concentrations were found in Adventdalen but not in Barentsburg for period VI with cold conditions and low PBL height. These condition with low wind (and maybe clear sky) would I believe enhance recreational snow mobile traffic and thus emissions, which is much more pronounced in Longyearbyen than in Barentsburg. In general, there may be a link between weather and emissions that is not mentioned in the paper.

Page 18, line 382-390. The paper concludes (elsewhere) that there is high correlation between ozone at Zeppelin and Barentsburg, thus the measurements represent region ozone levels. Since ozone data from Zeppelin is available for a number of years, this regime analysis could be extended for a much longer period and thus be much more robust.

Figure 8:

-This is not really the trajectory probability for the different regimes, but rather for the sub-periods. Having a longer (multi year) record to make these probabilities for the regimes would help. Very difficult to read the red contours.

- The maps are very small. There is no need to include the same label bar 9 times. Also I recommend that each map is labeled with the name of the corresponding circulation regime (applies also to figure 7).

Page 19, line 396. Why is the HYSPLIt model used for these back trajectories and not Flexpart?

Page 22, line 474. The authors claim that "The weather regime approach is novel in the air pollution research". However, this has been used in several studies before, although not for the Svalbard region I believe. See references below.

Muntasir A. Ibrahim*, Gabriele Curci, Farouk I. Habbani, Fred Kucharski, Paolo Tuccella, Susanna Strada, Linking weather regimes and air pollution/air quality.

https://doi.org/10.30799/jespr.210.21070101

M. Ménégoz,V. Guemas,D. Salas y Melia,A. Voldoire, https://doi.org/10.1029/2009JD012480