This paper reports springtime nitrogen oxides and tropospheric ozone measurements in Svalbard, using observations on 3 stations, during less almost 2 months in spring 2017, and Lagrangian backward trajectory analysis.

The observations itself are of interest because in the Svalbard are really sparse, so they can help to understand formation and evolution of O_3 in a remote area effected by interesting and mixed effects such us depletion due to reactive halogen compounds and photochemistry due to local well defined emission sources. However, there are several shortcomings that limit a lot the results of this paper, therefore I'm really skeptical to suggest to accept this manuscript for publication on ACP, unless it will be deeply revised, for the following reasons:

1) Only in the Barentsburg Station are observed both O_3 and NOx, that are the two species fundamental for this study, in the Adventdalen and Ny-Alesund sites are missed the measure of O3, therefore a comparison and correlations of these species among these station is misleading. To partially overcome this problem for the Ny-Alesund analysis are used the O_3 measurements of the Zeppelin observatory, but this is not the solution not only because the latter is 2 km away from Ny-Alesund site, but, more seriously, it is on the top of a mountain at 474m. a.s.l., whereas Ny-Alesund site is at 23 m. a.s.l. and near the sea. Finally, since in Ny-Alesund site are missed also meteorological measurements, for the analysis on this site are used data of the top of the mountain (Zeppelin station). It would have been much more worth to install the NO_x analyser in the Zeppelin station where meteorological data and O_3 measurements were available then in the Ny-Alesund site.
2) Lines 233-236: Since the CO measurements were stable at Zeppelin station so no sharp peaks to identify local emission were detected, this is a proof that the measurements at Ny-Alesund are not useful to understand potential impact of local pollution on O_3 evolution in the mountain top.
3) Lines 243-247: Here the Author affirm that local emissions are important because the correlation between NO_x measurements at Ny-Alesund and Adventdalen sites are weak, but at lines 233-236, looking at CO data they assert that local pollution are not important for the area under investigation. This is contradictory conclusion is due to another big issue: since CO measurements are available only at Zeppelin station, the signature of local emissions in other sites (where CO were not measured), were tried to find in the correlation between NO_x observation, again measurements of CO in Ny-Alesund and Adventdalen site would have been worth to make this conclusion.
4) Lines 255-257: Author here assert that synoptic transport is more important than local emission looking, now, at the correlation of O_3 measurements at Zeppelin and Barentsburg site, a couple of issues: a) $r = 0.69$ is considered a 'strong correlation', it means $r^2 = 0.47$, that is not that 'strong', b) again since O_3 is missed in the Ny-Alesund and Adventdalen, now to decide if dominate local emission or synoptic transport the correlation od O_3 between Zeppelin and Barentsburg site are used, while before (lines 233-236) were used NO_x for Ny-Alesund and Adventdalen sites, obtaining contradictory results.
5) Lines 295-298 and figure 4: The O_3 diurnal cycle of Barentsburg site is, as expected, completely different of that of the Zeppelin station, in the first is evident the typical profile dominated by photochemistry, in the later, a typical mountain station data, with no diurnal cycle. This is what expected, but again in contrast with what reported in lines 255-257 where Author affirm that the O_3 measured in these two sites showed a 'strong correlation': they have a completely different dynamics, as can be expected since one is in a mountain and the other in a site at 40 m. a.s.l.
6) Lines 385-399: Here initially, looking at trajectory analysis Authors affirm that the O_3 decrease can be explained by local depletion due to air masses rich of BrO, whereas at the end is supposed that may be due to less photochemistry due to 'lack of sunlight and O_3 precursors such as NO_x'. A good result of this analysis would have been if the two effects (depletion due to BrO vs photochemistry and NO_x emission) were well characterized and, from observations and model

analysis, quantified and compared, unfortunately here both effects are claimed, as can be guessed even without any kind of measurements and/or model analysis.

7) Lines 408-411: Finally, the Authors, looking O_3 sondes data confirm that Zepellin data are different of that in Barentsburg because one is at the top of Mountain and the other at 40 m. a.s.l. I think that this would have been the first analysis of the paper, and not the last one after different correlation analysis where was mentioned that O_3 data of those sites are strongly correlated.
8) Lines 471-473: From the data and analysis I'm not comfortable with the conclusion that local emission of NO_x may reduce O_3 level by few percent in the Ny-Alensund site, since O_3 there is not measured, but, again, are used for this conclusion O_3 measured at the top of mountain. Here for example, since O_3 where not measured would have been worth to use a Box model (such as MCM) to model O_3 at Ny-Alensund, constrained by local NOx measurements.
9) Lines: 484-486: From data analysis and model simulation it is hard to support these conclusions.

A general comment and a suggestion for further observations in this area: NO_2 measurements in remote area, where the concentrations are very low may have bias or instruments are below the detection limits for most of the times. There are several papers that suggest to use instrument that measure directly NO_2, using CAPS, LIF or CRDS techniques (actually CAPS are commercially available now) or, at least, chemiluminescence systems that uses photolytic conversion of NO_2 into NO, besides systems like those used in this work (model T200) that uses molybdenum oxide converters (Steinbacher et al., 2007; Dunlea, et al. 2007; Yang et al., 2004; Villena et al., 2012).

Minor comment:

Line 391: When in a time period, 67% of data are missed analysis and conclusion are very weak, so may be better not include that period in the analysis.

Reference

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