

Atmos. Chem. Phys. Discuss., referee comment RC1  
<https://doi.org/10.5194/acp-2022-319-RC1>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on acp-2022-319

Anonymous Referee #1

---

Referee comment on "Arctic tropospheric ozone: assessment of current knowledge and model performance" by Cynthia H. Whaley et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-319-RC1>, 2022

---

### REVIEW OF ACP-2022-319

The following points are mentioned on the ACP website for reviewing a manuscript:

1. Does the paper address relevant scientific questions within the scope of ACP?

Yes

2. Does the paper present novel concepts, ideas, tools, or data?

Not really. It's more like a review paper.

3. Are substantial conclusions reached?

To some extent

4. Are the scientific methods and assumptions valid and clearly outlined?

To some extent

5. Are the results sufficient to support the interpretations and conclusions?

Yes

6. Is the description of experiments and calculations sufficiently complete and precise to allow

their reproduction by fellow scientists (traceability of results)?

Not really since this is more like a review, summarizing previous work

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution?

No

8. Does the title clearly reflect the contents of the paper?

Yes

9. Does the abstract provide a concise and complete summary?

Yes

10. Is the overall presentation well structured and clear?

Yes

11. Is the language fluent and precise?

Yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?

Yes

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?

Yes

14. Are the number and quality of references appropriate?

No

15. Is the amount and quality of supplementary material appropriate?

Yes

**Main comments to the paper**

This is a well written and comprehensive paper assessing and reviewing the present state of knowledge of ozone in the Arctic troposphere. Despite that, this reviewer asks for major changes before the paper is accepted for publication. The reason for this view is given in the following.

The authors make many shortcuts when summarizing the results from existing publications. It seems as the authors are so focussed on trying to make an overall review that they leave out all the details and present everything as generally valid for the Arctic troposphere. The authors should investigate more closely the publications they refer to and clarify what conclusions are valid for certain sites and time periods only and to what extent the findings are relevant for the Arctic in general. The paper should provide more details of the previous work and should be less conclusive regarding the Arctic as a whole.

### **Specific comments to the content**

L122

With a reference to Walker et al. (2012) the authors claim that during summer, the dominant source of Arctic tropospheric O<sub>3</sub> is in-situ production in the Arctic, which in July is said to contribute more than 50% of O<sub>3</sub> in the Arctic boundary layer. This number seems high, and the authors are asked to provide more information what internal Arctic sources this includes as well as the uncertainty of this estimate. It seems like the Walker et al (2012) ref is based on a global model simulation with a coarse 4x5° resolution and on an adjoint model calculation for a few weeks in 2006 for Alert only. To what extent could these findings be generalized to the whole Arctic as the authors do in the paper?

This relates also to the statement in L145 regarding contribution from shipping to surface O<sub>3</sub> in the Arctic in summer. The Marelle et al. (2018) paper states that "Ozone production from shipping

emissions could be overestimated in the present study, since this is a known artifact of models run at lower resolutions”.

L148

The authors write: «Tucella et al. (2017) showed that background O<sub>3</sub> is influenced by emissions downwind of oil and gas extraction platforms in the southern Norwegian Sea.”

This is correct, but the authors don't provide any details, and thereby give the impression that this conclusion is generally valid for the whole Arctic boundary layer. In reality, the statement by Tucella et al. (2017) was based on two summer days in July 2012 and only for rather small domains. Furthermore, the ozone impact for the two small domains at these two days were less than 1 ppb on average. Thus, the authors should provide more details and rewrite their statement so that it does not give the impression being a general fact for the whole Arctic.

L170

“Interestingly, gradient studies at Barrow showed a positive gradient with height during O<sub>3</sub> depletion events (ODE) and atmospheric mercury depletion events (AMDE) suggesting that O<sub>3</sub> was removed at the surface due to fast photochemical reactions at or close to snow surfaces initiated by the release of halogen species (Skov et al., 2006).”

The link between ODE and AMDE has been extensively studied at Zeppelin/Ny-Ålesund. Why is this not mentioned?

L211-L212

“... simulations of the years 2014-2015 for comparisons to observations.”  
The authors should provide an explanation why they chose this early period while more recent measurement data are available.

L299

“In the high Arctic, there is very little diurnal variation in surface O<sub>3</sub>, most likely because the local and regional photochemistry is of limited importance most of the time and due to the

24-hour  
daylight during Arctic spring, summer and Autumn as well as the polar night during  
winter”

This is incomplete. The main driver of diurnal variation in surface ozone in other regions is the  
deposition to the ground and uptake in vegetation. Due to the inefficient deposition to  
ice/snow/water surfaces and the sparsity of vegetation, there is very little diurnal cycle in  
O<sub>3</sub> in the  
Arctic. This is the most important factor. The role of local photochemistry would anyway  
be very  
small in the Arctic due to the low levels of NO<sub>x</sub> and other precursors.

L320

“Regarding the more southerly Arctic and near-Arctic sites, a latitudinal gradient has been  
observed  
in the timing of the spring O<sub>3</sub> maximum. Anderson et al. (2017) found that monthly mean  
observed  
near-surface O<sub>3</sub> concentrations at background sites in Sweden from 1990 to 2013 had a  
maximum in  
spring, but the most northerly stations experienced their maximum in April while the more  
southerly  
ones in May.

Couldn't this simply be explained by the southerly sites being exposed more frequently to  
polluted  
air masses from the European continent in May? The phrase “near Arctic sites” is  
somewhat  
meaningless. The paper from Anderson et al. (2017) analysed data from all Swedish sites  
which  
includes stations down to nearly 55°N. It's not clear why these data are included in an  
assessment  
study of the Arctic troposphere.

L325

“In order to get an overview of the annual O<sub>3</sub> cycles at different types of Arctic surface  
measurement sites, we have calculated the monthly medians and interquartile range for  
the period  
2003-2019 for a series of sites. A map of the stations as well as their coordinates and  
elevation can  
be seen in Figure 4. Figure 5 illustrates the range of seasonal cycle behaviour observed in  
the Arctic  
at different measurement sites.”

Several of the sites in these figures are surely not Arctic. The Arctic circle is an easy  
boundary to use

for defining the Arctic region but less relevant for atmospheric studies. The conditions are highly different in the eastern/European region compared to the American sector. Besides, several sites in these figures are located much further south than the Arctic circle. The authors should outline this and explain why these sites are relevant for an assessment of the Arctic.

L341

"The largest differences between the stations are mainly found during the summer months, most likely due to the influence of local sources on photochemical O<sub>3</sub> production."

I disagree. The local photochemical O<sub>3</sub> production at the southerly sites are likely very small. The reason for the differences is most likely due to the distance to the main emission areas in Europe and the frequency of episodes transporting ozone from these areas.

L344

"Kårvatn in Norway has an unusual behavior with an O<sub>3</sub> maximum in March, possibly due to the local conditions: The site often shows a pronounced diurnal cycle in O<sub>3</sub> due to the location at the bottom of a valley that causes strong inversions leading to an enhanced impact of dry deposition at night on surface O<sub>3</sub> (Aas et al., 2017).

Why use this old reference when there exist several newer annual reports? And why use the Kårvatn site at all? It has very little relevance for assessing the Arctic.

L347

"Hurdal in Norway is included as an example of a more southerly Scandinavian non-Arctic station, which has an annual variation with a minimum in October while the more northerly stations have minima between July and September (Figure 5c), this difference may be explained by a stronger influence of local air pollution at Hurdal. At Hurdal, winter O<sub>3</sub> concentrations are particularly low, probably also in this case due to the influence of local emissions which in this period leads to the removal of O<sub>3</sub> by the reaction with NO."

What is the relevance of this station for the assessment of the Arctic? If the reason for the ozone differences is explained by local conditions at Hurdal, it seems meaningless to include this site in a paper assessing the Arctic troposphere.

L363

"... the occasional ODE has been reported there by Lehrer et al. (1997) and Ianniello et al. (2021)."

The Ianniello et al. (2021) paper was based on measurements down at the coast in Ny-Ålesund (40 m asl) and not on the Zeppelin Mountain. Either this ref should be removed, or it should be stated clearly that these findings refer to a coastal location. And why aren't more papers from Zeppelin mentioned? The occurrence of ODEs at Zeppelin has been extensively documented but none of these studies are mentioned in the present manuscript.

L424-440

There have been long-term measurements of VOCs at both Zeppelin and Pallas, so why are these data not mentioned? Presently, the assessment of VOCs in this paper is insufficient and should be significantly extended.

L462

"Methane has more than doubled since preindustrial times (from 0.8 ppmv to 1.8 ppmv)"

What year is this based on? And is it referring to the annual mean? Presently, methane levels have exceeded 2 ppmv in the Arctic.

L495

"All of these stations are located in the sector 95°W to 27°E meaning that regular soundings are lacking in a large sector of the Arctic."

Technically speaking, this is correct, but longitude values are difficult to imagine in polar regions, so it is recommended that the authors rephrase this sentence. In practice, the most obvious

lack of  
ozone sonde data is from the Russian sector + from Alaska.

L595

"...surface bromine/halogen chemistry needs to be included to simulate springtime surface O<sub>3</sub> at coastal Arctic locations (e.g., Villum, Alert, and Utqiagvik)."

Although the effect of the halogen chemistry is most visible at coastal station, this chemistry is also needed for a proper modelling of ozone elsewhere in the Arctic due to advection of the ODEs.

Fig 5

What is the rationale for grouping together so different locations as Hurdal, Zeppelin and Summit in this plot? This seems very strange and e.g., Hurdal has no relevance for assessing Arctic ozone levels.  
It should also be mentioned that ozone monitoring at Karasjok ended in 2010.

### **General comment regarding data use and acknowledgement**

The paper contains a vast number of references to previous publications, but there is very little information of how the data presented in this paper have been collected. This regards not only technical issues such as web addresses etc, but it also seems that the contribution from the various measurement data providers and institutions to this paper are absent. This represents a common modeller's attitude to the science: Open-source data could apparently just be downloaded and used without acknowledging the years of experience at the data providers' institutions.

Some data from the Finnish site Pallas are included, but why are there no co-authors from FMI? And why are there no mentioning of the long time series of VOC measurements from this site? Ozone data from Esrange in Northern Sweden are included, but no acknowledgement to the data provider

IVL is given and no IVL personnel are included as co-authors. The same holds for measurement data from Norway: There is no acknowledgement to NILU, and no NILU people are included as co-authors. Just a reference to an old monitoring report is given: Aas et al., 2017. And why are the VOC measurement data from Zeppelin not mentioned?