Authors' response

To acp-2021-527-RC2 (23 Aug 2021): We thank the anonymous referee #2 for their comments. We will address the specific comments in detail in our final revision of the manuscript. Here, we shall give a brief response to questions where it seems appropriate. We have already tended to the minor, technical comments.

Specific comments

- This paper is easy to follow and well written with a very thorough introduction and a good justification for why this analysis is appropriate to Northern Fennoscandia. It does not go as far as recommending how it could be used in other global regions, but justifies that this analysis is particularly applicable to these remote areas with large seasonal variations. Thank you.

- For those unfamiliar with reanalysis products, we assume that these 3 products are the only ones available, 2 global and 1 regional. Maybe the range of other products could be introduced and justify why these 3 were chosen. Thank you for pointing this out. Following the advises of referee #1, we add a paragraph in the final revision regarding other available reanalysis products and why these three (MACC, CAMSRA, CAMSRAQ) were chosen.

- The word Biome in the title is never used again, maybe just ozone risk products is needed? Or vegetation ozone risk products? We elaborate on the title and change it to *Technical note: Quality assessment of ozone reanalysis products and gap-filling over subarctic Europe for vegetation risk mapping*

- There seemed to be no explanation of how missing data during a forest fire period may be harder to fill than during a more normal period. Maybe this technique needs to be applied when something serious like a forest fire impedes entry to the station and data is lost.Thank you for this remark.
  As we described, gap filling is usually done by using mean values from the same period from previous years or by using mean values from the same time of day on previous days. If forest fires are rare, those mean values will not be good candidates for gap filling. Data from a reference station selected based on a high correlation factor alone is not sufficient, because a correlation does not account for systematic offsets or the transport of pollutants.

  We add a paragraph in the appropriate section.

- The analysis of the seasonality is interesting in itself- showing how it is still hard to model and predict. Indeed, this was the most astonishing result of our analysis which has implications even beyond vegetation risk mapping. The ozone concentration affects the oxidation capacity and halogen chemistry of the atmosphere. Hence, e.g., the rate at which mercury is processed and deposited in the Arctic (see Section 2.5.1.3. in the AMAP Assessment 2011: Mercury in the Arctic).

- Figure 6 is the final and most important figure. It should be put before the conclusions, otherwise it may be missed! Thank you this is probably subject to the final typesetting and cannot be taken care of at this stage.

- On line 278, you state that your devised method performs better (78% accuracy) than CAMSRAQ at nearest neighbour. This is very important and is stated in the abstract too but you could compare it to the other methods too. How much better is it? Given that we already confirmed a high correlation with observations at Pallas. The only other methods in accordance to ICP Vegetation, would be to compare with the data from Pallas directly without further processing (accuracy 69 %) or comparing with, e.g., the July 2019 at Svanvik (72 %). We will include these in the final revision of the manuscript.
  Thanks to your comment, we went through our calculation of mean accuracy again and found a shortcoming. We determine, the accuracy of our method to 76 % and for CAMSRAQ to 80 %. We account for this in the final revision and rephrase our abstract and conclusion accordingly. This result is actually more consistent with our expectations. As we have not taken chemical transformation and transport into consideration in our method.
  This mistake was unintentional and does not affect the overall content of the manuscript.

Minor comments

- Line 19 - O3 "acts" as a potent greenhouse gas Done.

- Line 92 "data taking" and line 272 "data taking" should be replaced by "Measurements" Done.

- Figure 3- The generalized ozone climatology shown as "a" gray band represents—— On average, all reanalysis products "underestimate" [O3]. Done.

- Line 172- Tromsø "where" [O3] Done.

- Line 188 - larger negative deviation from "observations" in DJF and MAM. Done.

- This indicates that CAMSRAQ might have different issues depending on the region of interest. "Different issues or different uncertainties- maybe this could be elaborated?" Thank you for pointing this out. We elaborate on the sentence and rephrase: *This indicates that underlying uncertainties in CAMSRAQ manifest differently at higher latitudes. Enhancements that lead to better model performances in mid latitudes, hence, do not necessarily affect results in the Arctic and subarctic in the same way.*

- Conclusions Line 248: You say "We confirm that a high spatial and temporal resolution, state-of-the-art mechanistic removal processes (land–atmosphere–ocean), and assimilation of in situ observations at ground-level are a must to constrain

reanalysis products," but have you really confirmed it or explained that the land–atmosphere–ocean interaction is applied here? You are right. We have, in fact, not proven that in particular. What we confirm is that *the assimilation of vertically ozone profiles, if applicable ground-level observations, and a higher spatiotemporal resolution lead to better constrained reanalysis products.*

- Line 263 – "updates may "also" play a role" Done.