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Comment on acp-2021-341

Anonymous Referee #2

Referee comment on "Overview: Recent advances in the understanding of the northern Eurasian environments and of the urban air quality in China – a Pan-Eurasian Experiment (PEEX) programme perspective" by Hanna K. Lappalainen et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-341-RC2>, 2021

I reviewed, and hence the following comments are targeted for, the abstract and the atmospheric component of this manuscript.

For a lengthy review paper like this one, a table of contents would make it easier for the reader to navigate.

The abstract needs to be rewritten. All it shows right now is what was done in this paper, followed by a generic sentence "although the scientific knowledge in these regions has increased, there are still gaps in our understanding of large-scale climate-Earth surface interactions and feedbacks", which tells nothing. You can put a sentence like this in many review papers notwithstanding the topics. The authors need to show a couple of most noteworthy advancements and gaps in knowledge of understanding the key processes in the Arctic-boreal regions.

With the impressively long list of authors, the review naturally includes a great ensemble of studies spanning different spheres. However what is lacking is the connection that integrates the cited studies and demonstrates how those studies serve to advance our knowledge of the atmospheric processes in the Arctic-boreal region.

PEEX tackles the Arctic-boreal region (lines 89-90), and the manuscript was supposed to summarize "results obtained during the last five years in the Northern Eurasian region" (lines 91 – 92). What is the authors' definition of "northern Eurasian region"? The one monitoring site and some of the air quality studies from China cited in the manuscript took place in a city of ~32°N latitude. Is that counted as within the "northern Eurasian region"? That city is in a different atmospheric circulation regime from those northern European and Russian cities and monitoring sites. I agree wholeheartedly that air quality in China and their influence on the Arctic should be studied. However, the inclusion of work from a

monitoring site from 32°N latitude in East China seems more like a happenstance than a strategic choice as the inclusion of studies from other locations and areas in the review. I also think the statement of SORPES being the “first such station in China” in need of fact-checking. There are sites in Hong Kong that have been running for decades. There are long-term sites operated under China National Environmental Monitoring Center. There are some sites on city or regional levels such as the ones in Guangzhou (Liu et al., 2013, ACP), which showed data from 2010, and the Sichuan Ecological Environment Monitoring Center from the study by Zhao et al. (2019, Atmos. Pollu. Res.) focusing on Southwest China showing SO₂ and NO₂ concentration data from 2008 to 2018. It is likely that I have not exhausted the list of long-term monitoring sites preceding SORPSE in China.

The review is written often times in rather general terms with no key, specific findings from cited works. To make my point, here are a few examples. In lines 652-653, the result cited from Mikhailov et al. (2017) was that “i(I)n summer, precipitation is removing the pollutants from the air and leading to relatively clean atmospheric conditions this region”. What is so revelatory here? The scavenging effect of precipitation is commonly known, or did they mean to emphasize the dominant effect of wet deposition of key soluble pollutants that caused smog in the region? In the “Methodological and model developments” section (starting in Line 718), they cited Dada et al. (2018) for “a new classification method for atmospheric NPF”, and cited Zaidan et al. (2018b) for “a mutual information approach to identify key factors contributing to the NPF”, but never stated what those new approaches really were. I understand that a review needs to be succinct but I doubt there is absolutely no way to succinctly explain those new approaches. In lines 828 – 834: it is not clear to me what specific information I can gain from these generic statements. In line 872, the authors stated “the longest urban continuous record is from the SORPSE station in the Yangtze River Delta” and they cited Qi et al. (2015) for the work. The study presented a 2 year worth of dataset. Please explicitly state the length of the dataset for clarity. Following that statement, the authors reviewed the key results: “NPF was in general the largest source of clusters and nucleation mode (<25 nm) particles, while traffic contributed to all the size ranges and dominated both cluster and nucleation modes on haze days. Aitken mode (25–100 nm) particles originated mainly from local emissions, with additional contributions from regional and transported pollution as well as from the growth of nucleation mode particles. Regional and transported pollution were identified as the main source of accumulation mode (>100 nm) particles” (lines 875-880). Aren’t these all rather universal, basic knowledge for a megacity? Similar results have been shown in numerous papers over the past decades. What is unique pertaining to the location? What is original about these points? Immediately after, it was the same problem with the review of Bai et al. (2018a) on the PM and O₃ link, which stated that “the contribution in chemical and photochemical reactions was found to be prominent in summer”. It is not clear to me what readers gain from a statement like this. In lines 909-910, it was stated that “i(I)n line with the proposed mechanism, Shen et al. (2018) showed that aerosol optical properties evolve clearly during the development of multi-day pollution episodes in heavily polluted regions”. Again, what exactly in this result contributes to understanding the BL-PM link there? More importantly, how are all those results reviewed here contributing to understanding Arctic-boreal processes?

More specific comments:

- Lines 529 – 531: N₂O came out of nowhere and no references were cited.

- There was spillover between sub-sections. Examples: under Northern Eurasian CO, they talked about CH₄ again (line 569). Before the review on black carbon starting in line 668, they already reviewed a bit about black carbon in previous subsections.
- The authors had the tendency to use adjectives to describe results, such as “this amount had decreased remarkably in the Moscow urban environment” (line 675). How much is “this amount”? What amount qualifies as “remarkable”? Be quantitative.
- Lines 794 -: The authors started with stating “new atmospheric aerosol instruments have been deployed in the PEEX area”, but then went on talking about a new laboratory (AHL). They then merely mentioned that “the state-of-the-art instruments” were used. It was confusing. I associated “new” with “novel”, instrumental advancements. But none of the following information suggested that.
- Lines 811-813: Were the authors suggesting that human influence suppressed NPF?
- Lines 820 – 823: Did this development improve model simulation of aerosol-radiation and -cloud interactions?
- Some references are missing, such as Wang et al. (2017a, 2019).
- Lines 947 – 952: references are needed.
- The manuscript can use a good amount of editing.