

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2021-437

Anonymous Referee #1

Referee comment on "Integrated airborne investigation of the air composition over the Russian sector of the Arctic" by Boris D. Belan et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-437-RC1>, 2022

SUMMARY

Belan and co-authors present gas and aerosol measurements, performed from an airborne platform in the Russian sector of the Arctic. As the authors stated, this sort of measurement in the Russian Arctic are extremely rare and, as a consequence, very valuable. Hence, will provide the needed reference for evaluating the performances of global models in a relatively unknown region of the Arctic. Despite the scientific interest and need for these measurements, the manuscript is very confusing, in its language, structure and objectives.

My main concern is related to the choice of the journal and the objective of the manuscript. The measurements are indeed a novelty, not from the technical point of view, but rather for the location. Most of the deployed instruments are commercially available or already described and validated in previous papers. The description of the atmospheric results is very chaotic and no clear conclusions can be drawn. If the manuscript aims to provide a general overview of the experiment, I suggest reducing the discussion to the grand average of each variable measured during the campaign and contextualizing the vertical variability as a function of the synoptic situation and air transport. I can easily see a resulting overview paper being submitted to ACP. The detailed description of each variable during each flight should be done in separate articles with a narrower scientific question. To promote the use of this dataset for modelling purposes, a technical description of the measurements could be, eventually, provided into a separate descriptive manuscript potentially submitted to Earth System Science Data. If I am correct, data must be, then, publicly available.

Considering the importance of the dataset, I hope that the authors will follow my suggestions and reconsider their strategy for publication. In its current status, the manuscript is not suitable for publication on AMT.

SPECIFIC COMMENTS

L45-50: The paper is already long enough. This part is very generic and not needed.

L51-55: "Spontaneous question", remove all the references, there are more words in the reference list than in the actual statement. Not of smooth reading.

L55-61: This part is unintelligible. Rephrase

L62-64: I would always suggest avoiding the use of a long list of references. Like it is, this sort of listing does not help the reader to identify a specific citation with a specific result, becoming, as a consequence, not useful. Provide one to three reference for each statement or scientific information, not more. This issue recurs along the entire manuscript.

L72-79: It is written "there are no systematic observations of the vertical distribution of gas and aerosol components of the atmosphere." In the following lines the vertical measurements of gas and aerosol are described. The writing is not coherent.

L88: replace "appeared" with "established".

L118-122: insert bullet points

T1: adjust legend. Range in ppm?

L181: black carbon not in capital letters

L182: I think you should use the equivalent black carbon (eBC) following Petzold et al. 2013.

L183: what is "IAO SB RAS"?

F4 is not needed, and difficult to interpret

L181-213: usually filter based transmission photometers calculate eBC mass from absorption or attenuation coefficient using the mass transmission or absorption cross section. Besides the fact that the authors do not specify which MAC or MTC values they use, they also state "The number concentration of BC particles in the air is calculated by the software". I am curious to know how the number concentration was calculated.

L215-221: are scattering coefficient corrected for truncation error?

L228: "This method is traditional and has been described many times in the literature". Provide reference.

F5 not needed, hard to read. See comment on Figure 4

F7 not needed. Does not provide useful information for scientific scope.

S2.4-2.5: These two sections could be merged. S2.5 does not provide enough information on the specific sensors, while I genuinely do not understand the description of CompaNav-5.2 in S2.4.

S3.1: irrelevant to the understanding of the manuscript. A short statement on planning change could be introduced in a different section, but it does not need a dedicated chapter.

L355: typically

L357: "The minimum height was 200 m above the sea and 500 m above land." Repetition.

S3.3: It is important to describe the synoptic conditions of the flight. However, this day-by-day report is unnecessarily long and tedious to read, and could be easily summarized by a table. The authors could then simply describe the difference between the various influence periods.

F9: revise this figure, make it easier to read, include flight pattern or interested region.

L425: "This is understandable, since the concentration of carbon dioxide in the atmosphere is increasing all over the world." I would expect some better wording and conclusion.

F10: adjust margin and size of the panels

L422: "However, this is due to the transfer from the continent. To check this, initially unplanned sensing was carried out over the Bering Sea. It confirmed this conclusion." The authors must provide evidence of what is stated. The reader could not verify this information, since is not shown.

F11: provide real legend. It might be one line of text!

F12: give more info on the backtrajectories.

L470: "This conclusion is, in principle, clear from the above synoptic maps (Fig. 9) and follows from the constructed back trajectories (Fig. 12)." Back trajectories are not discussed. So, I am not sure, what it is clear.

F13: to be removed. Not usefull.

L479-482: I do not see the reasons for mentioning that there is a source of methane that is not detected.