

## ***Interactive comment on “Characterization of Transport Regimes and the Polar Dome during Arctic Spring and Summer using in-situ Aircraft Measurements” by Heiko Bozem et al.***

### **Anonymous Referee #2**

Received and published: 16 March 2019

This work makes use of the atmospheric tracers CO and CO<sub>2</sub> measured on two NET-CARE flight campaigns together with 10-day back trajectories to describe air mass transport into the high Arctic during spring and summer. The authors find distinct transition zones between the mid-latitudes and the polar dome for the two seasons based on tracer gradients. The tracer derived polar dome boundaries are subsequently applied to aerosol number concentration data. In addition, the authors explore different transport pathways of air masses into the Arctic using a previously published phase-space approach that relates the maximum change in potential and absolute temperature along the trajectory.

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This manuscript is very comprehensive and represents a novel approach to localize the polar dome boundary and to characterize air mass transport into the Arctic. The work shall definitely be published with revisions as described below.

### General remarks

To give the findings more relevance for Arctic atmospheric research it would be of high interest, whether the derived polar dome boundaries are representative for other Arctic sectors and years as well? Or would each field campaign have to do their own analysis following this example? So a discussion on how far the results can be generalized is needed.

There is no discussion on the uncertainties of the variables along the trajectory such as potential and absolute temperature. Particularly the vertical location of the polar dome boundary would be subject to the uncertainty. At least some discussion on how the ECMWF input data compares to the in-situ measurements should be added.

The application of the polar dome boundary to aerosol data is intriguing and it would be very interesting to know how other aerosol properties relate to the boundary. Adding such information to the manuscript would make it even longer than it already is. I would recommend exploring whether a “part 2” manuscript on aerosols could make sense.

The manuscript is partly repetitive and the abstract reads almost like an introduction. Both the abstract and the whole manuscript should be shortened. There are some recommendations in the attached PDF.

### Specific comments

p. 7, l. 15: What is meant by “The stability of the instrument. . .”? Is this the accuracy? I wonder whether none of the instruments has been described before and whether such an extensive description here is necessary?

p. 8, l. 19: What is a “very stable stratification” compared to a stable stratification? Is the ECMWF data the analysis or re-analysis data?

Figure 6: The scales can be enlarged in both panels: 200 for a) and 415 for b). This way, more details would be visible. The legends will find place somewhere else. ... What is the purpose of the figure except discussing the seasonal cycle? If a direct comparison of the NETCARE data with the stations is the goal, there should be zooms for the short periods of time. Currently, one cannot see much because the symbols are so large and cover everything. Why is Mace Head chosen as a reference?

Figure 8 b: Why does the potential temperature increase below which CO maximum concentrations occur with decreasing latitude? Some explanation is needed.

p. 28, l. 6-18: This paragraph is a result and should be moved to the results section instead of being added to the discussions.

p. 28, l. 10-13: How can you infer that secondary aerosol formation is responsible for the concentration difference in the blue area? Based on the information provided, those particles are either between 5 and 20 nm or > 100 nm. For the first option, I find it difficult to believe that there is evidence to relate the increase in 5 to 20 nm particles to secondary aerosol formation based on the AMS and ALABAMA measurements in Willis et al. (2017). These instruments do not cover the relevant size range. If the second option is true, the information on SOA contributing to particle growth to form CCN does not make sense, because particles are already in the CCN size range, even for low supersaturations. What is the explanation for the difference between panel a and b? And what is the relevance for CCN? Please revise the statement.

Figure 15: The way the aerosol results are presented with the colored boxes makes it difficult to see the shading.

Technical comments:

Please the comments in the attached PDF manuscript.

Please also note the supplement to this comment:

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<https://www.atmos-chem-phys-discuss.net/acp-2019-70/acp-2019-70-RC2-supplement.pdf>

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2019-70>, 2019.

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