

The manuscript entitled 'Long-term Changes in Fog Frequency at Swedish Airports and its Potential Drivers', submitted by Sporre et al. focuses on investigating the time changes in fog occurrence at 12 Swedish airports in 1970s–2022. For their analysis the authors use the visibility measurements and supporting meteorological data (air temperature, pressure, relative humidity, wind speed and direction) measured directly at the airports, ambient SO_4^{2-} concentrations measured at five stations observing air quality (situated elsewhere in Sweden) and European SO_2 emissions. The outcomes of the study are interesting, the topic is within the scope of ACP and in interest of this journal readership including the fog community. Nevertheless, there are major concerns to be addressed before the possible accepting the manuscript for publishing.

My major concerns are as follows:

1. The hypotheses are lacking. They should be included to assist the readers understand what the authors were expecting and why.
2. The manuscript is deficient in statistical analysis. The description presented is mostly qualitative and not quantitative. The methodical approach should be enhanced and made more robust.
3. The input data should be described with respect to methods and instruments used, and QA/QC procedures mentioned. Furthermore, the tackling of missing values should be presented.
4. The considerations related to fog condensation nuclei are limited to sulphates, whereas it is known that nitrates as well function as anthropogenic condensation nuclei. This might be included in the performed analysis as the data on ambient NO_3^- concentrations should be also available (or it should be at least mentioned, why the authors do not account for them). Furthermore, close to the seacoast, NaCl from sea spray acts as the most effective natural condensation nuclei. This might be used for consideration on fog occurrence at the airports near the seacoast.
5. The authors refer to radiation and advection fog in their manuscript, though it is not clear how they exactly know what type of fog it is. The difference between radiation and advection fog types is theoretically known, though it is not clear how the authors might possibly know what type of fog is relevant for what airport based on the long-term record. The authors should either clarify or be careful with these labels.
6. In Conclusions it must be specified what is meant by 1–8% of annual fog frequency (are these hours?).
7. Finally, I would suggest the authors to focus on main findings and not to describe only what is presented by the plots. If some hierarchy of results is included it might help the readers in better orientation in the text and better understanding of the findings. This might be especially appreciable for non-Swedish readers.