

Interactive comment on “Sensitivity of free tropospheric carbon monoxide to atmospheric weather states and their persistency: an observational assessment over the Nordic countries” by M. A. Thomas and A. Devasthale

Anonymous Referee #3

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This study links the pollution transport into the Nordic countries seen by AIRS CO anomalies to eight typical circulation and meteorological states near that region (from ECMWF's ERA-Interim data). I believe the subject is very important, and I particularly like the discussion on the different periods of persistency of the circulation/weather systems (3, 5, and 7 days). However, I have some major concerns and I recommend publication if the authors can address them.

1. It is known that CO is a tracer and the transport of pollutants is correlated with

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the wind patterns and the motions of air masses, as the authors summarized in the Introduction. So in this sense, the result of this study is intuitively known knowledge to many readers. It would make this study more significant if more challenging issues are examined. One example is that it would be very interesting to know whether or not these pollution transport events actually affect the air quality of the Nordic countries. This would require near surface in situ measurements of pollutants (not necessarily CO). See study by Lin et al., 2011.

2. Another challenging issue is the vertical transport of pollutants. This study used AIRS CO at 500hPa (reason explained in the text), but winds are shown at 850hPa. Would the conclusions be different if the winds at 500hPa were used? Would vertical transport from nearby areas play a role in any of the eight meteorological states to explain the CO anomalies?

3. The temperature and water vapor anomalies (from AIRS?) are shown, but not clearly integrated nor sufficiently explained to support the focus of this study. Perhaps they will help in Sect. 4 to explain the sources of the transported pollutants.

4. “The fate of the pollutants . . .” were emphasized, but not discussed. I believe this discussion should involve: subsidence of the pollutants, outflow from the study area, and/or CO removing processes.

5. I suggest using larger area maps, as shown in the supplemental file, and over-plot a box indicating the study area. This way it is clearer where the pollutants are from. Similarly, if larger AIRS CO maps were used, it would be more obvious where the pollutants are from. There are too many places the authors had to “speculate” where the sources of the transport by using terms: “most likely” or “this may be because the air masses . . .”

6. I suggest listing the number of incidents in each year for all the eight states (in a table?), not just the normalized frequency. Are these all the cases that fit to the eight states, or are they selected as typical cases?

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7. This manuscript reads, in general, as qualitative descriptions. More quantifying is needed. Examples are: “higher than normal”, “colder than average temperature”, “a much lower trajectory”, “above normal CO concentrations”, “based on averages”, etc.

8. Sect. 3 is difficult to read. More care and details are needed to be clear, e.g., page 9257 lines 3-17 and page 9259 lines 1-6.

9. “Figures 1-3 show . . .” on page 9256 line 2 was introduced without giving descriptions. Should introduce in the later paragraphs where they were discussed. Also, Fig. 3 was discussed before Fig. 2.

10. The study area should be defined consistently with the figures. Should they be 40N, 42N, or 45N, since they were all used in different places?

Reference: Lin, M., A. Fiore, L. W. Horowitz, O. R. R. Cooper, V. Naik, J. S. Holloway, B. J. J. Johnson, A. M. Middlebrook, S. J. J. Oltmans, I. B. Pollack, T. B. Ryerson, J. Warner, C. Wiedinmyer, J. Wilson, and B. Wyman, (2012), Transport of Asian ozone pollution into surface air over the western United States in spring, *J. Geophys. Res.*, 117, D00V07, doi:10.1029/2011JD016961.

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