Atmos. Chem. Phys. Discuss., 14, C2037–C2040, 2014
www.atmos-chem-phys-discuss.net/14/C2037/2014/

Atmospheric Chemistry and Physics Discussions

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 #2

Received and published: 2 May 2014

Review of Thomas and Devasthale: Sensitivity of free tropospheric carbon monoxide to atmospheric weather states and their persistency: an observational assessment over the Nordic countries

The paper by Thomas and Devasthale links the distribution of satellite observed CO from AIRS to the synoptic regimes over northern Europe. They compare the data from 11 years of observations with the weather states based on a climatology (Chen 2000 and Linderson, 2001) and manual inspection of weather reports. On the basis

of ERA Interim wind fields (850 hPa) the states are classified by four wind directions, cyclonic/anticyclonic conditions and the NAO index and their persistency is considered (3,5,7 days). They show multiannual monthly averages from AIRS (CO) at 500 hPa for the respective weather states and deduce distinct patterns which become more pronounced with the persistency of the respective weather regime. According to their analysis positive deviations from the mean state are largest for south easterly wind and negative for north easterly conditions. In prinicipal I like the idea of linking weather patterns to tracer observations to learn something about tropospheric transport. The current manuscript tries this, but there are some points, which are not adequatly treated: The seasonality of emissions is not included at all, but could lead to opposite effects for the same weather regime in winter and summer (think e.g. of biomass burning only occurring in summer in distinct regions). Thus, the same weather regime can have totally different effects depending on season. As far as I can see, this point is not considered correctly by the method. I also do not understand, how the weighted mean is constructed (see below). There are also no measures of variability, uncertainty or significance of the mean deviations. The results are partly a bit surprising and look artificial and should be discussed deeper with regard of potential emissions and the source regions. In fact the weather regimes (wind fields) are partly discussed as being trajectories (see also below). In total I find the general idea and approach interesting and publishable, but not in the given form. The authors should revise their manuscript and consider the points as indicated below.

Major points: As described above, I see the need to subdivide the analysis by seasons or at least prove, that there is no seasonality e.g. in the emissions and source regions. The latter could lead to wrong conclusions and affect the weather regimes in different ways. The weighted seasonal cycle does not cover this effect. It is not clear, who this weighting is applied to the CO. I suggest, to use a simple multiannual seasonal cycle of 12 months and substract the respective monthly mean from the weather composite. No weighting is necessary then. Please show a Figure of the respective cycles. This should be done as mixing ratio (ppbv) for comparison with other data.

Fig.5,6,7 (and related discussion): In addition to a potential bias from seasonally varying emission regions: What is the significance and variability of the anomalies shown here? You could plot the relative variability. Is the deviation within the variability of the monthly multiannual mean? Please become a bit more quantitative here. I think this is important, since the patterns withe regard to the persistence are quite variable (e.g. Fig.5, SE). Please change the unit to ppbv, since this allows a better comparison with other climatologies. It is further stated, that the SW -case is cleaner, since one gets a flow mainly from North America north of 45N. This is speculative and would still include the source regions from large industrial areas. Also in the near field south westerly flow includes pollution from Paris, UK, the Netherlands, which are strong pollution emitters. I do not understand the SW-result. Also stagnant high pressure conditions (Fig.6) should lead to pollution accumulation, which seems however to be much lower compared to Fig.5. This is strange, since I would expect stronger dilution (also of polluted air masses) under the more dynamic conditions in Fig.5. than for stagnant accumulating pollution.

p.9255, l.5: Please provide a Figure of the averaging kernel since CO emissions at the surface can potentially affect the 500 hPa data (this seems to be the case e.g. in Figure 5 for UK - NE-case).

p.9256: l.17, ff.: How do you classify the regimes? The daily averaged ERA Interim winds and MSLPs give some value in the center of the study area as well as the pressure. When did you test for antiyclonic/cyclonic conditions, when for the wind directions? Even if the classification is given by Chen and Linderson, you could add a sentence on the main criteria.

Fig.8: The simple averages shown in Fig.8 (which are most likely over the whole areas shown in Fig. 5, 6, 7) are a coarse measure, since the averaging area is relatively big and includes different air masses. Why not analysisng the CO deviation in a smaller test area over central Scandinavia? Does it make sense to analyse both cold and warm sector of a cylonic system in the same average?

Technical: Figure 1: Please indicate the units for wind speed and colour bar.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 9249, 2014.