Norrköping, 2014-07-11

We thank the referee for her/his constructive comments and suggestions that lead to the improvement of the manuscript. Please find below point-by-point reply to your comments. Also, please have a look at the revised manuscript for updates.

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 principal, I like the idea of linking weather patterns to tracer observations to learn something about tropospheric transport. The cur- rent 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.

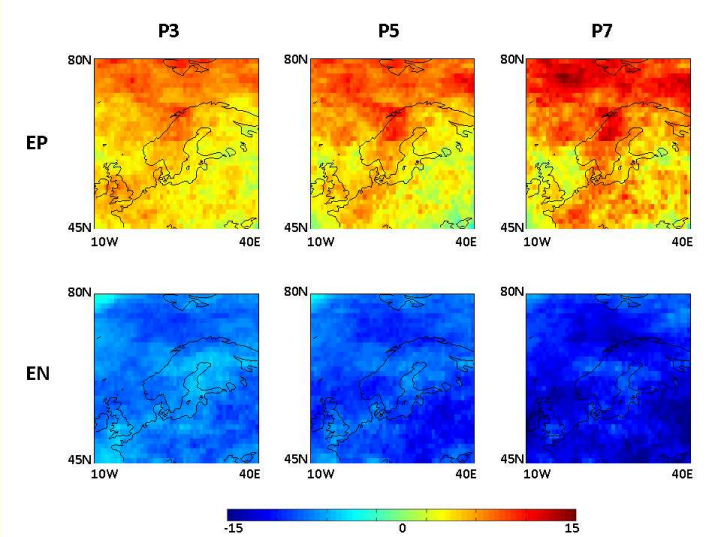
We find it encouraging that the referee liked the general idea and that our analysis approach is interesting. Below we try to address the concerns raised by the referee. We also kindly ask referee to take a look at our response to other referees for further clarifications.

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.

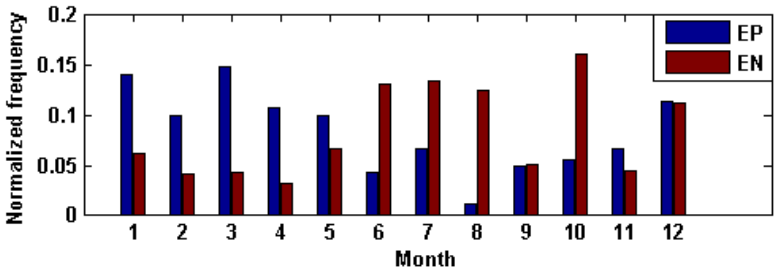
We are confident that we remove seasonality while presenting the results. Here we step-by-step demonstrate this for the NAO case. This is also a case where the monthly distribution of samples is most biased.

a) Let's first assume that the numbers of samples drawn from each month when NAO was in positive and negative phases are more or less similar. Then we could simply take the difference of two composites; one with CO concentrations observed during positive (negative) NAO and the other showing the annual mean climatology.

b) The resulting anomalies (in ppbv) are shown in figure below. Notice that there is striking difference between anomalies during positive and negative phases of the NAO. The anomalies are highly positive (negative) during positive (negative) phase of the NAO.



c) If we carefully look at the monthly distribution of samples (shown below), we see that the negative phase is predominant during winter half year and positive during the summer half. So the resulting anomalies shown above are clearly due to contamination from the seasonal cycle which we know peaks during winter-half year (e.g. see response to one of the questions from referee #1). This leads to overestimation of NAO influence.



d) To address this, we use revised climatology where we weigh CO concentrations in each month by the factor based on the monthly distribution of samples. This climatology is computed as follows:

$$C = \frac{1}{12}\sum_{i=1}^{12} w_i * c_{mc\,\lim}$$

where i is month, w_i is monthly weight (based on figure shown above), c_{mclim} is monthly climatology of CO.

e) Finally, subtracting this climatology from the NAO composites results in anomalies that are free from the influence of seasonality (see new anomalies below).

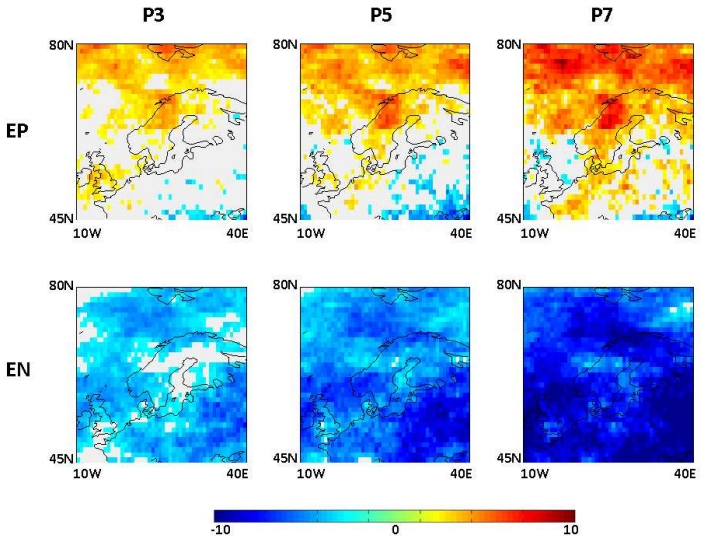
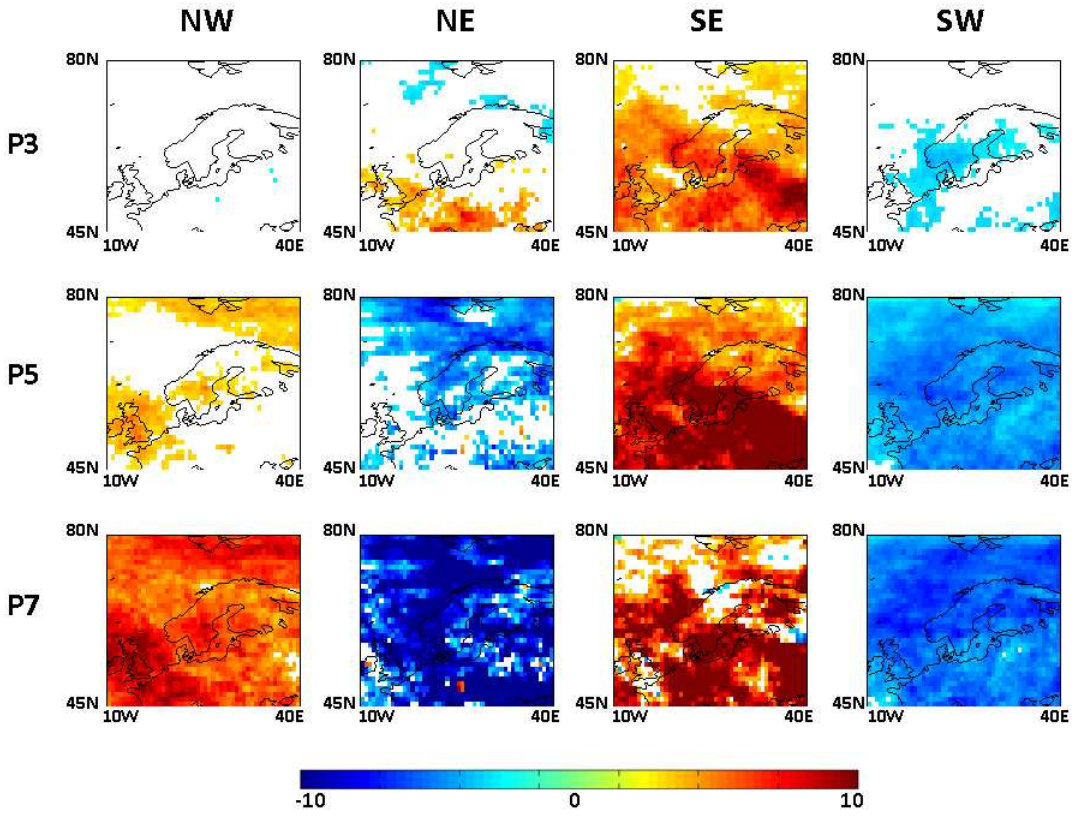
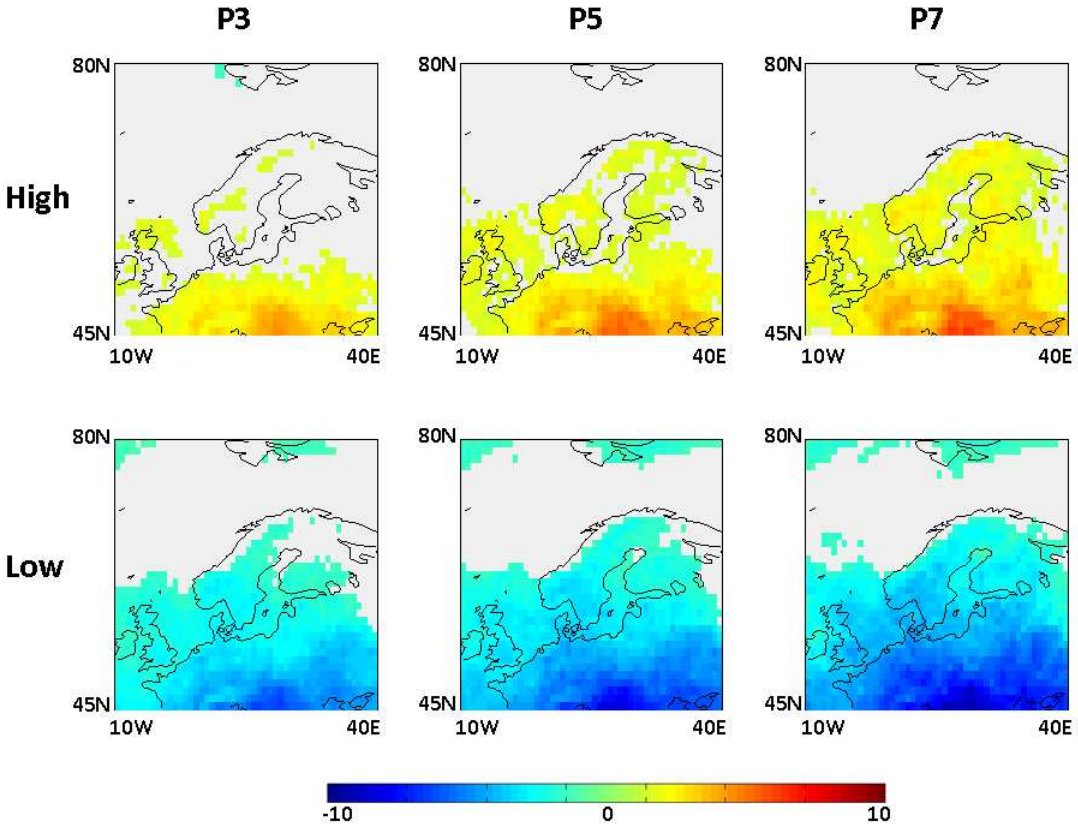
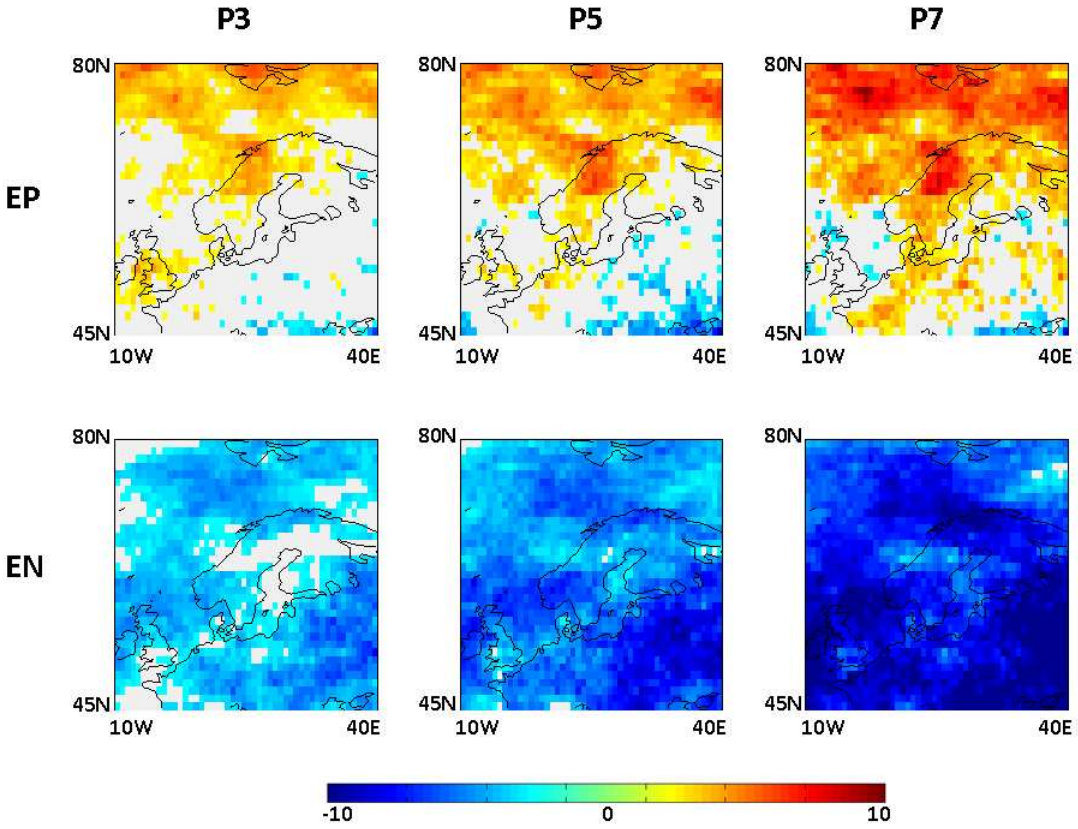


Fig.5,6,7 (and related discussion): In addition to a potential bias from seasonally varying emission regions: What is the significance and variability of the anomalies shown here? You could plot the relative variability. Is the deviation within the variability of the monthly multiannual mean? Please 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).

To address concern regarding significance and variability, we have now shown only those anomalies that exceed one standard deviation (and thus are significant). All figures showing CO anomalies are revised. The tendencies in CO anomalies (in ppbv) are now clearly visible in new figures as shown below.


NW
NE
SE
SW
P3
P5
P7
80N
45N
10W
40E
-10
0
10


P3
P5
P7
High
Low
80N
45N
10W
40E
-10
0
10



Please change the unit to ppbv, since this allows a better comparison with other climatologies.

The units are now changed to ppbv wherever applicable throughout the revised manuscript.

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 west- erly flow includes pollution from Paris, UK, the Netherlands, which are strong pollution emitters. I do not understand the SW-result.

We were also initially surprised with the SW result. As mentioned to Referee #1, we think the main reason for this is the mixing in the eastern Atlantic. If we carefully look at the larger wind maps, the southwesterly winds are mixed with much cleaner air masses by the Atlantic gyre. Also please note that in the other 3 cases (of wind directions), the transport pathway is relatively speaking much more direct.

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.

The effect of stagnation would be highest closest to the surface where ground sources will have first order impact, but in the free troposphere, the distribution is much more homogenized and the large-scale circulation will play bigger role. There are three factors we need to take into account while interpreting the results, namely the pollution concentrations at the source region, distance to the source regions and wind patterns that will either enhance or dampen the CO concentrations. If we carefully consider these three together, it is obvious that in the SE case, the winds from heavily polluted central and eastern European regions have shortest distance to transport the pollutants over the study area, resulting into highest positive anomalies. For the NW and NE cases, anomalies are of comparable magnitude to that of high MSLP case during 3P. But as the persistency period increases, we see that in the NW case where the airmasses are coming from heavily polluted North American region, the anomalies increase significantly as expected. Compared to these SE and NW cases, the airmasses drawn from over the European continent are mixed heavily with the relatively cleaner air of the northernmost parts of northeast Atlantic Ocean.

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).

Unfortunately, the averaging kernels are not directly available in the data set that we used (Standard Daily Level 3 Version 6 product). However, there is plenty of evidence available in the published documents to point out that the retrievals at 500 hPa are not affected by the surface CO (Warner et al., 2007, 2010, 2013; Yurganov et al., 2008). It is also well-known that the information content of AIRS CO lower tropospheric retrievals is peaking at 500 hPa during colder months and colder surfaces, further precluding surface contamination (Yurganov et al., 2008). Finally it is worth mentioning that that the L3 products are quality controlled since only retrievals with information content derived from AIRS spectra are reported in this product (http://disc.sci.gsfc.nasa.gov/AIRS/documentation/v6_docs/v6releasedocs-1/V6_L2_Quality_Control_and_Error_Estimation.pdf).

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.

We classify the regimes as follows:
Based on the literature review and synthesis of weather reports from SMHI, we selected eight weather states that dominate free tropospheric variability over the Nordic countries.
a) In case of the first four weather states representing wind flow from different directions,

we chose the center (55N-60N, 12E-20E) of the study area (40N-80N, 10W-40E) to average daily wind speed and direction at 850hPa. Based on these averages we selected days when a particular wind direction prevailed and persisted for atleast 3, 5 and 7 days. The same procedure is applied for selecting anticyclonic and cyclonic conditions based on average MSLP over the center of the study region. The classification algorithm intuitively excludes overlapping dates.
b) In case of remaining two weather states, the selection of days is based on NAO indices and, therefore, no averaging over particular region is required.

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?

Fig. 8 is mainly intended as a measure to evaluate/compare results from the chemistry transport model in future. We could in principle revise this figure for any sub-region, but for such models which are quite sensitive to boundary conditions and emission inventories, we believe it is better have large-scale average as a first measure to test essence of the coupling of weather and transport.

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

Done.

References:

Warner, J. X., Comer, M. M., Barnet, C. D., McMillan, W. W., Wolf, W., Maddy, E., and Sachse, G.: A comparison of satellite tropospheric carbon monoxide measurements from AIRS and MOPITT during INTEX-A, J. Geophys. Res., 112, doi:10.1029/2006JD007925, 2007.

Warner, J., Carminati, F., Wei, Z., Lahoz, W., and Attié, J.-L.: Tropospheric carbon monoxide variability from AIRS under clear and cloudy conditions, Atmos. Chem. Phys., 13, 12469-12479, doi:10.5194/acp-13-12469-2013, 2013.

Warner, J. X., Wei, Z., Strow, L. L., Barnet, C. D., Sparling, L. C., Diskin, G., and Sachse, G., Improved agreement of AIRS tropospheric carbon monoxide products with other EOS sensors using optimal estimation retrievals, Atmospheric Chemistry and Physics 10, 9521-9533, doi:10.5194/acp-10-9521-2010, 2010.

Yurganov, L. N., W. W. McMillan, A. V. Dzhola, E. I. Grechko, N. B. Jones, and G. R. van der Werf, Global AIRS and MOPITT CO measurements: Validation, comparison,

and links to biomass burning variations and carbon cycle. Journal of Geophysical Research 113, D09301, doi:10.1029/2007JD009229, 2008.