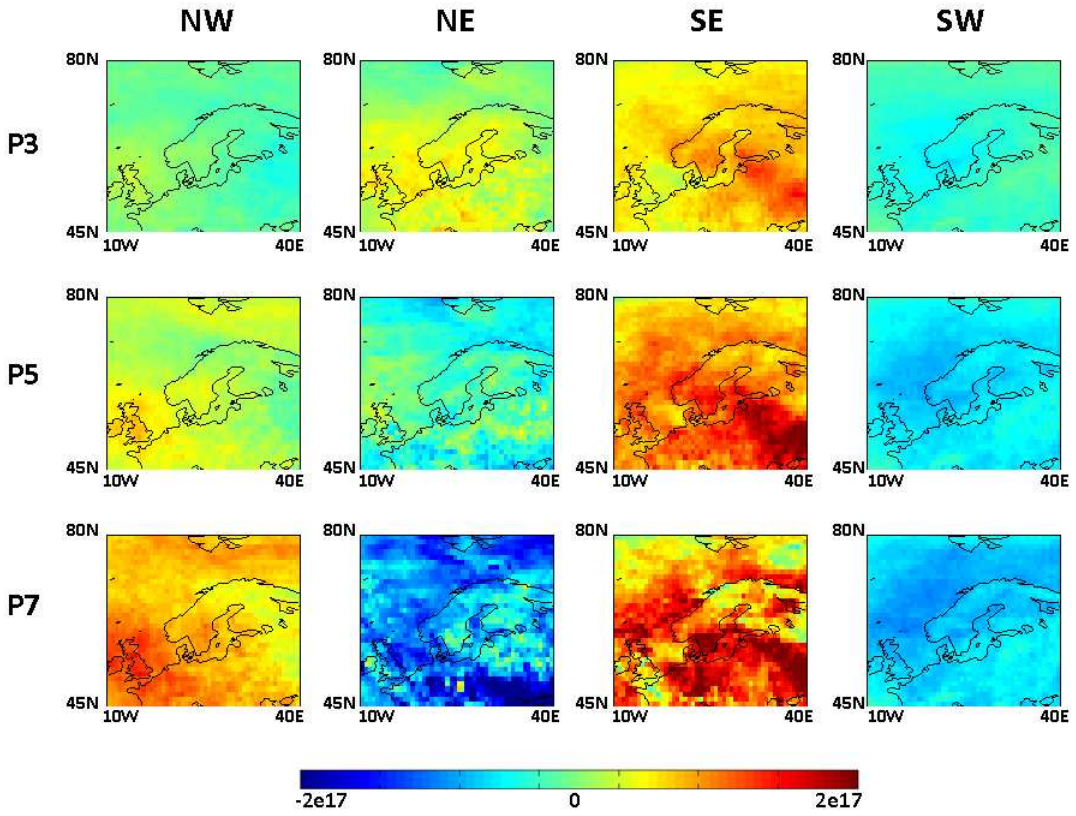
Norrköping, 2014-07-11

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.

You consider CO and wind speed in one layer only, it would be interesting to know how this is related e.g. to CO surface concentrations or the total CO column. Maybe some surface measurements or discussion on the influence can be added to this analysis.

Since the main aim of the paper is to investigate co-variability of free tropospheric CO and weather states to understand the importance of the transport processes, we have focused on 500 hPa level. It is worth mentioning that the observed co-variability and 3-to-7 day tendencies at 500 hPa are generally similar in the entire middle troposphere (50 hPa to 400 hPa).

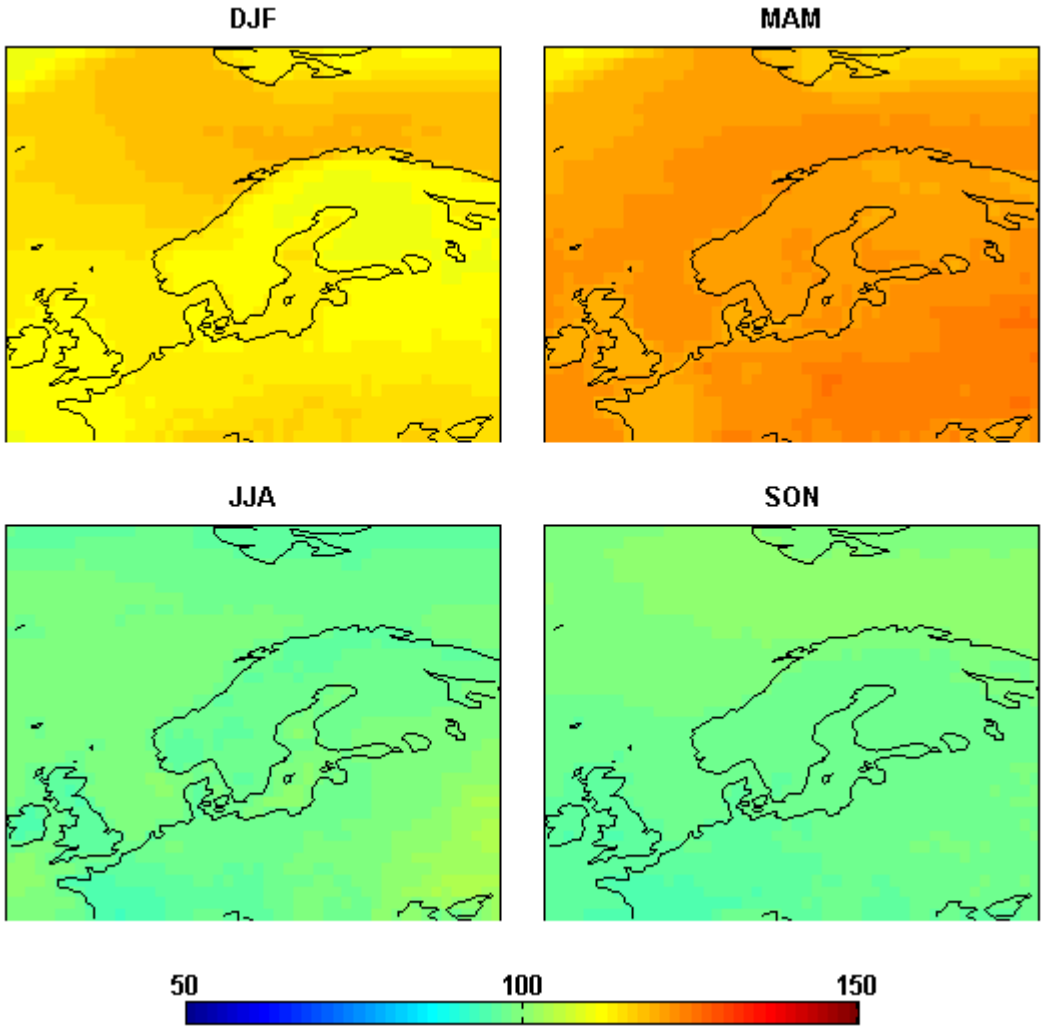
Having said this, we did indeed like your idea to look at the total column CO and we were curious to see if there is any relation. So we reprocessed the entire 11-yr satellite data set and found out that (see figure below) we get similar co-variability in the total column CO as well. This is an interesting result, since it underscores the importance of selected weather states in driving the CO variability ***in the entire column***, instead of just in the middle troposphere. In fact the tendencies in CO anomalies (molecules/cm^2) under different persistency periods are much clearer. We have added this figure as a supplement and corresponding description in the revised draft of the manuscript. We thank the referee for this interesting suggestion.



What about trends, did CO change on a global mean over the 11 years period? It would be interesting to have some mean CO plots to get a better impression on how important the anomalies are. But not only seeing changes of the 11 years but also during the seasons would be interesting.

Globally, the CO concentrations in the free troposphere show decreasing trend over the last decade. CO is also decreasing over the study area in all seasons. The figure below shows seasonal mean CO at 500 hPa (in ppbv) over the study area based on 11-yr AIRS data (2003-2013). A description is added in the revised manuscript.

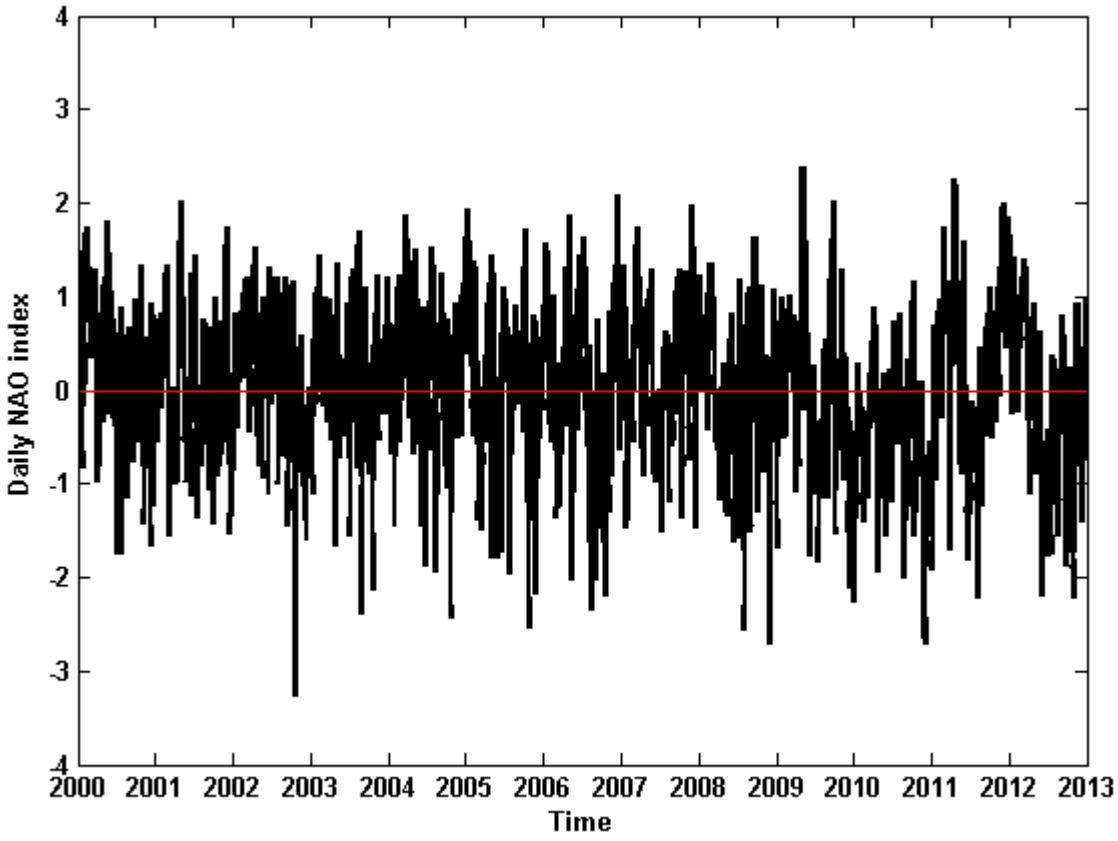
Please note that the trend in CO is least likely to have impact on our results since the occurrence of weather states (esp. w.r.t. their consistency periods) is randomly distributed in time. There is no strong bias towards particular years within the study period.



NAO shows the highest correlation with pollutants in winter/spring. How stable are your results during summer? It would be good to have some description of the temporal evolution of the NAO over this 11 years.

We cannot think of any physical or technical reason as to why our results shouldn't be robust during summer. We understand that NAO influence is generally speaking strongest in winter. But please note that NAO is after all representation of a certain state of the atmosphere. If such phenomenon is active during summer (although may be not as frequently as in winter), it will also have measureable impact on CO variability, esp in the free troposphere (depending on seasonal transport characteristics and emission source variability).

The daily NAO index doesn't show any significant trend during the study period (please see a figure below that is added as supplementary information in the revised draft).



If I would need to sort Fig 2c NAO-EP into Fig1, I would think it would correspond to Fig 1c, even though the CO anomalies in Fig 5 (SE, P3) and Fig 7 (P3, EP) look very different, why is this the case?

We think the main reason for this is the different 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, while in the EP case on other hand, there is more or less direct conduit from the polluted regions of North American and north European regions.

What is the conclusion from the temperature and water vapor anomalous, they are highly correlated, is it necessary to show both? (Fig 3)

These anomalies are shown as additional confirmation and sanity check of the influence of heat/mass transport under different wind conditions. However, we agree that it is probably not necessary to show both T and q anomalies as they tightly co-vary. We have revised this figure and kept only T anomalies.

Fig 5 shows anomalies depending on persistence periods, the lowest row looks very patchy, how many CO images where used for those?

A table is added in the revised draft showing the number of events studied for each weather state and its period of persistency. The probability of a particular weather state prevailing over the study area decreases with increased period of persistence. As a result, the results for 7-day periods are patchy, but as shown in the revised figures the CO anomalies exceed at least one standard deviation and hence are significant.

Often satellite retrievals cannot be performed because of the presence of clouds (you say you omitted when cloud cover was about 30%), are there regions for the respective weather pattern where clouds are abundant? What impact would that have?

This is an important issue. There is of course spatial and seasonal variability in cloud cover within the bounds of our study area. The chosen threshold of 30%, although sounds subjective at first, is actually based on years of experience working with AIRS data to balance accuracy and number of samples (Devasthale and Thomas, 2012; Devasthale et al., 2010, 2011, 2012, 2013).

More importantly, Susskind et al (2003) have previously presented detailed analysis of the accuracy of AIRS retrievals in presence of clouds. The yield and accuracy of AIRS retrievals should not degrade significantly up to 30% cloud cover. Recently Warner et al. (2013) showed that the AIRS CO retrievals in cloud contaminated cases are quite good as well. The degrees of freedom of the signal, an indicator of information content, are reduced only by up to 0.2 in cloudy cases (please refer Figs. 3 and 4 in Warner et al., 2013). The difference should even be smaller in our cases, since we allow only 30% cloud contamination.

Furthermore, the majority of opaque clouds occurring over the study area are low clouds (cloud tops less than 700 hPa). Since we analysed retrievals at 500 hPa, the cloud impact is estimated to be small.

Finally, the absence of any spatial correlation between cloud fraction and observed CO anomalies also suggests that the cloud impact is negligible.

Fig 8: it would be better to show a box plot, including mean and percentile instead of an average percentage.

To clarify, please note that the Fig. 8 is showing ***the percentage change*** in CO average composites.

I would find it useful if the satellite retrievals as well as the grouping algorithm of the windfields would be described in more detail. E.g. could all fields be classified? How frequent were satellite observations for certain regions/times?

More discussion is added in the revised draft on the satellite retrievals and weather state classification.

Minor/technical comments:
The figure caption need some better description: Fig 1, units of contours, how to interpret the length of the arrows. It says 10, maybe explain for the reader that there is an arrow which represents the wind speed of 10 xx.

Done.

Fig 4: describe in the caption what a,b,c shows

Done.

p 9256 line 2 and line 17 - the coordinates of the study area do not match

Corrected.

p 9253 line 4 and p 9252 Iine 14 - I noticed that some abbreviations are inconsistent e.g. N. America and North America

Corrected.

p 9254 line 12 - a reference to IASI could be given

Given.

p 9257 line 21 - MSLP was already used at 9256 line 21, define there

Corrected.

References

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