

Using a fully coupled general circulation model, the authors investigate potential NAO shifts in the future and its impacts on pollutants dispersions. The authors simulate the period 1950-2100. The results show a significant but weak positive NAO and strengthening of the correlation between NAOI(PC1) and CO mixing ratio. Based on that, the authors conclude that under global climate change scenario local air quality conditions in Europe and Northern Africa will become more extreme.

The manuscript is pretty easy to follow, although the presentation should be improved: in the introduction the authors mostly make a list of the papers addressing the relationship NAO and pollution and it lacks of a proper discussion where their results are contextualized within the outcome of previous studies. Further, I think the authors never mentioned the aerosol scenario adopted (MFR, CLE?)

I found the manuscript a bit poor in terms of content and analysis done. The authors only show an EOF analysis and a correlation analysis. I think that after adding further analysis, it may be suitable for publication in ACP.

Major comments:

1) I found the final statement that “under a global climate change scenario local air quality conditions over Europe and North Africa, influenced by North Atlantic teleconnection activity, will become more extreme” unsupported.

The authors also state at the end of pag. 7: “At the same time it seems that the region over the American east coast will be characterized by concentrations of pollutants in a range similar to the past, with respect to the NAO activity.” How can they possibly say that just through a correlation analysis? Could the authors explain why an increased correlation between PC1 and CO means that there will be more extremes? The correlation increase in absolute values also in Northern Europe, does that mean it gets more (or less) extreme there too?

An analysis of the extreme should be done to make that point and would also add more strength to the study. Also a regression analysis can also provide some insights on changes in the relationship between the PC and CO concentrations.

The authors should also try to understand why the correlation gets stronger in the future. Interestingly, there is a shift of the centers of action but the PC/CO correlation pattern doesn't change. This should be discussed and possibly explained.

2) Another question that I think it would be interesting to address is how the intercontinental transport of pollution changes in the future.

3) I would suggest plotting the PC1 time series for the entire 1950-2100 period. However, the EOF analysis should be done after removing the climatological SLP climatology (e.g. 1980-2010, or 1950-1979). Otherwise if the authors calculate the EOFs without removing any climatology, the PC1 mean of the entire period would be zero. The authors could also plot a pdf of the NAO/PC1 events to see how the distribution (extreme) changes.

These are only few suggestions but I am sure the authors can come up with some ideas and new analysis to perform in order to make the paper more interesting and suitable for the readership of ACP.

4) I am very surprised to see that the coupled simulation shows a similar NAO trend as the reanalysis. The model has its own internal variability therefore is not to be expected at all to show the positive NAO trend in the 80s and beginning of the 90s. It must be a coincidence and should not be presented as an evidence that the model is performing well because resemble the reanalysis.

Minor comments

- I would suggest to use superscript rather than subscript for CO²⁵ and CO⁵⁰.
- PAG 1 LL15-16: The authors write: “*The NAO, defined as the surface pressure difference between the Azores high and Icelandic low, influences weather conditions (Hurrell, 1995).*”
This is not the definition of the NAO but rather the NAO Index. By definition the NAO is an oscillation a swing between pressure systems not a difference.
The NAO was not discovered by Hurrell, who was the one who introduced the NAOI. Walker in the late 20s was the first to discover it. The reference for the NAO should be:
Walker, G. T. and Bliss, E. W.: World Weather, V. Mem. R. Meteorol. Soc., 4, 53–83, 1932.
- PAG 4 L28 change data to scenario (the RCPs are scenarios).
- PAG 4 LL29-34: I don't think this is needed. I think is pretty obvious that a coupled simulation is needed for future scenarios and is better than using prescribed SST. In theory you could use SSTs from another model but it's not an optimal solution. Hence, the coupling is the best option and is well known. If the authors weren't coupling it then you should justify it.
- PAG 5 L12-21 specify the years you are performing the EOF analysis. The 38.8% explained variance is for the entire time series?
- PAG 5 L25: The Eastward/NE shift of the centers of action in future climate is also shown in Pausata et al., ACP, 2015 using another technique.
- PAG 5 L28: change “does not reflect” to “is not able to capture”
- PAG 6 L33 I would avoid using New York to characterize the region. I would just add "of the NORTH American east coast")
- The manuscript has very few figures hence the authors can add the CO50 plot into fig. 4 otherwise what is even the point of saying that you have used the CO50 if you don't show it and the results are the same as CO25?
- PAG 7 L22 please, spell the period out or refer to the experiment as future/reference etc. Avoid using period B or period A.
- PAG. 8 L8 report the citations here as well. In any case a discussion is missing and should be added to the manuscript.
- The authors should take into account in their (future) discussion that in the future the aerosol concentrations will likely decrease and this should anyway lead to a better air quality even over Mediterranean countries. This should be discussed.