

Interactive comment on “North Atlantic Oscillation and tropospheric ozone variability in Europe: model analysis and measurements intercomparison” by F. S. R. Pausata et al.

Anonymous Referee #1

Received and published: 19 March 2012

This manuscript aims to examine the role of large scale atmospheric circulation variability in the North Atlantic basin in determining surface ozone concentrations over Europe using the fully coupled aerosol-chemistry climate model ECHAM5-HAMMOZ and observations. Overall, it is an interesting and well structured manuscript. It merits publication in ACP and I would suggest acceptance of the paper after taking into consideration the following comments.

Comments

a) Page 3137, lines 26-27: The authors mention a substantial overestimate of the modelled ozone compared to measurements by up to 15 ppbv in some regions. Please

C735

clarify which regions and possible reasons for this large overestimate. For example clarify the possible role of stratosphere-troposphere exchange or photochemistry in this overestimate.

b) The authors mention that the NAOI timeseries have been calculated as the difference in the normalized SLP anomalies between the model grid boxes corresponding to the location of Ponta Delgada, Azores and Stykkisholmur/Reykjavik, Iceland. It might be worth looking how the modelled NAOI compares to the NAOI based on observations.

c) Page 3139, lines 20-21: It is mentioned that the NAOI and PC1 monthly timeseries are strongly correlated in winter (0.86) and less correlated in summer (0.59). Please specify if these correlations are based on daily values, monthly values or seasonal values.

d) Pages 3142-3143: It is mentioned that the negative phase of NAO leads to a southward shift towards the Mediterranean Sea of the storm track and consequently, the O₃ enriched air masses from the Atlantic Ocean, increase of few ppbv the surface O₃ concentrations in the western part of the Iberian Peninsula (Fig. 6). Is this small and spatially limited increase of ozone over the Iberian Peninsula statistically significant to make the authors speculate for the reasons of its presence in their model results? Also I am not sure why the authors claim that the air masses originating from the Atlantic Ocean are enriched in O₃? If we exclude cases of intercontinental pollution transport from US then usually the Atlantic ozone baseline ozone is lower than the European continental ozone levels.

e) Page 3143, lines 3-6: The authors state that “More storms in the Mediterranean Sea enhance southerly flow towards Italy and northern Africa leading to a slightly increased surface O₃ concentrations over the Italian peninsula and decreased surface O₃ values in north-eastern Africa.” I find this statement rather speculative and I would ask the authors to clarify and explain what do they mean. Furthermore, what the authors claim as an increase over the Italian Peninsula in the negative phase of NAO is actually

C736

small response limited over a small area in Central Italy. Is this increase statistically significant at 95% or 90%. Similarly the negative ozone anomalies over the Adriatic Sea during positive NAO phases in summer are small and limited and I am wondering if they are also statistically significant.

f) Following my previous comments, I would suggest for clarity reasons to indicate in Figure 6 the areas where the anomalies are statistically significant at 95% or 90% (e.g. by using a t-test). This is the most common way to point the statistically significant differences between two groups of data.

g) Page 3145, lines 1-4: The authors state that in summer “The negative NAO phase leads to surface O₃ negative anomalies that are shifted south compared to positive anomalies, affecting western Africa, part of the Sahara desert and great part of the Mediterranean Sea, whereas mainly no anomalies are displayed in the Nordic Sea and Baltic Region.” The authors attribute these negative ozone anomalies over southwestern Europe to less ozone enriched European continental flow. Could it be also attributed to a cyclonic anomaly introducing more clouds, less solar radiation and hence less photochemical ozone production?

h) Page 3146, lines 9-12: The authors state that “Unfortunately, since in our study we do not include a diagnostic for transport of stratospheric air into the troposphere, we could not quantify neither the amount of surface O₃ variability associated with the STT, nor the frequency of STT events associated with the NAO phases.” Although I understand that the authors do not have stratospheric ozone tracer as diagnostic in their simulations, they could possibly use other common parameters from their simulations as diagnostics such as potential vorticity or water vapour mixing ratio. These diagnostics may not assist to quantify the ozone anomalies but at least would give a hint and stronger evidence for interpretation of their results.

i) Finally in the discussion section (page 3147), the authors state that their approach of PC1/O₃ relation can be used to identify areas at risk of high O₃ concentrations,

C737

without performing costly chemical weather forecasting at monthly timescale. I find this statement about chemical weather forecasting rather obscure and unnecessary. The use of statistical indices and relations may assist chemical weather forecasting but cannot substitute it. I suggest rephrase of this sentence in order to give the right message.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 3131, 2012.

C738