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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of the North Atlantic Oscillation on European tropospheric composition: an observational and modelling study" *by* Richard J. Pope et al.

Anonymous Referee #2

Received and published: 23 January 2018

General comments:

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The authors used satellite observations and the TOMCAT chemistry transport model (CTM) to investigate the influence of the winter-time North Atlantic Oscillation (NAO) phases on the tropospheric concentrations of NO2, PAN, Ozone over the North Atlantic and western Europe.

Though the methods are scientifically sound, the authors fail to properly motivate the study and it is not clear what is the underlying purpose other than to assertain what was presented in numerous other studies that the manuscript cites. The fidnings and

conclusions of the manuscript in my view do not offer any new general implications for atmospheric science. The manuscript fails to expand beyond what is readily established in the literature or introduce sufficiently novel methods or techniques, other than perhaps some some incremental improvements.

My recommendation is that this paper undergoes major revisions before publication in ACP to: properly motivate the choice of remote sensing products, explain the need and purpose of supplementing the analysis of observations with model simulations, and to include more detailed discussion of the scientific implications of the outcomes.

Specific comments:

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p.4 l.24: It's understood that November is sometimes included in the seasonal data to increase low statistics. However, to aid intercomparison wouldn't it be better to be consistent in all cases?

p.5 l.1: "significant" NAO+ and NAO- phases may be misconstrued by the reader. Propose to change to "high and low" as is usually found in the literature.

p.5. l.28: What is the point of including wind vectors in the Figs? They are not referred to in the discussion/analysis.

p.7.l.7: Please clarify what is meant by "The systematic errors will cancel considerably when comparing species-NAO composites to their winter-time averages". Why is that so?

p7.l.14: "range from approximately 10-40%, peak at 15-20% and are between 10-20%" meaning needs to be made clearer. Is that repsectively for OMI/MIPAS/TES?

Sec. 2.3: The section on the TOMCAT model has to be reworked to limit to information that is relevant to the present study.

p.10 l.12: Any reference as to why the higher tropopause potentially aids vertical trans-

port into the UTLS?

p.11 l.21: "We also have evaluated TOMCAT surface/tropospheric ozone against a range of observations. In all cases, TOMCAT has suitable skill to represent these chemical tracers and their responses to the NAO circulation patterns.". Is there any appropriate reference? OR Perhaps include the comparison in the SM?

The NO2 has a much short lifetime in the atmosphere than the timespan considered here and is heavily dependent on emissions. It is not convincing that the randomly scattered green regions of significance in Fig. 2 allow or support any generalised conclusions for NO2 concentrations to be influenced by a seasonal teleconnection pattern.

Technical corrections:

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p.6 l.3: change to "measurements of total column NO2 (TCNO2)"

p.6 l.4: UTLS PAN -> "UTLS Peroxyacetyl Nitrate (PAN)"

p.7 l.1 Printing the full file type list in the manuscript text is unecessary and extraneous. Either refer to it (or move details in a supplement).

p.7 l.6 and elsewhere "random error" -> "statistical uncertainty"

p.8 l.8: Cite IUPAC website as refence

Fig.3: It's very hard for the reader to discern the green lines and areas of significance on top of dashed lines in panels c) d). Perhaps make bolder?

Sec. 3.2 Title "Model Composition" -> "Model Results"

p.12 l.4,6: Missing TCNO2 units

p.14 l.14: larger -> higher

Fig.5 Squares and diamonds are cluttered over the errors. Perhaps a different rep-

resentation using horizontal bands and removal of the yellow sensitivity band may be clearer.

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