Review on

"The influence of synoptic weather regimes on UK air quality: regional model studies of tropospheric column NO2"

By Pope et al., ACPD 2014.

This manuscript describes the ability of UQAM to model the variability in tropospheric NO2 columns during cyclonic and anti-cyclonic weather conditions as compared to satellite observations, and aims to analyze the contribution from chemistry and transport, respectively, to this variability, by means of idealized tracers with fixed lifetime.

The paper is very clearly written, and is a natural extension to the work presented in Pope et al. (2014) where an analysis of satellite retrievals under different weather patterns was presented. I recommend this paper to be published after having responded to the following minor points of criticism.

Even though the general ideas and the conclusions drawn from this study are clear, it is hard to judge the methodology followed in this analysis. For instance, the selection methodology of LWTs is unclear, i.e. which locations are used for this, and what are effectively the predominant wind patterns during winter/summer anti-cyclonic and cyclonic weather conditions. I find it unfortunate that meteorological data is not available from the AQUM model, considering its pivotal role in this study. Now such information needs to be inferred from figures like those showing the idealized tracers. Even a sketch with an overview of prevailing winds during cyclonic / anti-cyclonic conditions could be helpful for understanding the anomalies seen in NO2.

Specifically the authors mention that '…any inconsistency between the NCEP reanalyses and AQUM flow fields will tend to worsen the comparisons…' and therefore tend to use the NCEP meteo data. This is only allowed when NCEP and AQUM flow fields are sufficiently identical. Even though this is likely the case, it is not explicitly shown nor mentioned. In fact, from my understanding the use of AQUM meteorological fields rather than those from NCEP would make more sense, as these fields are more consistent with the model tracer fields.

Secondly, despite the exhaustive introduction of the FGE score based on the 'anomaly cluster density' it is still difficult to appreciate this metric, for instance, in relationship with the correlation. The correlation is also reported, but shows a different message on the relative performance of the model under various LWTs. It would be helpful to expand on this relationship (or on its absence). In this respect it is also interesting to note that the correlation as well as the FGE score with respect to OMI observations are better for idealized tracer fields than for the actual model NO2 field. This is contra-intuitive considering the missing chemistry treatment. It could be worth to expand on this. It would have been interesting to study specific chemical or physical loss processes which are influential to the NO2 lifetime, and could improve the scores presented here.

Furthermore, the introduction of the four zones appears arbitrary to me and not really helpful for the discussion and may be omitted.

Finally, the authors introduce all 27 LWTs, and suggest from the abstract, conclusion and section headers of 4.1-4.3 that they have validated all LWT relationships. However, in their analyses they only discriminate between two families of cyclonic and anti-cyclonic weather types. In my opinion the authors should change the manuscript throughout in this respect, more clearly stating the chosen selection of LWTs for cyclonic and anti-cyclonic conditions only.

Specific comments:

Sec. 2.1: The methodology for selection of LWTs is described rather cryptic. Please expand on this (see suggestions above)

Pp 18585, l62: "Large background columns NO2 over the North Sea is indicative of cyclonic westerly transport off the UK mainland…" : Isn't there are contribution from NO2 originating from the continent?

Pp18586: "Under anticyclonic conditions …" : please brake sentence to two to improve readability.

Pp 18586, l10-l15: This is indeed an interesting observation. Do you have any suggestion why AQUM does show a different anomaly than OMI?

Pp 18589, l 7: The correlation appears highest for summer anticyclonic, while for this case the FGE score is lowest. Do you have an explanation for this?

Pp 18591, l 13-l16: This is a clear weakness of this paper, as mentioned above. The manuscript would benefit from a closer inspection of the prevailing weather systems.

Pp18592, l21: "NO2-LWT relationships": please change to something like: "captured the OMI column NO2 anomalies for cyclonic and anti-cyclonic LWT conditions".

Pp 18593, l22-l24: "This work…" I don't think this can be concluded from the current study, considering that the authors do not evaluate the absolute NO2 values during anticyclonic conditions. Also 'accumulation of air pollution' is obviously much wider than NO2 anomalies, as it should also include evaluations of other pollutants such as ozone. Finally it is unfortunate that for summertime anticyclonic conditions the FGE-score performs worst (even though the summertime positive anomaly indeed appears in line with those from other weather conditions).

Table 1: It is misleading to present all weather types, when only anticyclonic and cyclonic conditions are studied.

Figures 5, 8 and 9: Please improve readability of legends on axes and within the figure (font thickness)