

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 #3

Received and published: 27 March 2012

This paper examines the effect of the North Atlantic Oscillation (NAO) on European ozone using both measurements and a model simulation. The paper uses two indices to calculate the phase of the NAO: (i) the pressure difference between Iceland and Portugal; (ii) an index related to the 1st principal component (PC1) of sea level pressure. The latter measure proves more useful in relating European surface ozone to the NAO during the summer and spring months. The authors point out the relation between surface ozone and the NAO may prove useful in making seasonal predictions of European ozone levels and in interpreting interannual ozone variability. It also may have implications for future ozone levels under climate change scenarios.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

The paper is well written, easy to read and interesting. It provides a nice addition to the impact of the NAO on European ozone. I would recommend publication after the authors address the following comments.

Major Comments:

1. The authors state that the PC1 index is an alternative to the NAOI index (p 3134, l 22). This is not clear to me. In fact the AO is usually identified with the leading component of the 1000-hPa height anomaly variability. Thus it is not clear that the PC1 index as defined by the authors isn't actually an index of the AO oscillation. I don't think the authors really need to get into the potential differences between the AO and NAO, but I do think it is mis-leading to state the PC1 index is an index of the NAO. A better procedure would be to simply use the established AO index instead of the PC1 index in their analysis. This is unlikely to change their results but is more consistent with the literature.

2. The authors show a nice correlation plot of the NAOI and PC1 with ozone over Europe (Fig. 4), but they do not show the magnitude of the measured ozone anomalies associated with the correlation. The authors simulate very large ozone anomalies (e.g., Figure 6) associated with the NAO, up to 10 ppbv. They should additionally analyze the NAO ozone anomalies from station data. Does the station data support the large simulated anomalies?

3. The authors claim that the NAO may help to explain the ozone trend over Mace Head, and show measured and simulated ozone over Mace Head in Fig. 9. I think if the authors want to show this figure they need to, at a minimum, examine the correlation between the NAO/PC1 and Mace Head ozone. To what extent does the NAO explain the measured/modeled ozone anomalies at this site? I think more analysis is necessary with regards to this figure.

4. I think the references in the paper could be improved somewhat. There are a numerous locations where a statement needs additional references.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



-p 3133, l 5 and 6. This needs some references. Some relevant papers of which I am aware: Brownsteiner and Hess (2011), Liang et al. (2004), Fiore et al , 2002 . . .

-p 3133, l 14 “specific STT events”: see study by Lin et al., (2012)

-p 3134, l 19: references needed with regard to the importance of NAO during summer months

-Hess and Lamarque (2007) and Lamarque and Hess (2004) examined the response of STE to the AO/NAO. This previous work seems relevant to the current paper (i.e., in section 3.3)

Minor Comments:

1. p 3135, l 24 Hess and Lamarque (2007) analyzed ozone variability due to the AO, not the variability in general.

2. p 3136, l 23: I think the authors should say something more about how these particular stations were picked.

3. p 3137, l 19: What do the authors mean by a “consistent physical state”? Does the model calculate surface heat and moisture fluxes or are these input?

4. p 3137: Please give more information on the specification of biogenic emissions. Also please give more information on the specification of the stratosphere and the relevant boundary conditions used. Does the simulated stratosphere reflect variability due to the NAO? If not, I think the authors need to make some caveats about the impact of the NAO on middle and upper tropospheric ozone.

5. p 3138, l 6-7: What do the authors mean by monthly anomalies from the climatological seasonal cycle?

6. p 3139: “investigates” should be investigate, “trough” should be through.

7. p 3139, l 13: Please specify which index is referred to here.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

8. Figure 3: The authors should really use the same color scale for both the model and the measurements. Otherwise, it is very difficult to compare the two. However, I think it is reasonable to change this scale between the different seasons.

9. p 3140, l 11: Does the model overestimate the measurements everywhere?

10. p 3140, l 18: “does not present an issue”. I would say this a bit differently: it is not critical to the study.

11. Figure 4: this is a nice figure, but it is hard to discern the stations with significant correlations. One solution might be to make the size proportional to the significance and the color proportional to correlation. However, the authors might have a better idea.

12. p 3141, l 27-29: I think the authors need a reference here. While ozone lifetime is longer in winter photochemistry is also important. One example is the NO titration of ozone in the European boundary layer. Probably want to reword this somewhat.

13. p 3142. The springtime pattern here looks remarkably similar to Hess and Lamarque (2007). This is probably worth mentioning.

14. p 3142, l 18: by “down” due the authors mean southward? It is not clear what is happening in the vertical.

15. Figure 6: please indicate the regions where the correlation is significant (from the previous figure) instead of where the standard deviation is larger than 0.5

16. p 3144, l 4: “the different correlations could be” – the authors should be able to easily check this hypothesis.

17. p 3145, l 24: it is not clear why the authors use “extreme” here

18. p 3146, l 14-16: I find that the points the authors are making could be stated better. I find point (ii) somewhat misleading as stated and in fact the authors seem to contradict it in the next few sentences. Many studies show STT has an appreciable effect at the

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

surface. This point could be clarified somewhat. In addition, in regard to point (i) the “smog” photochemical reactions are likely to influence the ozone throughout the depth of the troposphere. Please clarify point (i).

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

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper