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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The North Atlantic Oscillation controls air pollution transport to the Arctic" *by* S. Eckhardt et al.

Anonymous Referee #1

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The authors address the effect of the North Atlantic Oscillation on the transport into the Arctic region of atmospheric pollutants from the three populous areas of the northern hemisphere: North America, Europe and Asia (from Turkey to Japan). By means of Lagrangian calculations based on ECMWF archives for the past 15 years, they organise quantitative arguments to show that the winter and spring Arctic pollution is mainly due to European emissions and stronger during the high positive NAO periods. The effect of a linear decay of the tracer with an e-folding lifetime is included in the analysis. The results secondly introduce a well documented discussion about the not yet determined positive or negative feedback between the NAO and this pollution with an invitation to pursue the investigations.

The interest of the orientations suggested or speculated (e.g. the recent cooling of the Arctic related to European emissions reduction) by the authors is adequately strength-

ened by concrete numerical arguments. CO, Black carbon and especially NO2 observations are invoked to guide the interpretation of the output of the model. The guidance ability of these data is unfortunately limited by their scarcity (CO, BC are observed at a few stations) or by a non linear behaviour not included in the model (NO2, with abundant satellite data from the GOME). Accordingly the paper would have a greater capacity to foster its important debate if some too rapid explanations were clarified and if some interpretations were either more substantiated or renounced.

The NAO is a current topic in climate change, as is the El nino-Southern Oscillation. I would like to ask the authors if, to their knowledge, the (ocean-)atmosphere models now available in the community reproduce with enough accuracy these structures to succesfully explore their reaction to anthropic effects? Which improvements of the models they would recommend? And which observations they would like to organise as a support to their investigations?

More specifically:

Page 3224, line 9-11 Which percentage of the (sea level pressure) variance is explained by the NAO (I have found the value of 30% by further exploring the web site, quoted by the authors, of Jim Hurrell at NCAR: http://www.cgd.ucar.edu/~jhurrell/nao.pc.html)?

Page 3224, lines19-21 The formulation is not clear as it suggests that the results of the paper are based on observations. It should be said more clearly at that moment that the results are based on a 15-year model simulation. In particular, the word 'signal' on line 21 is inappropriate.

Paragraph 2: I understand that, for each selected month of low or high NAO index, a dispersion calculation has been performed and stored for twelve 'age classes'. It is nevertheless not clear, despite some indications on page 3226, lines 1-8, if these monthly results have been averaged by considering several release dates in each month (how many?), one single release date (which one?) for each month, or a continuous release

during several days (how many?), or... . The words 'suitably defined' on page 3226, line 2, mainly arouse curiosity: more details are expected. I understand anyway that the results have been finally averaged as NAO+ and NAO- dipersion patterns: it would be interesting to give explicitly, as a figure or an array, the list of the months selected as low or high NAO. Also, it is not said if the NAO index values ranging between -3.0 and +3.6 are monthly, seasonal or yearly averages. The number of the Lagrangian particles released for each calculation should be given. Finally, it would be convenient, in order to better physically interpret the results, to describe the horizontal and vertical resolution of the grid and to describe from which model or theory the diffusion coefficient was derived.

Page 3226, line 8 , 'The NAO index was derived from ...' I suggest to give a complete definition of the index.

Page 3226, line 26 I propose the following formulation: '.... the differences between the NAO+ and NAO- composites are much smaller'

Page 3227, line 1 Of the following two words 'during' and 'for' one is (apparently) too much

Page 3227, lines 12-13 (and page 3230 line 3) The third conclusion that the NAO dependence decreases with the e-folding time is not clear at all from figure 4 apparently rather showing an independence. It is perhaps an unfortunate visual effect. If the authors are to maintain this conclusion, either it should be more substantiated or the figure 4 should be rearranged in order that its appearance does not go against the conclusion.

Page 3227, lines 16-18 Is it possible to give more detailed explanations about this slope and this regression analysis?

Page 328, line 4 : Which seven winters?

Discussion and conclusions:

Page 3229, lines 25 I suggest to remind that the results of the paper are numerical results with, for instance, the following clearer formulation: 'Using a 15-year tracer transport climatology based on ECMWF archives and model simulations it was found ....' .

Page 3230, line 3-6 and figure 6 The comparison with NO2 measurements is encouraging but not convincing. Are there any plans to carry out more detailed comparisons with measurements or to include explicit chemistry calculations? The caption of figure 6 is confusing because the descriptions of parts a) and b) are not parallel. I suggest the following structure : '..... a) NO2 residuals b) 1 day-life-time tracer (representative of NO2) residuals plus correlation (after residuals instead of before) coefficients...' . The correlation coefficient with NAO could also be calculated for the GOME observations. Is it possible to use the same geographic frame and colour scale for figures a) and b)? Furthermore, the colour scale of the figure b) has a clear meaning only if the intensity of the source is clearly given which is not the case. I suppose that, as said on page 3225, lines 9-11, the EDGAR source of CO has been used as a proxy for the source of NO2. Nevertheless, some proportionnality coefficient must have been used to convert CO into NO2, and this coefficient with the way it was chosen (the ratio of total releases?), is not mentionned. Finally a reference for the continental chemistry and dispersion of NO, NO2 would be welcome.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3223, 2003.