Using an atmospheric general circulation model, the authors investigate potential NAO shifts in the future and its impacts on pollutants dispersions. The authors perform two simulations, one nudged to ERA-Interim reanalysis for the period 1979-2013 and the second one forced with HadGEM2-ES SST for the period 1950-2099.

I struggled to find the original message of this study that should warrant publication in ACP. To me it does not provide an added value compared to what has been already shown in previous studies, e.g. in Christoudias et al., ACP, 2012. Or better, in my opinion it does not present results novel enough to warrant publication in ACP. Specifically, the influence of the NAO on atmospheric dispersion of pollution using idealized insoluble gaseous tracers has been shown in Christoudias et al., ACP, 2012, using the exact same model. The authors use different SSTs and they extend the simulation to the future, investigating whether the NAO shows any trends, which can be considered as the added value of this study. However, the set-up used (forced SST from another coupled model) makes the result weak, since the NAO behavior, as also stated by the authors (LL 19-22 pag. 33058), is to some degrees influenced by the SSTs (e.g. Mosedale et al. 2006) and therefore the results are not independent compared to the HadGEM2 coupled simulations.

As mentioned above, the only potentially new result from this study is the investigation of the NAO in the future. To do that the authors perform relatively complex analyses, such as the Empirical Orthogonal Function and a linear regression coefficients of the principal component time series of the EOF1 in sliding windows with variable lengths. However, a very simply analysis and only 1 plot would do the same: calculate the station-based NAO Index for the period 1950-2099 taking as a reference the first 30 years and plot it. This simple analysis would show whether or not there is any trend in the NAO. Otherwise, it is like using a cannon to kill a mosquito.
Therefore, the use of more complex analysis if needed should be clearly justified and I really do not see the need to use either the EOF or the linear regression coefficients of the PC in this manuscript - as it is now. The authors indeed mention that the PC timeseries of EOF1 is interchangeable with the station-based NAOI and they do that in the text (see Fig. 4), which I actually find it confusing. If the authors decided to use the PC should go for that rather than randomly use one or the other.

Furthermore, I found the text poorly written at times (see minor comments below), I would urge the authors to read carefully the text and in particular the references before submitting the manuscript.

These concerns together with presentation of the results prevent me to recommend it for publication in the present form. I am puzzled whether this manuscript – even after an extended major revision – may be suitable at all for publication in ACP.

Additional major comments:

1) The discussion of the EOF analysis is extremely superficial, that’s the reason why it looks completely unnecessary:

a) The authors always show the EOF analysis or the correlation analysis for the entire time series 1950 – 2099. How can they infer, for example, any change in the EOF pattern between present and future?

The authors state (pag. 33056 LL 23-27) that the patterns in Figure 1 and 2 are comparable. I wonder how they define "comparable"? By the fact that there is a dipole in the EOF1 and a monopole in EOF2? To me the patterns are different.

The tilt of the pattern in the EOF1 is different as well as the location of the centers of action. The EOF2 is also different but that maybe due to the fact that in figure 1 the EOF2 and 3 may not be separated while the EOF2 and 3 in figure 2 likely are (see comment #1c).

Furthermore, the authors compare the present day nudged simulation (fig. 1) with the whole time series 1950-2099 (fig. 2), which may actually be influenced by potential changes in the NAO pattern (EOF1). The authors indeed show - albeit poorly discussed - that the correlation coefficient, **R** (not R^2), between NAOI and PC is slightly higher in the period 1979-2013 compared to the future (pag. 33057, LL 9-11), which may indicate a shift in the NAO center of actions, making the station-based index less representative of the true NAO. The authors gloss over just saying the "correlation is very strong".

b) The authors show the EOF2 but they never discuss it. I'm not mistaken they don't even mention what that pattern refers to (i.e. East Atlantic pattern, EAP). What's the point of showing it?

c) The authors show the EOF2 in the nudged experiment, which seems not to be completely separated by the EOF3 (the Scandinavian pattern, SCAND). If the authors decide to keep the EOF2, please provide the explained variability of the EOF3 (and in case the difference between the explained variability of the two EOFs is very close the authors need to assess whether they are or not independent) and discuss the results and the implication for the pollution transport.

2) The authors compared the model results associated to the NAO/tracer transport relationship to the observations. However, the comparison shows a weak agreement (besides the fact that the authors forgot several stations in figure 6 bottom panel, see comment on the figure below) and should be further discuss. The authors only state that stations have small sample size without quantifying the actual length available for each station. In addition, the authors should determine whether the correlation for each station is significant or not.

3) Pag. 33061 L10 onwards. The authors mainly focused on the similarities of the correlation patterns, again glossing over the differences in the patterns. I much more intrigued by the differences instead: it is obvious that the main characteristics won't change in the future: positive phases of the NAO will always be characterized by increased westerly strength. On the other hand, the changes are definitely worth investigating for example showing the model results for the first part of the long simulation 1950-2013/1979-2013 and the future. Are the changes in the patterns due to the fact that the model is nudged in one case, whereas is *free* in the other case? Or are the changes due to climate change? As mentioned above the conclusions will be in any case weakened by the use of HadGEM2 SSTs.

Minor comments:

Abstract:
The abstract should contain only essential information and the key new finding of the study. Specifically:

I) LL 9-11: It's been already documented that the model is able to reproduce the NAO (e.g., Christoudias et al. 2012), so I don't see the need to put it in the abstract.

II) LL 17-20: It's been already shown by for example Christoudias et al. 2012 with the exact same model that *"during the positive phase of the NAO, the transport from North America towards northern Europe is stronger and pollutants are shifted northwards over the Arctic and southwards over the Mediterranean and North Africa, with two distinct areas of removal and stagnation of pollutants."*

Introduction

III) When discussing previous work on NAO impacts on pollutants, the authors should mention the study of Pausata et al., GRL, 2013 in which they show for example that extreme NAO phases in the 1990s modulated most of the interannual variability of winter PM concentrations in several European countries.

IV) When discussing previous work on future NAO trends, the authors should at least mention the studies of Stephenson et al., 2006 and Frei et al. 2006. Other early/"pioneering" studies on that topic are Fyfe et al. 1999 and Shindell et al., 1999.

V) The authors wrote (LL 6-9 pag. 33053): *These results* (positive NAO trends in the future) *were superseded by more recent studies (Scaife et al., 2007) showing that the upward trend has recently reversed downwards, suggesting that the previous positive trend was due to natural climate variability (and not external factors);*

a) The authors cite only one study;

b) Scaife et al., 2007 (should actually be 2008 and the author list of that paper in the reference is incomplete) are not actually dealing with future NAO trends at all. They only mentioned in the conclusions that the NAO trend, seen in the 80-90s, may have reversed, which is not a result of their study but a matter of fact, based on observation.

VI) Following the above comment, the authors wrote (LL 9-11 pag. 33053): *if that is indeed the case other trends with changes in direction and duration can be expected in the future, leaving open questions about their origin and the predictability of the NAO.*
I don't know which *other trends* the authors are referring to. In my opinion, this sentence is very vague.

VII) L 12 pag. 33053: *global climate circulation model*: GCM is either Global Climate Model or General Circulation Model, pick one. Furthermore, I would specify right there that you are using an atmospheric chemistry climate model.

VIII) L17 and 22 pag. 33053: why do the authors use such difficult acronyms

(RC1SD-base-09 and RC2-base-05) for the 2 experiments? Can they find something easier to remember?

Methodology

IX) L 15 and 17 pag. 33055: The authors refer to Hurrell and NCAR 1995 and Hurrell and NCAR, 2003. I would urge the authors to proof read the manuscript and double-check the references.

NAO representations and trends

X) LL 7-11 pag 33057: in general R refers to the correlation coefficient, while R^2 is the coefficient of determination. Please clarify which one has been used.

XI) LL 8-9 pag. 33057: I don't think it is reasonable at all to interchange the station-based NAOI and the PC1 (EOF1-ts), since they are not exactly the same thing: while the latter is potentially able to take into account shifts in the NAO pattern, the former doesn't. Furthermore, it's confusing.

XII) Finally, I would encourage the authors, if they keep the EOF analysis, to refer to the PC1 as the EOF-based NAOI: the EOF usually refers to the pattern and the PC is the temporal series (hence no-need to call it EOF-ts, but just PC1).

XIII) LL 1-7 pag. 33058: this entire part does not belong to the main text, but to the figure caption.

XIV) LL 12-13 pag. 33058: the authors refer to other studies (Visbeck and Gillet) to confirm the positive trends in the 80-90s. Again, this is a matter of fact that can be deducted by observations, so no need of referring to other studies, but only to the dataset where one can find the data.

XV) LL 19-25 pag. 33058: the authors refer to 12/14-year old studies for SST-NAO relationship. I would urge the authors to do some literature search and provide some more updated studies: e.g., Mosedale et al. 2006, Xin et al., 2015. Furthermore, they state that the response of the Atlantic Ocean to changes in the NAO is well established, which is indeed the case, but they don't provide any reference. Please add some.

XVI) LL 26-27 pag. 33058: this part does not belong to the main text, but to the figure caption. I would encourage the authors to cut to the chase in the manuscript and discuss the results shown in the figures show, not what is in the figure, which belong to the captions. Therefore, I would encourage the authors to avoid, throughout the manuscript, the use of sentences like "In Fig. * this and that are shown" or "Figure ** show this and that", makes the text heavier and does not add any useful information, since such info can be found in the captions.

Comments on the figures

i) Why the authors sometime use 90% confidence level and some others use 95%? Please be consistent and specify the test used to assess the confidence level.

ii) **Figures 1 and 2**: what's the point of showing the EOF2? Is it well separated by EOF3? I don't think so in Figure 1 for the nudged simulation.

iii) **Figure 3** why putting there also the time series performed with forced Had SST which are not comparable at all with the nudged simulation? I found it quite confusing.

iv) **Figures 4 and 5**: what's the unit of the window length, please specify.
Very nice analysis but what's the added value of just showing the NAOI for the entire time series taking as climatology the first 30 years and see if there is any trend in the time series?
In Figure 4 the font size is way too small to be readable.

v) **Figure 6**: why the authors use the EOF-ts in the above panels and the NAOI-press in the lower panels? Even though the time-series are indeed highly correlated I don't see the point of mixing them up? I would strongly suggest being consistent unless there are strong reasons to do otherwise.
Missing dots in the lower panel compared to the upper one: one in the Pyrenees; two south of Sicily, one in Greece. One green dot is also missing in Greenland (in the upper panel).

vi) **Figure 7:** The pattern is quite different. It would be interesting to divide the PD period from the future, i.e. plot 1950-2013/2014 and then 2015-2099 and/or 2050-2099 to see whether the different correlation pattern is due to the free running model or to climate change.

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