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Atmospheric Chemistry and Physics Discussions

Interactive comment on "North Atlantic Oscillation model projections and influence on tracer transport" *by* S. Bacer et al.

Anonymous Referee #2

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Review of North Atlantic Oscillation model projections and influence on tracer transport by S. Bacer et al.

This paper describes NAO variability in two simulations from the ECHAM/MESSy chemistry climate model and then links this variability to variability in CO. The first simulation runs from 1979-2013 where the atmosphere is nudged toward the ERA-Interim reanalysis. The second simulation runs from 1950-2099 and is driven instead by SSTs and sea ice from the CMIP5 simulation of HadGEM2-ES.

This paper lacks a clear message, and I am still unsure what the authors were trying to convey. While the introduction is largely framed around the relationship between the NAO and chemical transport, the majority of the results section pertains to the variability and trends of the NAO in the two simulations. CO appears to be added

almost as an after thought. In addition, the paper was poorly written - the word choices often made it hard to understand (e.g. "Their results showed and expectation for more positive NAO mean. . .") and there were many typos (e.g. NCAR Sea Leve Pressure).

More concerning to me, however, is confusion about the NAO calculations in the models as well as the issue of comparing a model with fixed SSTs from a fully-coupled GCM (HadGEM2-ES) and the use of SSTs from a different model entirely than the model that is being used (ECHAM vs HADGEM2). A significant portion of the results section is spent comparing the NAO in the nudged simulation to observations which seems to me to be a near waste of time given that the atmospheric circulation in the nudged model is being nudged to observations in the first place. It is no surprise the two look alike. Furthermore, it is unclear why the authors calculated the NAO in the nudged model anyway. Why not instead use the observed NAO (Hurrell time series)? Or use the observed station-based definition? Given that the observed and nudged NAO time series were correlated at 0.99, it is unclear to me why so much time was devoted to this endeavor.

Furthermore, from what I can tell, this paper offers little to no novel findings. Present and future NAO variability and trends have been studied extensively in the literature over the past decade (e.g. Gillett and Fyfe, 2013; Woollings et al 2014, IPCC AR5, IPCC AR4) and thus it is unclear what is novel about this study - especially given the strange setup with SSTs from one parent model (HadGEM2-ES) acting as the forcing for the ECHAM-based model. Why was HadGEM2 used instead? Since models have well known circulation biases that are potentially related to differences in SSTs, it is unwise to switch between models in this way. With that said, I did like the presentation of trends in Figures 4 and 5. It was a very nice and concise way of showing the information. However, I don't believe the information is particularly useful or novel. As for the CO transport, the authors list in the introduction that the CO/NAO connection have been studied in the past. In that case, what is novel about Figures 6 and 7? What have we learned from these simulations that we didn't know already? From the text it sounds as though Christoudias et al. (2012) already performed similar analysis, so what is different here?

My interpretation of the text was that the authors forced the second simulation with SSTs from the CMIP5 simulations of HadGEM2-ES - a fully-coupled GCM. Thus, there is no reason to expect the NAO over the observational record to agree with what was observed (since HadGEM2-ES is is free-running). However, given that Figure 3 suggests there is a correlation, I am left confused about what was actually done. Were these SSTs the observed SSTs? If not, how can the authors explain the high-correlation since HadGEM2-ES is free-running?

Finally, I am a bit concerned about the EOF analysis in Figure 1 and Figure 2. First, the percent variance explained of 53% seems high from my experience, unless the authors zonally-averaged the fields first. Secondly, I'm concerned as to why the variance explained drops by 20% between the nudged and "long" simulation. This seems very odd, as multiple studies have shown only small changes in the variance of the NAO and circulation over the 21st Century. I wonder if the authors forgot to subtract any trends in the long simulation before calculating the EOF? Otherwise, I am quite confused as to why they would look so different. Perhaps this is just a reflection of the problem with the mix of HadGEM2-ES and ECHAM?

In summary, I find it difficult to determine what major revisions would need to be performed for this article to be acceptable for ACP. My impression is that the entire study would have to be re-thought out to include a clear and novel message and that the paper would likely have to be re-written. Thus, I have recommended reject.

Gillett, N. P., and J. C. Fyfe (2013), Annular mode changes in the CMIP5 simulations, Geophys. Res. Lett., 40, 1189–1193, doi:10.1002/grl.50249.

Woollings, T., F. Franzke, D.L.R. Hodson, B. Dong, Elizabeth A. Barnes, C. Raible and J.G. Pinto, 2015: Contrasting interannual and multidecadal NAO variability. Climate Dynamics, doi:10.1007/s00382-014-2237-y.

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