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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of US air quality to mid-latitude cyclone frequency and implications of 1980–2006 climate change" *by* E. M. Leibensperger et al.

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Response to Comments on "Sensitivity of US air quality to mid-latitude cyclone frequency and implications of 1980-2006 climate change" by Referee #1

We would like to thank the reviewer for their insightful comments. Our responses to the comments are listed below. The referee's comments are italicized and our responses are in normal print.

Major Issues:

1) *First, the authors found that "agreement is excellent" between the NCEP/NCAR reanalysis (referred to as Reanalysis 1 in the paper) and the GISS GCM results*

(lines 5-7, page 12261) but no agreement between the model and the NCEP/DOE reanalysis (denoted as Reanalysis 2 in the paper). More specifically, the decreasing trend in cyclone frequency over 1980-2006 captured by the model was present in the NCEP/NCAR reanalysis but was clearly missing in the NCEP/DOE one. They stated that the NCEP/DOE reanalysis is "an updated version of Reanalysis 1 incorporating updated physical parameterizations and various error fixes" (lines 20-22, page 12257), which one would assume should produce better quality data closer to reality. Then, curiously, why would the GISS model results being in agreement with the NCEP/NCAR reanalysis only be adequate? If indeed the NCEP/DOE data is more reliable, even though their GCM results agreed with the NCEP/NCAR data, it implies that increases in greenhouse gases over the past 5 decades did not necessarily lead to the decline in the cyclone frequency over 1980-2006, which is the foundation of this work. Apparently, this indicates that the validity of this work is in question. Therefore, this reviewer suggests that the authors investigate why there is such a glaring discrepancy between the two reanalysis datasets and make sure that the trend in cyclone frequency, captured in the GISS GCM results, is in fact correct before they pursue further interpretation.

Climate model results need to be validated rigorously using observational data before being used in applications. Statements such as "with the understanding that the 1980-2006 cyclone trend from reanalysis 1 is tentative (since it is not seen in reanalysis 2, but it is supported by the GCM simulation)" (lines 9-10, page 12264) do not lend support to their hypothesis. On the contrary, the reviewer came away with the impression that the authors seemed to be enchanted by Reanalysis 1 in agreement with the model results, rather than the other way around.

Trends in mid-latitude cyclones have previously been identified in the NCEP/NCAR Reanalysis 1 dataset (McCabe et al., 2001; Gulev et al., 2001) and have been found within the observational record without use of reanalyses (i.e. weather maps (Zishka and Smith, 1980) and surface data (Wang et al., 2006)). The trend in both NCEP/NCAR Reanalysis 1 and the GISS GCM are significant at the 99% level. The NCEP/DOE

Reanalysis 2 time series is relatively short (27 years) and displays large interannual variability in the number of mid-latitude cyclones. In calculating the trend in cyclones for Reanalysis 2, this variability leads to a large 95% confidence interval of -0.15 a^{-1} to +0.08 a^{-1}, a range which encompasses the trend detected in NCEP/NCAR Reanalysis 1 (-0.15 a^{-1}). Equivalently, using a Student's t-test with the null hypothesis that the slopes (dC/dt) in the two Reanalyses are equivalent, we found that the hypothesis cannot be rejected at the 95% level. So the two reanalyses are in fact not inconsistent.

We will add text to the introduction, Section 4, and the conclusion in order to make this clearer to the reader.

The reviewer's statement, *"Climate model results need to be validated rigorously using observational data before being used in applications,"* sets a very high bar for application studies investigating the potential impacts of climate change, and certainly that standard is not met in publications investigating regional climate change. Rigorous validation of climate models for regional applications is difficult, but these applications are important for drawing attention to possible societal impacts of climate change and stimulating further research. The decrease in cyclone frequency is robust across GCMs, makes sense in terms of basic climate physics, appears to be present in the observational record, and has very important implications for air quality management. We believe that this meets the standard for publication.

2) *The second problem is the authors' decided view, without support from references and/or their own results, that "air quality is sensitive to cyclone frequency, not intensity" (line 11, page 12257), which is the foundation of their approach to quantifying cyclones. This statement seems to be counter-intuitive in this reviewer's opinion. The intensity of a cyclone is intimately linked to the intensity of the downstream high pressure system which impacts the regional build-up of pollutants and subsequently the occurrence of O3 exceedence in the eastern U.S. Furthermore, the intensity of a cyclone affects the intensity of convection and the horizontal areal extent of its influence, which are all important factors affecting the regional pollution build-up and transport. It can be*

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misleading to think of a cyclone as an isolated system. The use of an index that accounts for both cyclone frequency and intensity is imperative in this work.

We disagree with the reviewer. Logan (1989) found that slow moving and weak anticyclones are more readily associated with pollution episodes than their stronger analogs. Hegarty et al. (2007) also found that the intensity of their most frequent "map types" containing high pressure systems is negatively correlated with summer ozone levels, indicating that the less intense high pressure circulations (lower pressures) lead to higher ozone levels. Owen et al. (2006) found that mid-latitude cyclones effectively ventilate the North American boundary layer whether they are intense or weak.

In any case, we show in our work that cyclone frequency is indeed a good predictor of the interannual variability of ozone pollution (Figure 7).

We will add text to the introduction and Section 2 to clarify this for the reader and also point out in Section 3 that our result in the bottom two panels of Figure 7 (correlation between episodes and cyclones) supports our argument that frequency is the dominant variable

Specific Comments:

Figure 2 showed the GISS GCM simulations over 1950-1977 for comparison with Reanalysis 1. How about the ones over 1980-2006 for comparison with Reanalysis 1 and 2?

The main point of Figure 2 is to show general consistency in spatial patterns and frequency statistics between the different products. The distributions for 1980-2006 are similar to that for 1950-1977. We have added text to this effect in the revised manuscript and a figure showing this can be accessed at: ftp://ftp.as.harvard.edu/pub/exchange/eml/response_figure.png

Not sure why Figure 3 is needed. They could manage to put the blue/red boxes on Figure 2 which would be sufficient for their purpose.

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In our view, the southern track would not show up well in Figure 2. Also, Figure 3 shows individual cyclone tracks, i.e., the actual predictor variable that we are using, and so this Figure is useful as an illustration of our method. We have added some text to explain the purpose of Figure 3.

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