

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.

Anonymous Referee #1

Received and published: 3 July 2008

The authors presented an investigation of the relative roles climate change and emission reductions play in air quality with a focus on the impact of decreased mid-latitude cyclone frequency on the occurrence of O₃ episodes in the northeastern U.S. It is a straightforward paper, well organized, and clearly written.

This reviewer has two critical issues with the paper. 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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Interactive
Comment

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.

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

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

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 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.

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?

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 12253, 2008.

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