Atmos. Chem. Phys. Discuss., 8, S8661–S8668, 2008
www.atmos-chem-phys-discuss.net/8/S8661/2008/


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.

**E. M. Leibensperger et al.**

Received and published: 30 October 2008

We respond below in detail to Reviewer #1's second round of comments.

The reviewer's comments are ***bold/italicized*** and our responses are in normal print.

***This reviewer did not find the authors' response satisfactory.***
***This reviewer was well aware of the agreement between GISS and R1 as, in their original version, the authors elaborated on the agreement between the two throughout the entire paper. In contrast, they avoided comparing the model with R2; they compared one segment of the model results (1950-1977) with R1 in spite of the fact the decline in cyclone frequency occurred in 1980-2006 (Figure 2).***

The reviewer seems to be confused over the purpose of Figure 2. Figure 2 focuses on the spatial distribution of cyclones for comparison to the 1950-1977 observations of Zishka and Smith (1980), and simply demonstrates that the spatial distributions are consistent across datasets. It does not matter for that purpose whether one uses 1950-1977 or 1980-2006. We have provided an additional figure (ftp://ftp.as.harvard.edu/pub/exchange/eml/response_figure.png) that shows the 1980-2006 distribution of mid-latitude cyclones in the GISS GCM, NCEP/NCAR Reanalysis 1, and NCEP/DOE Reanalysis 2. As expected, the spatial distribution does not significantly vary between the two time periods. If the reviewer insists we will add this 1980-2006 Figure to the paper but it seems to us to be unnecessary.

***In Section 4, the disagreement between the model results and R2 can be readily inferred based on the comparisons between R1 and R2 and between R1 and the model. Now, in response to this reviewer's first critical concern, the authors decided that "the two reanalyses are in fact not inconsistent" based on the trend of -0.15 a-1 in cyclone frequency calculated from R1 and -0.15 a-1 - 0.08 a-1 from R2. These numbers are marginally close and the conclusion can go either way. The reviewer found it hard to be swayed by these two numbers only without being given meaningful and in-depth analyses on the possible causes for the discrepancy between the two reanalysis datasets.***

While we agree with the reviewer that the overlapping confidence interval of the reanalysis trends is marginal, an in-depth analysis of the discrepancies between the two reanalysis datasets is outside the scope of our expertise. We have added a statement to that effect in the text.

***One of this reviewer's previous comments that "Climate mode results need to be validated rigorously using observational data before being used in applications" was made specifically toward the problems in the authors' approaches and points of views, and was not meant to be a philosophical one. This reviewer hoped to see the authors address convincingly the large discrepancy between***



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***the two reanalysis datasets and between the model results and R2. However, again, the authors seem to be confident enough once "the decrease in cyclone frequency is robust across GCMs". It would be indeed "robust" if this result was captured in different observational datasets, and that was the point this reviewer tried to get across, not to "set a very high bar" for anyone.***

We are currently analyzing the cyclone trend from NOAA daily weather maps as suggested by Reviewer # 2. Maybe this will help to allay Reviewer #1's concern? We have replaced "robust" by "consistent".

***The authors disagreed with this reviewer on the second critical issue based on the results from Logan (1989), Hegarty et al. (2007) and Owen et al. (2006). It appears to this reviewer that the point in Logan (1989) is to make a link between the high pressure system and high O3 episodes without giving consideration to the intensity of the pressure system. Logan (1989) did not explicitly and quantitatively define how weak is "weak" for the slow moving high pressure system that was associated with the occurrence of O3 episodes, and in many places of the paper she simply dropped "weak" and used "slow-moving high-pressure system".***

We disagree with the reviewer. From Logan (1989, page 8519):
"While ozone episodes were associated with tracks of anticyclones, the converse was not true. I examined the characteristics of 34 anticyclones that moved through the region in 1978 and 1979, between May and August. All four of those that persisted for 4 or more days were associated with ozone episodes, as were five of the 10 that lasted for 3 days, but only six of the 20 that lasted for 2 days. ... Of the anticyclones that lasted 3 or more days, the median pressure for those associated with episodes was 1022 mbar, while it was 1028 mbar for those that were not; the average surface pressure for these months is 1016 mbar."

The following sentence summarizes her finding: "The analysis of anticyclones shows



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that ozone episodes occur preferentially in the presence of weak, slow-moving, and persistent high-pressure systems, as they migrate from west to east, or from northwest to southeast, across the eastern half of the United States."

***Let's take a look at the more recent two references the authors used. In the 5 map types Hegarty et al. (2007) identified, the first map type over their study domain is dominated by the Bermuda High and Map Type 5 is the later stage of Map Type 1, and the domain-averaged sea level pressure of these two map types is positively correlated with the O3 level (Table 3). Map types 2-4 showed the dominance of low pressure systems whose SLP was negatively correlated with the domainaverage O3 level (Table 3). It baffles this reviewer where the authors got the idea that "their most frequent map types containing high pressure systems is negatively correlated with summer ozone levels". Isn't it counter-intuitive just thinking about it?***

Below, we have reproduced Table 3 from Hegarty et al. (2007) and added a column, which contains the descriptions of the map types from their Table 1. These are their descriptions of the synoptic conditions of each map type.

**MAP TYPE I:**

From Hegarty et al. (2007):
"For map type I the 1020 hPa contour was much further onshore in 2002 than in any other year suggesting a more intense Bermuda High pattern which produced warmer, more stagnant and less cloudy conditions over the northeast." As they point out, the positive correlation between the strength of the Bermuda High (Map Type 1) and air quality in the northeastern United States is indeed logical. A stronger Bermuda High means that the Northeast will actually feel the effects of it. The Bermuda High will also induce southwesterly flow, which brings pollutants from the Ohio River Valley into the Mid-Atlantic and Northeast. The strength of the Bermuda High is almost more of an on/off switch in terms of air quality in the Northeast.



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**Table 1.** Tables 1 and 3 from Hegarty et al. (2007)

| Map Type | Correlation Coefficient, r | Description |
|---|---|---|
| I | 0.55 | Bermuda High |
| II | -0.72 | High-pressure centered over southern Ohio Valley and a trough off the east coast |
| III | -0.94 | Large high-pressure system centered north of the Upper Peninsula of Michigan |
| IV | -0.46 | High-pressure centered east of Maine; low-pressure center or trough near western Michigan |
| V | 0.23 | A sharp trough extending from a low over Quebec through eastern New York and down the east coast, likely in association with a cold front |
| All Types | -0.17 | |

**MAP TYPE II:**

From Hegarty et al. (2007):
"In contrast, the high-pressure system of map type II was weakest in 2002 with a central pressure just over 1018 hPa in the extreme southeast corner of the domain. It was strongest in 2004 with a central pressure near 1022 hPa. These characteristics resulted in weakened transport of cooler, aged Canadian air into the northeastern half of the study domain during 2002 compared to 2004."

The reviewer seems focused on the cyclone that has moved offshore and not the anticyclone that has parked itself over the eastern US. In their analysis, Hegarty et al. focus, as we would, on the high-pressure system. As they point out, stronger anticyclones bring clean, cool air from Canada to the US. This would be a negative correlation



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between ozone and the strength of the anticyclones, which is precisely what Hegarty et al. found.

**MAP TYPE III:**

From Hegarty et al. (2007):
"The large Canadian high pressure represented by map type III was by far the strongest in 2004, with an average central pressure near 1023 hPa and a closed 1016 hPa isobar extending to the east coast of New England and as far south as the Gulf of Mexico (expanded domain plot not shown). It also appeared strong for 2000 and 2001 but was weakened considerably in 2002 and 2003. This indicates that cooler, drier and relatively aged air was situated over much of the northeast in 2004, 2000, and 2001, in contrast to 2003 and especially 2002."

Again the reviewer misses the fact that a large anticyclone has parked itself over the northeastern US. Hegarty's review of the situation makes sense and further explains the clean conditions found in 2004.

The correlation here is especially negative. If the reviewer wants to focus on the cyclone, his argument for cyclone intensity still falters. A negative correlation means that higher pressures (weaker cyclones, although the average could be dominated by the strength of the anticyclone) are correlated with lower ozone.

**MAP TYPE IV:**

From Hegarty et al. (2007):
"For map type IV, the most striking difference was the strength of the offshore high in 2004. This system had a closed 1024 hPa isobar extending into southwestern New England and a 1020 hPa isobar protruding into the southern Appalachian Mountains. For this map type, 2004 featured cooler conditions and a greater influence of maritime air over the northeast than during any of the other years."

Again, the stronger high-pressure system has a greater influence in the Northeast by



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providing maritime air. There is a low-pressure system moving in from the west and the same argument can be made about the negative correlation meaning stronger cyclones are correlated with lower ozone.

**MAP TYPE V:**

From Hegarty et al. (2007):
"Examination of map type V showed only minor differences in the strength of the trough along the east coast. However, the trough was somewhat deeper in 2004 as evidenced by the location of the 1014 hPa isobar which extended to just off the coast of the Carolinas, a meteorological situation that facilitates stronger and more penetrating cold fronts."

This is very important in our current debate: "... only minor differences in the strength" coupled with a pretty low correlation coefficient (+0.29). We concede that 2004 does have a stronger trough that allows cold fronts to reach further south, but all of the years appear to have troughs capable of producing cold fronts that influence the Northeast.

In summary, we find most of the reviewer's comments on this paper to be questionable. Hegarty et al. refer to the maps and synoptic conditions as anticyclones and analyzes them as such.

For further information on the map types, please see Figures 2 and 6 from Hegarty et al. (2007).

***In Owen et al. (2006) this reviewer did not find any statement suggesting that "mid-latitude cyclones effectively ventilate the North American boundary layer whether they are intense or weak". Ventilation is a function of wind speed and boundary layer height. Conceivably, the intensity of a cyclone determines the intensity of ventilation and hence that of continental export which is intimately linked to the regional buildup in the eastern U.S. In fact, in paragraph #44 on page 9 of 14 of Owen et al. (2006), Owen et al. stated that "the export height,***



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***however, was limited to the lower free troposphere because of the relatively weak intensity of the low-pressure system". This implies that a stronger low pressure system may lead to higher export height and subsequently the export may not be limited to the lower free troposphere only, which is to say a stronger low pressure system would be conducive to a stronger continental export.***

But in terms of U.S. air quality we are simply interested in ventilation of the continental boundary layer; we don't care about the altitude of the export.

Finally, and to make the point again, we find in the paper a good anti-correlation of pollution episodes with cyclone frequencies, regardless of cyclone intensity.

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