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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

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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). 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.

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 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. 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". 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 domain-average 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

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negatively correlated with summer ozone levels". Isn't it counter-intuitive just thinking about it? 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, 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.

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

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