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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of the Bermuda High on interannual variability of summertime ozone in the Houston-Galveston-Brazoria region" *by* Yuxuan Wang et al.

Anonymous Referee #1

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Inspired by previous research on the impact of the Bermuda High on surface ozone in Eastern and Southern US, the authors constructed an MLR model to quantitatively describe the relationship between MDA8 ozone in the Houston-Galveston-Brazoria region and the Bermuda High, using an intensity index, BHI, and a location index, BH-Lon. The analysis found that the Bermuda indices, in particular, BH-Lon, are better predictors for ozone prediction than local meteorological parameters such as temperature. The authors suggested that underlying mechanism is that the location and intensity of the BH control whether low-ozone maritime air from the Gulf can enter the HGB region, and the method may apply to other coastal regions along the Gulf coast. The paper is well written and provides good insights into the topic. I recommend the paper for

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Discussion paper

publication if the following comments are addressed.

Major comments

1. I am concerned that the de-trending method for BH-Lon is not robust and add noise to the regression analysis. I also suspect that the V wind may not be necessary for August if there weren't the statistical noise induced by the de-trending method.

Fig. 4 shows that BH-Lon has large inter-annual variations and tends to cluster in two groups. This feature is most significant in August, where the two groups of data are distant apart from each other. One group is around -70o (eastern group) and the other around -100 o (western group). As a result, the 7-year moving averages may be very noisy, depending on the specific time series. For example, in both Year 1997 and 2000, BH-Lon are -100 o, and we expect the values for these two years are close after de-trending. However, after removing the 7-year moving averages, they become -7o and -3 o (calculated based on the figure), respectively. The 4 o difference (quite significant according to the regression coefficient) between the two years results mainly from the variation in the 7-year averages. Notice that the 7-year moving average for 1997 includes two "eastern" years (1995 and 1999), while that for 2000 only includes one (1999). This kind of noise induced by the de-trending method may degrade the explanatory power of BH-Lon, especially in August.

In my opinion, it is a better method to just use the raw data because the trend is not significant in the first place. Even for August, I suspect that the trend is mainly from the first 4 years. Should the first 4 years are removed or a non-parametric regression method is used, the trend for August might well also be insignificant ($p>0.1$).

Minor comments

1. Line 195-197. The authors used a southern Great Plains domain that is different from the definition in Zhu and Liang (2013). Are these two domains very different?

2. Line 255-256. The logic here is a little confusing because the previous paragraph

seems to suggest that BH-Lon is able to capture the intra-season variation pretty well (at least much better than temperature does), so what "described above" does not support the "month by month" analysis. That said, I am fine with the "month by month" analysis.

3. Line 269. "The squares of the Pearson correlation coefficients (R2)" should be changed to "The coefficients of determination (R2)" because the latter is the correct term in this context and is also consistent with the caption of Table 1.

4. Figure 8. Fig. 8 used the stream line. But these stream lines were not discussed in the text. I suggest to use wind vectors rather than stream lines, for 1) the consistence with other Figures (Fig.3 and 9); 2) The V wind speed for the two months is mentioned in the text (Line 348-349) and the information can be better visually shown with wind vectors.

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