

Response to Reviews

We thank both reviewers for their constructive comments to improve the manuscript. Their comments are reproduced below with our responses in blue. The corresponding changes in the manuscript are highlighted in blue.

Reviewer #1

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 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 -70° (eastern group) and the other around -100° (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° , and we expect the values for these two years are close after de-trending. However, after removing the 7-year moving averages, they become -7° and -3° (calculated based on the figure), respectively. The 4° 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 ($p>0.1$).

We agree with the reviewer's concern and suggestion. To address them, we have conducted additional analysis, as presented below, and verified that our results are not affected by different detrending methods.

The following table lists the regression coefficient of determination (R^2) using different treatments of BH-Lon and O_3 . Note O_3 is the dependent variable and BH-Lon the independent variable, thus we favor the model settings in which both data are processed the same way. The 1st setting (raw data for both) has low R^2 for July and Aug when HGB ozone has a significant decreasing trend. Such trend is driven by decreasing emissions, a factor not considered in the MLR (hence poor model performance). The 2nd setting uses raw data for BH-Lon, as suggested by the reviewer, and this setting has R^2 values only slightly less than what we reported in the manuscript (i.e. the 4th setting). The 3rd setting uses the simple linear regression to remove trends in both BH-Lon and O_3 . This yields R^2 higher than 0.5 for all the months, although 10-20% lower than the 4th setting.

BH-Lon and O_3 in MLR	June	July	August
(1) Raw data for both.	0.52	0.35	0.24
(2) Raw data for BH-Lon only; O_3 is de-trended by subtracting the 7-yr moving average from the raw data.	0.60	0.67	0.65
(3) Both de-trended by removing the linear trend from the raw data.	0.52	0.58	0.56
*(4) Both de-trended by subtracting the 7-yr moving average from the raw data.	0.61	0.72	0.70

*This setting is reported in Table 1 of the manuscript.

We have added the above table in the Supplementary Material (Table S2) and added more discussion on this issue in the main text (line 283-289). We chose to report the 4th setting in the main text based on the consideration of four factors: (1) the BH-Lon and O_3 (and other variables) should be processed in a consistent manner in the MLR model; (2) O_3 data needs to be de-trended to remove the influence of changing anthropogenic emissions; (3) we do not know if the ozone trend is linear during the timeframe of 1990-2015, thus the 7-yr moving average appears to be a better choice to de-trend ozone data; and (4) a number of previous publications which analyze the effects of meteorology on air quality variability de-trend the data by subtracting the moving average from the raw data (e.g. Shen et al., 2015; Tai et al., 2010) and thus our results can be directly compared with them.

A remaining question is whether we should include V wind into the regression model for August. We found that the model performance would be significantly degraded if V wind was removed, regardless of which de-trending methods were used. This suggests the effect of V wind in the MLR is not random or resulted from noises, but robust. The table below shows the MLR R^2 for the model in August if V wind is

removed.

BH-Lon and O ₃ in August MLR model	Without V	With V
(1) Raw data for both.	0.08	0.24
(2) Raw data for BH-Lon only; O ₃ is de-trended by subtracting the 7-yr moving average from the raw data.	0.32	0.65
(3) Both de-trended by removing the linear trend from the raw data.	0.20	0.56
*(4) Both de-trended by subtracting the 7-yr moving average from the raw data.	0.41	0.70

*This setting is reported in Table 1 of the manuscript.

We have added the table in the supplementary material (Table S3), and clarified the main text (line 289-292). In addition, following the suggestion by the reviewer, we have added that for August the trend of BH-Lon would become insignificant if the first four years were removed from the time series (line 183-184),

References:

Shen, L., L. J. Mickley, A. P. K. Tai: Influence of Synoptic Patterns on Surface Ozone Variability over the Eastern United States from 1980 to 2012, *Atmos. Chem. Phys.*, 15, 13073-13108, 2015

Tai, A.P.K., L.J. Mickley, D.J. Jacob: Correlations between fine particulate matter (PM2.5) and meteorological variables in the United States: Implications for the sensitivity of PM2.5 to climate change, *Atmos. Environ.*, 44, 3976-3984, 2010

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?

We have corrected this typo in our old manuscript. The southern Great Plains domain is same as the definition in Zhu and Liang (2013), while the Gulf of Mexico domain is slightly different. Our Gulf of Mexico domain is located 2.5° east from the one in Zhu and Liang (2013), and this is because our domain was selected based on the correlation of SLP with the HGB-mean ozone, while their domain was based on the correlation of SLP with low-level jet. We have clarified in the text:

Line 201-205. "...we defined a pressure-based BHI as the mean SLP difference along the west edge of the BH, between the same location in southern Great Plains as selected by Zhu and Liang (2013) (35°-39°N, 105.5°-100°W) (box 2 in Figure 1a) and the other in the Gulf of Mexico (25.3°-29.3°N, 92.5°-87.5°W) (box 1 in Figure 1a) where the SLP exhibits the largest correlation with the HGB-mean ozone. Our Gulf of Mexico domain is located 2.5° east of that defined by Zhu and Liang (2013)."

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.

Thanks for making this good point. We have clarified in the text.

Line 263. “Considering the large intra-summer variation in ozone, BH and their association, the effects of the BH on the HGB-mean ozone are analyzed month by month ...”.

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.

Done.

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 consistency 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.

This is an excellent suggestion. We've replaced the stream lines in Fig 8 with wind vectors.