Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-571-AC1, 2016

Atmospheric Chemistry and Physics Discussions
Open Access
EGU

Interactive comment on "The Dynamical Impact of Rossby Wave Breaking upon UK PM10 Concentration" *by* C. P. Webber et al.

C. P. Webber et al.

c.p.webber@pgr.reading.ac.uk

Received and published: 4 November 2016

Response to the major comments made by Anonymous Referee 1

1. I appreciate the work done to create a "super site". However, few additional information would indeed help. A correlation plot between the 3 different sites before and after the "tendency outliers" removal would be great to have, and possibly these should be added to the electronic supplement. I would be very interested to see if the 3 sites do correlate at least after your corrections. If this is not the case, probably the super-site estimations are without any real meaning, as mainly influenced by local emissions.

Following the advice to clarify the data verification steps taken to generate a "super-site", further analysis was undertaken. Pearson's correlation coefficients were determined between each constituent PM10 dataset, prior to and following the data verifica-

tion step of removing tendency outliers. Prior to the data verification step the Pearson's correlation coefficient values between each [PM10] dataset varied between 0.73 and 0.86, with the lowest correlation coefficient seen between the Leamington Spa and Birmingham Central [PM10] datasets. Following the data validation stage, the Pearson's correlation coefficient values varied between 0.86 and 0.87.

These results are referred to in the main text on P4 L50:

"Additional analysis (not shown) has found that the data validation step of removing PM10 spikes has improved the Pearson's correlation coefficient between each original PM10 dataset. In the original [PM10] datasets, the correlation coefficients between the three observational PM10 sites varied between 0.73 and 0.86. Following the data validation step, the correlation coefficients varied between 0.86 and 0.87. "

2. I was not really able to find the information regarding threshold in PM10 concentration in the manuscript of Gehring et al. (2013), i.e. concentration value below which PM10 does not have any health effect. To my knowledge, this is matter of debate, and the World Health Organisation writes that: "Small particulate pollution have health impacts even at very low concentrations indeed no threshold has been identified below which no damage to health is observed." (see http://www.who.int/mediacentre/factsheets/fs313/en/). Therefore I do not argue with the thresholds selected in section 2.2, but I would remove the health considerations. See also the discussion in Burnett, R. T. et al. (2014).

Reference

Burnett, R. T. et al. An integrated risk function for estimating the Global Burden of Disease to ambient fine particulate matter exposure. Environ. Health Perspect. 122, 397-403 (2014).

We agree that PM10 below the threshold used has not been shown to be dissociated with detrimental health effects. The threshold used is intended to highlight a thresh-

old that concentrations above which have been shown to lead to significant increases in detrimental human health effects. Perhaps a better argument would be to refer to Katsouyanni et al. (2001) instead. Katsouyanni et al. (2001) find a statistically significant increase in mortality rates, associated with episodic PM10 increases of 10 μg m^{-3} above the background mean concentration.

The justification for the threshold has remained health motivated. We have instead used the results found by Katsouyanni et al. (2001) in the APHEA2 Project to motivate the hazardous UK Midlands [PM10] threshold.

The point that PM10 is hazardous to human health at low concentrations and a reference supporting this have been added to the text (P5L58).

"It is worth noting that while significant increases in detrimental health impacts have been shown for the threshold used in this study, detrimental health effects have been shown to occur at lower ambient [PM10] (Brook et al., 2010)."

An additional reference was included:

Brook, R. D., Rajagopalan, S., Pope, C. A. III, Brook, J. R., Bhatnagar, A., Diez-Roux, A. V., Holguin, F., Hong, Y., Luepker, R. V., Mittleman, M. A. and Peters, A.: Particulate matter air pollution and cardiovascular disease an update to the scientific statement from the American Heart Association. Circulation, 121, 2331-2378, 2010.

Response to the minor comments made by Anonymous Referee 1

3. Page 1, line 2: please mention that only daily PM10 values are used

This clarification was added to the abstract at this point.

4. Page 2, line 53: I am puzzled with the "climatological mean sea level pressure", defined here. I think that "climatological" refer to 30 years average. Maybe the authors refers to daily mean sea level pressure as produced by the ECMWF era-interim product.

The terminology has been changed, as we have not used 30 years of MSLP to obtain our averages. The term "daily mean" has replaced the term "climatology".

5. Page 2, line 54: Please specify that theta is potential temperature.

Theta has been replaced by potential temperature on Page 2, line 46

Θ *has been replaced by Potential temperature on Page 2, line 47.*

The text "The Θ-2PVU surface" has been replaced by "Θ-2PVU" on Page 2, line 48.

6. Page 5, line 62: You refer to a correlation between RWB and [PM10]. From your work I understood that the BI was used to represent RWB. I would therefore either change RWB with BI or specify that the time lag is estimated between BI and [PM10].

The text has been edited, so that RWB has been replaced with BI.

7. Page 6, line 74: There is an inconsistency between figures and text. In Fig. 3 the caption mentions only log[PM10] and not loge (or ln). However, in the color bar, the absolute values are used (i.e. without any logarithmic calculation). Why not use "ln" (natural logarithm) in the entire manuscript? Additionally, in Fig. 3 you mentioned that the solid/ dashed lines represent regions where the subset average of daily PM10 are higher/ lower than the mean of $\log_e$([PM10]). Should not be the logarithm in both cases? Alternatively you could remove the logarithm in the second case. The text read as the solid dashes are all the region with the average subset of daily PM10 higher than 2.3 μg m^{-3}.

We are unable to find log[PM10] in the caption of Fig. 3, however there does exist an inconsistency between $\log_e$[PM10] in the text and [PM10] in the figure (colour bar). This inconsistency has been corrected, so that the colour bar now represents ln[PM10] values. Furthermore all instances of $\log_e$([PM10]) throughout the text have been changed to read ln[PM10].

The text in the caption to Fig. 3 describing the solid contours has been altered. The *solid contours have been defined to, "constrain regions where mean ln[PM10] for each grid point is significantly greater than the entire dataset ln[PM10] mean (p<0.01)". This is consistent with the description in the main text.*

8. Page 8, line 74: I just disagree, as Sect. 3.1 only showed that without RWB events (i.e. BI lower than 0), PM10 is more effectively removed (transported due to the zonal flow, while Sect. 3.2-3.3 showed which kind of special RWB could lead to increased [PM10].

The line in question refers to raised UK [PM10] associated with positive BI values. This is shown, in Fig. 2 b) and c) for GP 2 and 3 respectively. Due to the positive correlation that exists between BI and UK [PM10] in Fig. 2 b) and c), it can be stated that increased BI values (positive BI values) are associated with increased UK [PM10].

9. Page 9, line 15: I find this paragraph extremely difficult to understand, and I had to read it many times to guess what the author means. Would it be possible to reformulate it?

The following text has replaced the paragraph originally beginning on P9L14:

"The mechanism primarily dictating the direction of RWB is the meridional hear of the zonal wind, which is imparted on the background atmospheric flow by the EDJ. To the north/ south of the EDJ, a cyclonic/ anticyclonic shear is imparted on the background atmospheric flow. Consequently, the region of most frequent CRWB has been found to occur to the north of the EDJ mean-state, in the Northwest Atlantic region (Weijenborg et al., 2012). These regions are similar to those for ACRWB and predominantly to the south of the EDJ mean-state. A hypothesis has been developed to explain the importance of CRWB in the Northeast Atlantic/ European region. It will be shown that the majority of Northeast Atlantic/ European CRWB events, which were found to significantly increase UK PM10], are dependent on the prior occurrence of ARCWB."

10. Page 12, line 10: I think you mean [PM10] > $\overline{[\mathrm{PM10}]}$ + 10 $\mu g\ m^{-3}$. However, please check the major comments on this issue.

This inequality has been corrected in the text.

11. Discussion: Would be good for the author to extend the outlook in their discussion:

11.1 From this work I could also conclude that this should be valid not only for PM10, but also for PM2.5, which are by far more influenced by transport due to their lower settling velocities. Do the authors have any opinion on that?

This is a good suggestion and therefore a brief discussion has been included on P13L41

"In this study, PM10 advection from Europe is hypothesised to greatly influence UK [PM10] episodes. A potential extension for this study could be to analyse the relationship between the smaller PM2.5 and RWB. PM2.5 is a smaller and subsequently lighter particle than PM10, with a reduced gravitational settling velocity. Consequently, PM2.5 is more readily advected than PM10 and RWB may therefore be more influential in facilitating the advection of PM2.5."

11.2 Could the author extend the manuscript or discuss the absolute frequency of European CRWB?

We feel that sufficient literature has been included, for readers to find this information; for frequency Fig. 2 in Masato et al., 2013 and for distribution Fig. 5 in Weijenborg et al., 2012.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-571, 2016.