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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Receptor modelling of secondary particulate matter at UK sites" *by* A. Charron et al.

A. Charron et al.

r.m.harrison@bham.ac.uk

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RESPONSES TO THE COMMENTS OF THE REVIEWERS

We are pleased to respond to the points raised by the reviewers. The vast majority are minor points which we have addressed by small changes to the manuscript which we believe will have improved the clarity. We respond here to the points of substance raised by the reviewers.

Two reviewers point out that the reference in the title to "UK sites" is misleading as the vast majority of the results are based upon a single site, i.e. Harwell. In response, we have changed the title to "Receptor Modelling of Secondary and Carbonaceous Particulate Matter at a Southern UK Site" which we believe better describes the content

of the paper.

Two of the reviewers questioned the novelty of the work and Reviewer #1 states that "there are previous publications (cited) on this matter and same dataset". We heavily dispute this inference. The only earlier work of which we are aware which analysed aspects of this specific dataset is the work of Abdalmogith et al. (2005). That paper analysed a much smaller dataset from the same data series all dating from before the current data, and only for sulphate, nitrate and chloride, and did not take account of the carbonaceous aerosol, as data were not available at that time. Additionally, the work of Abdalmogith et al. (2005) was based solely on back trajectories and did not use the Concentration Field Map Method utilised in this paper and did not look at other factors such as dependence upon temperature and season. Consequently, the conclusions of the Abdalmogith et al. (2005) paper are far more limited than those from the current paper.

We have looked carefully at the Discussion section and see no obvious means of shortening it without significantly reducing the evidence content and reject the idea of reporting the results before embarking on the discussion as this would simply lead to duplication of text and hence greater length. We have extended the Conclusions section by a further paragraph in response to the comment that this section was rather brief, and the fact that we had omitted to draw attention to one of the more significant conclusions of the work relating to the elemental carbon data.

Referee #1 complains that we have included too little information about the Harwell site and other possible local influences. We are not aware of major industrial activities in the vicinity, other than the power station to which we have referred, and we have given very full details relating to the traffic activity on the "nearby" A34 at a distance of approximately 2 km. Referee #2 raises the issue of emissions from the power plant which have been addressed in some detail in the earlier cited paper of Jones and Harrison (2011) and hence we do not think it appropriate to repeat those results in this paper.

Referee #1 questions the difference between trajectory clusters C5 and C7 and between C1 and C6. C5 and C6 include continental air masses, unlike C1 and C7. Referee #1 also comments that C2 and C6 are very similar in values whereas in fact C2 concentrations are very much higher than C6, although this is somewhat obscured by the log scale of the box plots. Although the trajectory portrayed for cluster C6 in Figure 5 crosses only Brittany, it must be remembered that this line is representative of many back trajectories of which probably 50% will have had a much greater residence time over mainland Europe.

Referee #1 also questions why the differences are so big between concentrations in Figures 7 and 8 and provides the example that in Figure 7 with east winds, concentrations are about 25 μg/m3 and in Figure 8, back trajectory C2 has a concentration of around 15 μg/m3. We make the point earlier in the text that wind directions are much more reflective of local influences and can hence be highly elevated, while back trajectories include a wide range of regional influences and hence a great deal more averaging, producing lower concentrations.

Referee #2 is much more positive in his/her comments. Both he/she and Reviewer #3 comment on the fact that some sulphate will arise from sea salt. This is a valuable point which we have now incorporated in the manuscript, taking account of sulphate:chloride ratios in sea salt. Reviewer #2 also asks why the CFMM maps for OC were not correlated with the EMEP inventory for OC precursors. This was not carried out because we were unaware of the EMEP data. The correlation has now been examined, found to be significant and included in the paper.

Two reviewers raised the question of the conversion factor from OC to OM and a reference has now been given to the paper from which the 1.8 factor was taken. We agree with Referee #3 that the r2 value between secondary OC at Harwell and EROS does not merit a description of “rather closely related” and have used a more gentle statement to sum up the relationship between the sites. Reviewer #1 questions the value of including the EROS data. We feel that these provide a valuable source of additional

information by more clearly demonstrating the regional nature of the secondary organic carbon.

Reviewer #3 asks why only cells with >20 endpoints are regarded as statistically significant. This is a very complex matter and the use of the number of 20 is based upon past experience of the method, rather than any formal statistical evaluation, which would be very hard to conduct.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 27255, 2012.