

Interactive comment on “On the aerosol weekly cycle spatiotemporal variability over Europe” by A. K. Georgoulas and K. A. Kourtidis

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We would like to thank the anonymous referee #2 for his comments that have substantially improved the quality of the revised version of our paper.

General comments:

1) “From Fig. 1 I understand that the most reliable data are shown in panels g and h. In Section 3.1 the authors focus their attention in the results shown in panels a and b. I think that they should to focus on the results using the datasets with more quality and omit to extract conclusions from Fig. 1a and Fig. 1b. On the other hand the different maps in Fig. 1 are hardly visible and legible. Please, increase the size of the maps, or simply delete some of the panels containing not very relevant information (e.g. panels

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c,d,e, and f).”

Answer: We thank the reviewer for this comment because we had the opportunity to improve our reasoning in the revised version of the paper. There is a misunderstanding here which is possibly due to the quantity of information and figures given in the corresponding paragraph. However, in this paper we propose some corrections to an already existing method, so we could not avoid showing first the maps without the correction and afterwards the maps with the corrections since this is a significant point of this work. The initial patterns (Figs. 1a and 1b) were corrected using the $\pm 1000\%$ limit. This led to the corrected patterns which appear in Figs. 1c and 1d. This correction is critical and is an addition to the method originally proposed in Xia et al., which could return false values for some pixels as shown here. The corrected with the $\pm 1000\%$ limit WCI values (Figs. 1c and 1d) were used thereafter for the investigation of the weekly patterns. The two aerosol flags were used only diagnostically (separately and not successively). So we removed Figs. 1e, 1f, 1g and 1h from the manuscript and are now available as supplementary material (Supplfig. 1). We make this clear in the revised version of our paper that what we wanted to show with the application of the two aerosol flags is that they did not change significantly the weekly cycle patterns, which is an indication that the observed weekly cycles are due to continental aerosols and hence could be attributed to anthropogenic activities. The panels appearing in Fig. 1 of the revised manuscript are now much bigger.

2) “I would like to know if the corrected dataset (after to correct the data with flags 1 and 2) is also used for the APD results showed in Section 3.2. It is not specified in the text and I think that they will might to use and comment only the results for the dataset with the better quality.”

Answer: As discussed in Sect 2.2, the WCIs and the APDs were calculated from two separate datasets (dijmy,new values and wimy values) that emerged both from the original MODIS dataset. The $\pm 1000\%$ correction that was applied on the “wimy” dataset for the calculation of the WCI values was an empirical method which emerged from

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a significant number of tests using several critical values. We found that the limit of $\pm 1000\%$ was the best since it combines effectiveness in clearing the patterns and preserving the statistical sample. The APDs were calculated from the “dijmy,new” dataset and though a similar correction could not be used. The aerosol flags were applied only diagnostically as discussed above and in the manuscript in order to show that the WCI patterns do not change significantly which is a sign of the continental origin of the aerosol weekly cycle. Keep in mind that the FMR which is used in the aerosol flag calculations as stated in the manuscript is not considered reliable over land. We had to be very careful using this parameter and this is why we used the aerosol flags as a diagnostic tool only. Since, no significant changes were observed on the WCI patterns we did not have to use the flagged AOD values for the calculation of the APDs. In fact, the spatial averaging method (originally used in Quaas et al., 2009) that we applied for the investigation of the weekly variability over the 6 selected regions afterwards, could be considered a correction, since it eliminates the contribution of episodically high values which could insert drawbacks.

3) “Fig. 4 is hardly legible. I think that the authors should to keep only some of the panels. For example I suggest to keep only one of the two MODIS datasets (or AQUA or TERRA) panels since both show very similar results.”

Answer: We agree with the anonymous reviewer that the figure is very small in the printed version of our manuscript. This is possibly because of the standard templates used for the production of the discussion paper. Also we have to notice that in the ACPD version the pages are printed as “two in one” and this makes the panels even smaller. We will collaborate closely with the production team of ACP to obtain the best possible figure quality (we have also provided the production team with all the individual subfigures appearing in the manuscript) (also see the answer to comment 1 by Dr Weedon). We tried to reduce the margins around the panels in Fig. 4 in order to increase the size of these figures. We did not want to keep only one sensor (TERRA or AQUA) in the text and provide the other sensor figures as supplementary material

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because the use of both sensors is a significant point of this paper. Since the two sensors, overpass different time of the day, we get an insight into the diurnal variability of the weekly cycle. Moreover, due to the fact that the data used here from the two sensors are for different time periods, we also get some insight into the stability of the patterns throughout the years.

4) “I would like to know why in Section 3.4 the authors not focus only their descriptions and discussions in the datasets with the corrections (which appear in Fig. 8). I think that it is a bit confusing for the reader to follow the text, especially concerning the differences between Fig. 5 and 8. I will appreciate if they can clarify the goal of the section and try to simplify the text. Also I would like that the authors will consider to delete Fig. 5 or 8 and keep only the most interesting maps for the discussion of the main results.”

Answer: In fact the answer to this question was already given in the first comment by the reviewer. The two aerosol flags were used only diagnostically as discussed above. We removed Fig. 8 from the manuscript and it is now available as supplementary Figure (Supplfig. 2). We make this clear in Section 3.4 of the revised version of our paper that what we wanted to show with the application of the two aerosol flags is that they did not change significantly the weekly cycle patterns, which is an indication that the observed weekly cycles are due to continental aerosols and hence could be attributed to anthropogenic activities. The discussion primarily focuses on Fig. 5. The goal of this section is given in the first two paragraphs and then Sections 3.4.1, 2, 3 and 4 follow where the seasonal patterns and variability are discussed in detail.

5) “I would appreciate more discussion about the negative weekly cycles found over the South-Western Europe (region SWE). The authors detect the strongest negative weekly cycles during the summertime, arguing that “this is a local weekly cycle” (p. 1408, line 3). I cannot think that local emissions are greater during the weekends in the summer period over the Iberian Peninsula. One hypothesis can be linked to a possible indirect aerosol effect of the aerosols on the convective clouds (in line with Bell et

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al., 2008), with an enhancement during the mid-week due to the increase in aerosols emissions and a possible wash out of the aerosols by the rain. On the other hand I consider that the authors might point that Xia et al. (2008) showed some indications of negative weekly cycles over the Iberian Peninsula and Eastern Europe (see Fig. 9 in Xia et al., 2008)”

Answer: We thank the reviewer for giving us the opportunity to further discuss the negative weekly cycle appearing over South-Western Europe. We have added a short discussion on this, suggesting that like in Bell et al. (2008) the cloud lifetime effect and by extension the storm invigoration effect could explain this phenomenon. A direct response of clouds to the increase of aerosol emissions in the beginning of the week through the cloud lifetime effect gives increased total cloud cover (TCC) and precipitation is suppressed. The storm invigoration effect could finally lead to an intense precipitation in midweek washing out the aerosols. This is also supported by the fact that the negative weekly cycle is much stronger in summer when most clouds are of convective rather than frontal origin over the Iberian Peninsula. The AOD-TCC slope (results not shown here) is very strong (especially for AODs less than 0.2) and the AOD, TCC weekly variabilities are very close, which could be a sign of the aerosol indirect effect on cloudiness. We also cited Xia et al (2008) in this paragraph indicating that they also found a negative weekly cycle over the Iberian Peninsula.

Specific comments

1) “p. 1387, lines 12-13. There are works previous to 1958 analyzing the weekly cycles in urban pollution and their possible effect on meteorological variables (e.g. Ashworth, J. R., The influence of smoke and hot gases from factory chimneys on rainfall, Quarterly Journal of the Royal Meteorological Society, 55, 341-350, 1929). Rephrase the sentence.”

Answer: We agree with the anonymous reviewer #2. We have rephrased this sentence also citing two pioneer studies from J. R. Ashworth on the weekly cycle of meteorolog-

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ical and pollution parameters (Ashworth, 1929 and Ashworth, 1933).

- Ashworth, J. R.: The influence of smoke and hot gases from factory chimneys on rainfall, Q. J. Roy. Meteor. Soc., 55, 341-350, 1929.

- Ashworth, J. R.: Rainfall and atmospheric pollution, Nature, 132, 443, 1933.

2) “I agree with the interactive comment by Dr. Schultz. I think that in the Introduction, especially after to show the list of citations finding significant weekly cycles (p. 1389, lines 3-7), you might include some of the other works that did not find cycles. To complete the references suggested by Dr. Schultz I also recommend to cite the following comments and replies papers which focus on Europe: . . .”

Answer: Following the comments of Dr Schultz and the anonymous reviewer #2 we added in the introduction a paragraph citing a number of articles that have challenged the existence of weekly cycles (e.g. DeLisi et al., 2001; Schultz et al., 2007 and references therein; Hendricks Franssen, 2008; Bäumer and Vogel, 2008; Hendricks Franssen et al., 2009; Sanchez-Lorenzo et al., 2009). We agree that the inclusion of papers that do not favor the existence of weekly cycles leads to a more balanced introduction.

3) “Due to the results of the current manuscript for Eastern Europe, I suggest to cite (in the list which report significant weekly cycles) and discuss somewhere a recent paper published by Dr. Sitnov: - Sitnov, S.A. (2010): Weekly Cycle of Meteorological Parameters over Moscow Region, Doklady Earth Sciences, 431, Part 2, 507–513.”

Answer: We agree that this paper by Dr Sitnov was a significant contribution for the investigation of the weekly cycle of atmospheric parameters. In the revised version of our paper, we cite this work in the list with studies that report significant weekly cycles and we also discuss the fact that even for highly populated megacities (e.g. Moscow) the Weekly cycle index does not reveal a strong weekly variability of AOD550 in contrast to what was observed from space for other pollution parameters like tropospheric

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NO2 (Sitnov, 2010 for the region around Moscow).

4) “p. 1389, line 11. Delete the details of the periods that span the MODIS TERRA and AQUA. Few paragraphs ahead (in section 2.1) the periods for the analyzed data are specified.”

Answer: We agree with the reviewer. We have omitted these time periods in this paragraph in the revised version of our paper.

5) “p. 1391, line 8. Add Kistler et al. (2001) reference. - Kistler, R. et al. (2001): The NCEP/NCAR 50-year reanalysis: Monthly means CDROM and documentation, Bull. Am. Meteorol. Soc., 82, 247–267.”

Answer: We cite this paper in the revised manuscript.

6) “p. 1391 line 12 – p. 1393 line 14. I will appreciate more paragraphs in this text in order to clarify the different indexes used (APD and WCI), the test to improve the datasets, etc.”

Answer: We believe that the methodology was already explained in a very detailed way. However, following the reviewer’s comment we have made some minor changes in this section in order to clarify the different indexes and methods used.

7) “p. 1393 line 19. Delete “, as used originally in Quaas et al. (2009)”.”

Answer: We have deleted this phrase in the revised manuscript.

8) “p. 1395 lines 14-20. In this sentence delete “2000-2009”, “(Kalnay et al, 1996)”, and “The wind data are available on a 2.5x2.5_ spatial resolution.”. You described these details in previous Section 2.1.”

Answer: We rephrased this paragraph according to the reviewer’s recommendations.

9) “p. 1403, lines 5-15. The paragraph might be rewritten deleting details about the reanalysis data which has been explained in previous sections.”

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Answer: We rephrased this paragraph according to the reviewer’s recommendations.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 1385, 2011.

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