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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability of aerosol, gaseous pollutants and meteorological characteristics associated with continental, urban and marine air masses at the SW Atlantic coast of Iberia" *by* J.-M. Diesch et al.

Anonymous Referee #1

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Review of the paper entitled "Variability of aerosol, gaseous pollutants and meteorological characteristics associated with continental, urban and marine air masses at the SW Atlantic coast of Iberia" by Diesch et al., ACPD.

This work presents simultaneous aerosol and gas phase pollutant measurements at the station of "El Arenosillo", conducted during a campaign within the DOMINO project. Its main added value comes from the fact that it gathers and combines a large ensemble of high resolution measurements, both of physical and chemical characteristics of aerosols as well as of gaseous species. Disadvantages rise from the very short du-

ration of the campaign, which cannot probably be representative for the intensity and specific features of pollution transport over the area throughout the year, and the accuracy of the methodology followed to distinguish the different transport patterns. Despite these constrains, to my opinion, this work merits publication to ACP, after strengthening parts of the work that relate to air mass classification. Please find below suggestions for improvement and corrections that should be implemented in the text prior to potential final acceptance.

1) Abstract, pg 31587, ln 6-8: These lines referring to ozone variability are too generic and are possibly valid everywhere in the globe ... so I suggest removing it from the abstract.

2) Pg 31587, ln 18-21: The significance of African dust transport during the summer months is true for the Western Mediterranean Basin and not for the whole South Europe. For instance, in the Eastern part dust transport is encountered mainly during spring. See Moulin et al., JGR, VOL. 103, NO. D11, PAGES 13,137âĂŤ13,144, 1998.

3) Pg 31589, ln5-7: "Our contribution to ... parameters simultaneously" This sentence needs restructure from the grammatical point of view.

4) Section 2.2: At this particular part of the manuscript my main concerns are born. In particular, even though back trajectories are surely one step forward compared to simple wind direction classification for medium to long range transport, it is yet over-appreciated by the authors concerning its validity to distinguish air masses from closer distances. The limitations of this methodology should be clearly stated and at certain points could be empowered by additional proofs. Please consider the following relevant points:

âĂć The authors should go through the HYSPLIT site and relevant publications (e.g. Draxler and Rolph, 2003) and document the models limitations. To my knowledge trajectories below 100 m suffer from high uncertainties. For the current study I would suggest the choice of one back trajectory inside the boundary layer- BL - (e.g. 500

m) and one outside the BL (e.g. 1500 m). That would additionally enable authors to support their air mass classification regarding inside BL transport, possibly from close sources, and free tropospheric transport. Coincidence of the two trajectories would increase the confidence regarding the sector that air masses originate from.

âĂć What is for sure is that back trajectories cannot identify air masses coming from a narrow domain like Huelva, since their spatial uncertainty exceeds by far the extend of the city. In this case, trajectories can give just a first indication that should be additionally certified in two ways: one is the wind direction from the meteo station provided that there are no physical blocks in between El Arenossillo and Huelva (distance 20km), and the other is for these cases possibly influenced by Huelva pollution, to run forward trajectories with Huelva as starting point and at various altitudes (mostly inside the BL) and check whether indeed El Arenosillo is among the receptor points. In all cases combination of methods and statistical support would increase the level of confidence concerning air mass origins.

âĂć Another issue is how authors have attributed back trajectories to a certain angle direction in order to compare it with local wind direction fields. That should be clarified since good agreement between the two methods actually strengthens their conclusions.

âĂć In Fig. 4 and on 28/11/08 there seems to be an interesting case of an event during which all parameters peak. This event is not classified into any of the existing classes. Is it a case that falls between other classes and the authors cannot distinguish? Is it a case of stagnant conditions? In the first case an attempt to classify it a posteriori based on its "pollution" characteristics would be interesting, while in the latter case the addition of the "stagnant conditions" class would probably be appropriate.

5) Pg 31600, ln 13: Some of the CPC error bars are missing in Fig 6 thus the discussion cannot be easily followed. In lines 18-20, I do not understand the argument why error bars are not presented in the graph.

6) Pg 31600, ln 20-23: The comparison between the different classes is not clear.

7) Pg 31601, ln 16-19, and caption in Fig 7: The authors present size distributions from two different instruments based, as they also mention, on different techniques, thus providing results that are not comparable both by means of diameters but also on the absolute amplitude of the observed aerosol modes. The discrepancies in the overlapped area is not due to the fact that the instruments reach their limits, as they say in Fig. 7 caption, but due to this correction which also affects the measured quantity since there is a change in the integration interval of DN/Dlog(Dp). I would suggest they use their full chemical data set and current relevant literature to infer on the optical properties of aerosols in that range, proceed to diameter type homogenization, based on well documented assumptions if necessary, and then readjust OPC data to FMPS, in order to provide a continuous reliable distribution. Another option would be to discuss each mode separately and not mix between number, surface and volume distributions.

8) Pg 31602, 1st paragraph: In the discussion of number concentrations, I would like to mention that nucleation is also found for the Portugal-Marine case. Additionally, if Huelva is the reason why Portugal-Huelva distribution shows the maximum number concentration at 30 nm then why isn't this also the case for Marine-Huelva? What is the role of pollution from Portugal?

9) Section 3.4.3: Authors base their discussion and interpretation of particulate organics diurnal patterns on the mean diurnal course. Is this for the whole period? Is the pattern the same when different air masses are encountered? The high error bars indicate that during different days much different patterns might be observed.

10) Section 3.5: The whole analysis and discussion on ozone behavior seems to be detached from the rest of the document. Moreover, the analysis depth does not comply with the respective analysis for aerosols, suffering in many point from generalizations and lack of interpretation depth. I do not see how this section can add something to the paper and I suggest it is removed. In all cases, the paper is way too long to follow

undistracted.

11) Section 3.6: What is here meant by "relative" standard deviation"? Is the fact that inner-category variability is larger than the inter-category an admission of biased classification methodology? I wonder whether these two standard deviations are comparable, since the conclusions potentially drawn could have severe effect on the validity of the presented results.

12) I would strongly urge the authors to shorten Section 4 by maintaining only major findings, possibly better under discrete bullets.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31585, 2011.