

Interactive
Comment

Interactive comment on “Meteorology during the DOMINO campaign and its connection with trace gases and aerosols” by J. A. Adame et al.

Anonymous Referee #1

Received and published: 16 September 2013

General comments This work by Adame et al., presents the weather conditions, air-mass circulation, and observed behaviors of a number of atmospheric compounds (O₃, NO, NO₂, SO₂, particle number, benzene, isoprene), chemical properties (OH reactivity) and aerosol chemistry during the DOMINO campaign which took place from 21 November to 8 December 2008 at the El Arenosillo observatory (South- West of Spain).

The paper is well written, even if some Section appears redundant to me (ex. Section 2.2). However, my principal concern about this paper is the lacking of a clear/strong scientific goal. Moreover, I did not think that substantial conclusions have been reached.

The authors stated that the goal of the paper is to “investigate weather conditions dur-

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ing the DOMINO campaigns, emission sources and land uses, to identify air masses of different origins and to analyses trace gases and particle in the air-masses, in the south-west of the Iberian peninsula during late-autumn". Even if of some importance in the framework of the DOMINO campaign data interpretation, I do not think that the analysis presented in this work represents a serious advance in the scientific knowledge, as requested by ACP for publication. In particular, also considering the relatively short time period of the campaign as well as the fact that, as stated by the authors, the investigated period was characterized by anomalous atmospheric circulation in respect to the previous 14 yr which partially prevents a generalization of the results.

Moreover, a number of paper describing the DOMINO results as well as the long-time measurements at El Arenosillo were already published, and similar analysis presented in this work by Adame et al., were already presented (e.g. Diesch et al., 2012; van Stratum et al., 2012; Sinha et al., 2012; Sorribast et al., 2013; Notario et al., 2013; Adame et al., 2010). So what's new with this paper? This should appear more clearly before the paper can be accepted for publication.

The data set of the DOMINO campaign is certainly of high value (as demonstrated by the publications already present in the special issue). So my suggestion for the authors is to find an original/novel topic and try to better focus the paper on that (maybe the contribution from ship emissions can represent an interesting point to be better exploited? Or the relative contribution of mesoscale dynamic vs synoptic-scale circulation?).

Below my specific comments are reported.

Specific comments Section 2.2 I do not think that this detailed description of the emission data-base is really necessary. This also considering that the list of number here presented were not specifically used/cited in the following of the paper. . . Moreover, it looks to me that some large differences existed between the two cited data-bases (EPER vs CMAJA), e.g. SO₂ for industry at Huelva. You can simply indicate what are the major emission sectors for each regions as a function of atmospheric compound.

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Suggestion for the paper: can your data be used to verify the goodness of these emission inventories (maybe also using long-term measurements)?

Section 2.3 A table reporting all the available measurements with details on instrumentation and location can be useful for the reader! Can you provide a combined uncertainty for O₃ measurements?

Section 3.1.1 Which was the advantage of using ERA interim fields? Are the obtained results significantly different from that calculated using the 1deg X 1 deg meteorological files? How about representation of topography? In naming the air-mass circulation classes you completely forgive the “long-range” origin of air-masses. Thus, only the local/regional emissions are important for El Arenosillo atmospheric composition?

Section 3.3 The general implication of the observed variability (especially for OH reactivity) should be better addressed here or in the Discussion section! Here, I have serious concerns about the robustness of the depicted diurnal cycles. Looking at the Table 2, each mean cycle is resulting from averaging a maximum of about 4 days (92 hours for Huelva-Portugal air-masses) to a minimum about 1.5 days (35 hours for Marine air-masses). This is evident looking at the plots, where very noisy behaviors can be seen (especially for NO₂, SO₂, VOCs). I suggest to the authors to simply calculate night-time/day-time average values (with a proper interval of confidence) and put them in a Table for the different parameters and air-masses and then discuss it. Other way can be to discuss representative case studies. . .

Row 11, pag 19258: “no significant photochemical production in more polluted air”. I do not think that photochemical production can be neglected in explaining the O₃ diurnal variability (especially for Seville air-masses. . .), see also Diesch et al., 2012.

Row 13, pag 19262: Do you have a very small number of filter samplings. How about their representativeness? I do not think that you can use these info for generalizing, but just as “ancillary” information to reinforce the discussion. . .

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4. Discussion In general this section relies too strongly on the description of the diurnal cycles depicted by Fig. 8 for which I have serious doubts about robustness. E.g.: row 18, pag 19266: “from this time to midnight daily maxima of No2, benzene, and isoprene appears”: actually they are just single peaks (probably related to specific events).

Row 23, pag 19263: “transport for Huelva.in less than 3 h”. How did you calculate the transport time?

Row 26, pag 19267: Actually the OH reactivity and NO2 decreases appeared to be related with a decrease of wind speed (not an increase), at least looking at Fig. 8 and Fig. 6.

Row 20, pag 19268: you should see more in details to the actual weather/transport conditions which promoted these NOx and O3 peaks for trying an attribution.

Row 24, pag 19628: please, consider that you have only one filter for this air-mass class: avoid generalization!

Section 4.4 Sinha et al., reported significant SO2 mixing ratios in marine air-masses (see their Fig. 4). Please can you comment this discrepancy with your results? Significant difference appears also comparing NO2 dirunal cycle (Fig. 5 by Sinha et al.).

Row 21, pag 19269: I think it is important to consider air-mass back-trajectories longer than 48 hours, especially for ozone variability.

Conclusions Row: 25, pag 19270: “long-range transport. ...on local chemistry”. You should try to better quantify this contribution.

Row 1. Pag 19271: I do not think that these conclusions are really substantial!

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 19237, 2013.

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