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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Presenting SAPUSS: solving aerosol problem by using synergistic strategies at Barcelona, Spain" *by* M. Dall'Osto et al.

Anonymous Referee #1

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General Comments:

The paper by Dall'Osto et al presents an introduction to the SAPUSS project. The paper discusses the aims/objectives of the project, and presents logistical and instrumentation issues, meteorological conditions, time series of certain measured quantities, and results from a model simulation. While a huge amount of data is presented in this paper, very limited analysis/interpretation is undertaken by the authors. The met overview is shallow, instrument inter-comparisons are incomplete, case studies are not fully investigated, overall conditions/concentrations are not put into context, and unsupported claims of findings are presented.

One only has to read the abstract and conclusions to realise that there are no results of importance to report in this paper. The authors do acknowledge this fact early on, but state this study is so unique that it deserves an introductory paper. I strongly disagree, and while this paper is useful to the SAPUSS community, it is of little use to the wider scientific community due to the lack of scientific analysis/interpretation.

The authors touch on many different topics, but in a manner so superficial it leaves the reader frustrated and un-informed. The comments below (which i have still included for the authors benefit) may be being addressed in detail by other manuscripts and therefore not within the scope of this article, but if so it begs the question: What is the overall message of this manuscript?

Therefore I recommend this paper is NOT taken forward to full publication. I do however look forward to the wealth of information which is sure to be generated from the more focused manuscripts found within the SAPUSS special issue. The information in this paper is not relied on heavily by the other papers in the special issue (based on a brief scan of the other papers), so its exclusion should have minimum impact.

Specific Comments:

In the special issue introduction, it is stated that "Compared to other European regions, the metropolitan area of Barcelona sees relatively high particulate matter due to high anthropogenic emissions, a dry and warm Mediterranean climate and low dispersive conditions due to a unique topographical situation." Given the introductory nature of this paper, it would have been useful for the authors to demonstrate/introduce this, possibly by comparing air quality monitoring datasets between cities etc.

In general, the paper is poorly written (with the exception of the modelling section). Countless technical and general statements/descriptions are inaccurate (e.g. "..... some atmospheric parameters of interest are derived indirectly by the changes in atmospheric radiation that result from the presence of the parameter" is highly suggestive of passive remote sensing, but LIDAR is used as a subsequent example; Barcelona

is stated as having the highest population density in Europe, despite it actually being Paris). Poor grammar is widespread and sentence structure is frequently confusing.

Figure clarity is very poor: axis labels/values are difficult to read; too much data is shown to be useful (e.g. 4 weeks of data at high time resolution on small figures such as Fig. 2a); inappropriate figure legend size/location (e.g. Figure 9). Figures 1a and b are repetitive and unclear. A plan view of topographical data would be more appropriate. Sigma is not defined as standard deviation in Figure 4. Variability/uncertainty values in Tables 4-5 are not defined (standard deviation?).

Values of measured parameters are quoted inline in the text and in various tables. It is not stated in the text what these represent (presumably standard deviation), and values of e.g. 8.8+-12μg m-3 (l2, p18766) are both non-physical and misleading, and should be quoted correctly. Discussing PDFs of parameters which exhibit such skewed distributions is more appropriate than simply means and standard deviations.

Incorrect symbols are occasionally used (e.g. l20, p18764).

Generally speaking, gas phase concentrations are reported in μg/m-3 in this paper. They are later reported in ppb. While different communities do use different units (air quality, atmospheric chemistry etc), it would make sense to use only one set of units in the paper. Also, the quoted conversion between ppb and μg/m-3 for ozone varies (albeit weakly) as a function of T and P, not a constant as implied in l5, p18770. Conversion factors for NO, NO2 etc have been omitted, and should have been presented to allow the reader to quickly convert.

No discussion of the gas phase concentrations reported relative to e.g. exposure limits, other European urban/background sites etc, takes place.

The overview of the large scale meteorological situation is poor. How was classification of the back trajectories performed? Was an automated clustering technique used, or was it a subjective decision? What was the variability of the (a) local (<24hours) and

(b) more distant (>24hours) sections of the trajectory within each individual group? It would have been interesting to examine MSLP and 500hPa geo-potential heights to identify the features responsibly for the flow regimes (particularly for the interesting case of strong easterly flow from Europe).

Local flow phenomena are discussed in the introduction, but are rarely related to any trends observed in the data (occasionally discussion of the observed of the sea breeze is found). The presentation of the met data in Fig 4 shows standard deviations for RH, T etc for each site as a whole. To make this more useful the standard deviations should be reported for each trajectory group. Also, an estimate of precipitation would be useful due to its impact on the aerosol population.

Detailed descriptions of the measurement sites and instrumentation are provided. However, important information is missing, such as inlet flow rates/residence times, humidity conditioning etc.

A description/plot of the comparison of all the ToF-AMS instruments with each other and the off line filters would be useful in such an introduction paper, as would discussion of the collection efficiencies used for these instruments. Was this collection efficiency constant with time/instrument? Was the comparison between involatile (NH42SO4) and semi-volatile (e.g. NH4NO3) components for online and offline techniques favourable?

An average particle size distribution for the entire 1 month study is presented, and lognormal modes are fit to this distribution. Is this appropriate and are there really ever 3 modes <1μm at any one time?

No discussion of whether the conditions encountered during SAPUSS were typical for the time of year etc.

Virtually no analysis on the evolution of the aerosol composition and size distribution is presented. It is hard to criticise analysis when virtually none has not been conducted.