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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Variability of levels of PM, black carbon and particle number concentration in selected European cities" *by* C. Reche et al.

Anonymous Referee #2

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This MS contains a wealth of information and should be published. I am not sure, however, whether all the lengthy discussions with numerous repetitions are really necessary. They might actually make the MS less accessible to readers and detract from the real value of the investigation.

The real message of the MS, i. e. that BC rather than total particle number concentration should be measured in addition to PM10 as an indicator for combustion aerosols is already given more prominence after the first revision, as well as the fact that aerosol dynamics in southern Europe and Central and Northern Europe are different and that there is a difference between coastal (or near coastal) sites and inland sites.

Apart from the comments by Reviewer 1, I would suggest several other modifications.

The descriptions of the sites should also include typical meteorological features such as insolation, daily patterns of mixing heights and, where applicable, land/sea-breeze systems (including their effects on the aerosol at the measurement sites). This would collect all necessary information for the interpretation of the diurnal variabilities in one spot and would obviate the need for the frequent repetitions in all the sections dealing with one or another variable.

I am not quite comfortable with the frequent strong statements about nucleation of new particles (or the absence thereof). Without SMPS measurements showing nucleation bursts one cannot really say whether an increase in N concentration is really due to freshly nucleated (secondary) particles or to the advection of a plume, etc. etc. If indicators for photochemistry increase concurrently with N concentrations of course this is an indication of particle formation, but in the presence of high N concentrations new particle formation should be at least partly suppressed by condensation onto pre-existing particles? In the absence of SPMS data, these strong statements should be qualified.

Is there a way to quantitatively estimate the effect of the different lower cut sizes of the CPCs? At least give an estimate of the magnitude of this effect, which in turn influences S1 and BC/N ratios, etc.

BC concentrations are compared, although they were measured with different instruments. How well are the instruments really comparable? Only a single conversion factor (based on Sunset Analyzer data) is used for each of the sites, which will not be correct for seasonal differences in light absorbing carbonaceous aerosols (e.g. wood smoke in winter time). This neglect of seasonal differences should at least be discussed – seasonal trends might turn out to be different.

As Reviewer 1 noted (twice), the interpretation of the N concentration patterns in MR was quite debatable. Actually, the trend of increasing N from Monday to Friday may

well disappear if not only the average values are shown, but also the variability of these values. It is highly unlikely that number concentrations show an accumulation effect from day to day – PM2.5 mass might be another matter.

Regarding the response to Reviewer 1's comment on the 150 m spatial distance "causing a time shift of hours": can the "strong local source" be identified? If there is such a source, it should show up in the data intermittently depending on meteorological conditions

Minor points: In the "old" section 2.8, there is no separation between the text on SCO and the description of the instrumentation employed at the different sites – there should be a clear heading "Instrumentation".

Please give S content of gasoline and diesel fuel to substantiate the statement that SO2 levels are related to traffic emissions.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 8665, 2011.