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# *Interactive comment on* "One-year record of organic and elemental carbon in fine particles in downtown Beijing and Shanghai" *by* F. Yang et al.

**Anonymous Referee #1**

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Review acpd/2005-5-217

The paper presents data for a whole year of weekly meaurements of the carbonaceous fraction of Beijing and Shangai aerosols (˜ year 1999 to 2000). The data are interesting by themselves as carbon measurements are still scarce for China megacities. However in the present state of the paper, the reviewer do not see actual comparison, and striking features which could differentiate and justify the two studies. Generally speaking the paper appears to be too quicky written and the data might have been analyzed more deeply. It is recommended to make substantial additions to the manuscript.

A very weak point of the work is firstly the experimental section which is not acceptable





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as it does not present sufficient cautions or details especially for an analysis (carbon aerosol split into EC and OC fractions) which is known to to present several artefacts. 1) there are no indication for total volume filtered on average, nor for carbon deposit (expressed in $\mu$gC/cm2). This last point is highly important to bring (or not ) confidence into the TOR method results, as this analysis relies on an optical correction which has limitations for filter carbon loadings. 2) The problem of carbonates is completely ignored. Actually, carbonates may form a severe artefact especially at spring time during the dust season (thus artificially enhancing the apparent EC fraction). 3) It is assumed abruptly (p221, 28) that "it is recommended to analyze only the front filter" an assessment which is not universally followed , by far. 4) It is quoted p222, 12, that mild local wind in summer provides conditions for slight weekly variations. This is not clear ; an may be in contradiction with line 22 . 5) Temperature data should be provided at least monthly means and compared to other year data (223, 21). 6) page 226/17: suddenly, authors use potassium data. There is no indication at all how they obtain these data: what kind of filter they used, are they from colocated samplingsĚ? How was potassium analyzed? It is not clear how they calculate Kexcess. I presume they use iron (Fe) data but from what data setĚ.? There are several different dust sources impacting Beijing (Wang et al., 2004) so the treatment from iron data appears a delicate operation . 7) Same with aerosol C-14 data: nothing is said about filtration, sample treatment (what about the removal of carbonates?Ě.) nothing is said about the amount of carbon necessary for the analysis (classical analysis or AMS?) 8) The Beijing (Chegongzhuang) sampling site is (I believe) situated among a small inner square with trees and vegetation very close to the sampling container. How do the authors feel there samplings are protected against biological debris in summer?

As said earlier, discussion of the EC and OC data set could have given deeper insights into the determinants of atmospheric concentrations. 1) p 224 p226Ě authors either repeat what may be simply seen in table 1 (a comparison with other cities, which aerosol was analyzed by the same mthod) or they argue with comparisons with remote sites which have nothing to do with urban situations. Almost 10 to 15 lines could be avoided.

2) p 223 and followings: although I follow the authors when they refer to different combustion sources (coal, trafficĚ) and different ways of using a given fuel (eg: domestic honeycomb or industrial use of coal), it is frustrating to see that they actually never use the collected quantitative indications (EC/OC ratio) to sustain argumentation for seasonal variability or inter-city differences. So the presented data are not exploited as deep as they could have been. 3) The calculation of SOC estimation is questionable. Firstly, as apologized by the authors data are given for weekly samples, which might not be adequate to trap primary aerosols. Secondly, it is not acceptable to presentnot a single value for primary aerosols as the source mixture displays seasonal variation (more coal in winter). I suggest that at least, authors extract two values : one off the heating season and one inside the heating season. May be a third one should be adequate if the authors have a strong feeling that spring is significantly impacted by biomass burning. Authors should give “a priori” values or trends (from literature data) for EC/OC values of primary aerosols.

Organisation and paper clarity: 1) abstract: the time period (1999-2000) should be indicated. The last third of the abstract is much too vague: we need evaluation numbers for SOC contribution, modern carbon contributionĚ. “signficant” “important”Ě is not a scientific indication!!! 2) 230,10 : typos 3) table 2: give indication on sampling duration dans volume 4) Figure 1: not very lisible especially the X axis has to be simplified and clarified I suggest that the 2 figures show the same dates (from 20/03 to 08/06) in order to be able to compare the 2 cities season by season. Exotic OC/EC points might be discussed if not outlyiers. If not figure 1 is not necessary as the following figure (figure 2) display seasonal mean values. 5) figure 2: it could be interesting to get the seasonal values for OC/EC. Figure quality to be improved 6) Figure 3: might be deleted and more interestingly replaced by a Table with both r2 (not r) and slope value (OC/EC)

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