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Interactive Comment

Interactive comment on "Correlation between cloud condensation nuclei concentration and aerosol optical thickness in remote and polluted regions" *by* M. O. Andreae

Anonymous Referee #1

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Recommendation: Accept with major changes

My (weak) recommendation for (eventual) acceptance is only based on the fact that the paper compiles a large data base of aerosol data that someone might find useful. My major concern is that in its current form, the work will be seriously misused. The author should carefully express the many caveats. I urge the author to look at a much more interesting aspect of the in-situ data, i.e. at the relationship between CCN and aerosol extinction (i.e., "local optical depth") measured at the same point. Such a study will be of far more use to the community, and to climate models.

The manuscript summarizes a large sample of AOT and CCN measurements from various parts of the world and classifies them by location, and clean vs. smoky. A regression fit then shows that there is "a strong empirical correlation" between these parameters.

Major comments: Presumably the goal of such a study would be to connect AOT to CCN and eventually connect CCN to cloud microphysical characteristics such as drop concentration or effective radius. Although there is a good correlation between AOT and CCN over large enough spatial/temporal scales, the use of an empirical equation such as that in Figure 1 completely misses the mark because clouds are localized, phenomena, driven by convective scale processes. Column conditions, averaged over many data sets, do not drive cloud-scale processes.

1) The study ignores a list of processes that make application of such an equation dangerous in the context of indirect effect studies. A few of these are mentioned here:

(i) A column measurement like AOT does not represent the aerosols affecting the cloud, particularly, but not only, in conditions of mid-tropospheric transport; the troposphere is frequently poorly mixed and aerosols have a distinct scale height.

(ii) Measurement of AOT is affected by clouds because skies are rarely clear;

(iii) RH affects AOT and introduces variability. (Kapustin's work is referred to but the lessons learned by that study are ignored.)

(iv) $N=CS^k$ is a poor approximation to CCN spectra; activation spectra exhibit curvature on log-log plots.

None of these issues is discussed.

The danger is that those unfamiliar with these issues will use the empirical relationship and produce results that are only tenuously linked to atmospheric processes like aerosol indirect effects.

2) It is claimed that empirical equations of this kind will be useful in climate modeling

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studies. Climate models at least have the benefit of a height-resolved aerosol and don't have to rely on column-integrated quantities. Of what use will a relationship like this be to a climate model? Why not explore the relationship between light extinction and CCN concentration? This would be analogous to the work by Hegg and Kaufman (JGR 1998) which looked at the relationship between number concentration and volume of an aerosol population. The author should explore this avenue even if the size of the data set is smaller than what is currently used.

Other comments:

1) The font size in the Tables is so small that they are basically illegible.

2) There is no mention of the published relationships between AOT and surface PM2.5 which is very similar in concept and has similar problems.

3) What is the mean and standard deviation of the distance between AOT and CCN measurements? How well synchronized are they in time?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 11293, 2008.