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

Anonymous Referee #2

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This manuscript presents a nice set of data that shows the correlation between observed AOT and boundary layer CCN concentrations. The combined data set and analysis are unique for the peer-reviewed literature, and as such makes a significant contribution.

I have two major comments which must be addressed as a condition of acceptance.

1) There are numerous places in the abstract, Section 3, and Section 4, where the author asserts that some of the difference between remote land and remote ocean CCN concentrations must be anthropogenic. This is plausible, at best, but I am confused why it is asserted so prominently in the abstract and conclusions. The statement is not

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supported in a quantitative way. The only support for the statement is the observation that pollution events occur more frequently in remote land observations than in remote marine observations. This is not sufficient to quantify at all whether the land-marine CCN difference in remote regions is anthropogenic, natural, or some meaningful combination between the two. If there is a more quantitative way of demonstrating this conclusion, it needs to be shown in the paper rather explicitly. At present, the assumption is stated as a conclusion, and should be eliminated from the abstract and conclusions and softened in the text.

2) The penultimate sentence of the paper needs to be eliminated: "...the radiative and microphysical effects of aerosols on clouds, and therefore on climate and precipitation, are correlated and cannot vary independently of one another, at least not on larger scales." Again, this is totally unsupported by the data. The correlation shown between AOT and CCN statistics is regional and statistical, and is hardly a general constraint. Furthermore, the implication that average CCN concentration constitutes the primary forcing parameter in aerosol-cloud-climate interactions is incredibly naive. The author seems to be unaware of black carbon effects on precipitation and clouds, the importance of IN on precipitation, dynamical feedbacks that vary in sign from one cloud type to another, etc...

This otherwise excellent contribution should be accepted only when the interpretation fits the analysis.

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