

Interactive comment on “Investigation of aerosol indirect effects on monsoon clouds using ground-based measurements over a high-altitude site in Western Ghats” by V. Anil Kumar et al.

Anonymous Referee #2

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This paper describes a series of ground based measurements made from a hill top site in south western India during the summer monsoon season when the site was subjected to cloud for extended periods. The authors use both aerosol and cloud data to investigate the relationship between the aerosol indirect effect derived from cloud droplet number and also from effective radius and the effective dispersion. The authors report relationships and compare and contrast these with other previous publications. In addition, the authors go on to conclude that there is a significant difference between AIE derived from CDNC and Reff and this difference can be explained by the need to account for dispersion in the derivation of the former. My main concerns with the result are that there is no evidence presented to show the statistical robustness of

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the correlations used to determine AIE and their susceptibility to biases, particularly by incorporating data at low CCN concentrations which appear fewer in number. In addition, there is no discussion of how the aerosol data were sampled (see detailed comments below). I would like to see these aspects of the papers developed before recommending publication in ACP.

Comments: I am very surprised that only 20 hours of data are selected for this analysis if, as the authors say, the site is covered by warm continental clouds most of the time during the summer monsoon season. It would be very useful to have statistics on the cloud frequency during the whole sample period and the method by which cloud events were screened for removal of precipitation. Without this it is impossible to gauge whether bias has been introduced into the sampling through data selection.

Page 2, line 10-13 and elsewhere: Given the paper is about the differences in AIE based on different methods of representing it the authors need to say how AIE was derived in each of the past work they cite.

Page 3: the aerosol instrumentation is not described, how were the number concentration measurements as a function of size between 5 nm and 30 μm measured? How were the larger particles ($>1\mu\text{m}$) sampled, was an inlet used and if so what was its transmission? If the large aerosols were measured at ambient humidities how were large aerosol separated from cloud droplets or were these counted as part of the same? Are the aerosol measured at the hill top in cloud, or below the cloud base? If the former, how are the interstitial aerosol collected, if the latter, how is flow connectivity established? This section needs a much more thorough description.

Figure 1a shows the relationship between CDNC and the total aerosol number and CCN. There is considerable curvature for low CCN and high aerosol load. This comes as little surprise since the aerosol concentration includes particle sizes from 5 nm upwards, which at times dominate the aerosol number concentrations but play no role in cloud activation. I fail to understand why figure 1a and 1b are plotted in the way

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they are since the only comment in the paper is about the relationship between cloud droplet number and effective diameter which cannot be discerned from a single figure and instead the confusing use of total aerosol is included. It would be better to plot CDNC against Reff and colour the plot by CCN which would, I think, be far clearer to interpret.

Page 6, lines 1-3: Given the argument made here I cant help thinking that figure 2 would be better presented as a plot of ED (Reff) versus LWC and coloured by CDNC. That is the way the argument is made at least.

Page 6, lines 8 and 13: A statement is made that the correlations between CDNC and CCN are statistically significant, no such statement is made for the relationship between Reff and CCN, is the latter also statistically significant? I would like to see the method by which the significance of these relationships are tested statistically as by eye both figures 3a and 3b appear to have rather a low correlation, particularly figure 3b and I would take a little more convincing before I am persuaded that they are robust especially as the plots are on log-log axes. Given that the whole premise of the paper rests on the AIE being 30-40% different when derived from Nc than when derived from Reff there has to be a full uncertainty estimation of the slope I am particularly concerned that the plateau and tail off in Reff at low CCN concentrations greatly skews the slope of the fit. If data points below $CCN=1000\text{ cm}^{-3}$ are excluded from fig 3b it would not surprise me if slopes of around -0.07 or steeper were derived which would be very close to the estimates determined by the CDNC data. A much more complete evaluation of the statistical robustness of the data and the possible biases at the ends of the data set is needed before I am willing to believe the differences the authors purport to show and conclude in lines 2026 of page 6. Given this LWC is at the maximum in the frequency distribution (figure 4) and shows the maximum difference between AIE_n and AIEs then I suggest that a similar analysis is carried out for the different LWC bins to establish which parts, if any, of the distribution in figure 5 are robust statistically and may or may not be subject to the low CCN concentration biases shown in figure 3.

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Page 8 lines 11-14: It is important to report here how the previous authors calculated the AIE from data as they are reporting a difference between AIEn and AIEs.

Minor Comments: Page 2, line 23: It would be good to be clear about the regional location and not just name the field site at this point. Page 3 line 8: A number of parameters are introduced without definition. For example CDNC here but earlier AIEn and AIEs

Page 3, lines 9-10: How were the non-rainy conditions defined?

Page 3, , lines 22-24: The CDP does not measure effective diameter, it measures cloud droplet number as a function of size, the other parameters are derived. The authors should be clear about how this was done.

Page 3, lines 29-30: It would be good to see a mathematical definition of how the spectral width of the droplet size distribution and the relative dispersion were calculated.

Page 5, lines 4-5: I do not believe the article show state that it “demonstrates” anything in a methods section. It is best to state what the paper seeks to achieve at this point in the text.

Page 5, lines 15-16: is the term b (beta) a percentage and is it an offset or an enhancement in the Twomey effect? Be clearer in the definition.

Minor Corrections: Page 1, Line 29: Twomey 1974

Page 2, line 1: “. . .but the field studies of the indirect aerosol effect shows. . .” should be show

Page 2, line 3: define epsilon, I realise this is defined on page 5 but it needs to be introduced as the relative dispersion at this point.

Page 2, line 6, “. . .cloud parcel(s) woith droplet(s) of the same. . .”

Page 2, line 8: show not shows

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Page 2, line 9: "...and a slight decrease.."

Page 2, line 10: relationship(s)

Page 2, line 14: "...indicated that (the) dispersion effect..." Introduction: Throughout the introduction the authors mix up how they refer to their citations. At times this is done by reference to the work as a paper, eg (The paper by) Smith and Jones shows..., and at times by reference to the authors "...whereas Smith and Jones argue that. This needs to be consistent.

Page 2, line 20: "...decrease(s) the spectral width and in turn enhance(s)..."

Page 2, line 27: "(The recently set up..."

Page 2, line 29: "...situated in (the) Western Ghats..."

Page 2, line 31: "...during (the) summer monsoon..."

Page 3, line 1-2: "Interestingly, observations from the laboratory have shown that..."

Page 3, line 3-4: "...The aerosol and CCN concentration (measurements) shows that the region (experiences) higher aerosol concentration(s) during monsoon season..."

Page 3, line 17: "...in which (a) super saturated water vapour..."

Page 3, lines 15-20: include a reference to the CCN, typically Roberts and Nenes or similar.

Page 3, line 23-25: "...which is a combination of Cloud Droplet probe (CDP) [and a hotwire probe. The CDP measures the] cloud droplet size distribution and concentration from 3 to 50 μm , categorized 25 into one of 30 channels."

Page 3: define DSD

Page 4, line 19 and equation 3: It may be best to use n rather than N for the number of bins to clearly differentiate with N_c .

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Page 4, line 19: “particle count”

Page 4, line 20: “. . .use (a) 1/3 power law..”

Page 4: in equation 4, the liquid water content is given as L yet elsewhere it is defined as LWC, it needs to be consistent.

Page 5, line 2: “. . .which is a function of (the) spectral shape of (the) cloud droplet size distribution..”

Page 5, line 3: “. . .for estimating AIE”

Page 5, line 4: “. . .this study (is) uniquely different. . .”

Page 5 line 6: “. . .to (a) Gamma distribution..”

Page 5, line 10: “. . .as the ratio of (the) standard. . .”

Page 5, line 13: “. . .to explain (the) dispersion effect.”

Page 5, lines 15-16: “defined as the percentage of (the) offset/enhancement (in the) Twomey cooling effect due to the dispersion in the cloud droplet size distribution.”

Page 5, equation (6): define alpha_beta

Page 5, line 21: why introduce ED without defining it as effective diameter except in figure 1 when you have already defined effective radius. I suggest redrawing figure 1 and also figure 5.

Page 6, line 12: “The linear fit to (the) log-log plot. . .”

Page 6, line 27: but the AIEs have already been estimated?

Page 7 line 11: “may cause (a) large number of”

Page 7, line 12: “and reduces the”

Page 7, line 17: “cloud albedo thus tend(ing) to reduce the AIE”

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Page 8, line 4: “..spectra, has been calculated from CDP data. . .”

Page 8, lines 17-23: Why introduce DE as a term at this stage. Remove it.

Page 15: formatting of the figure caption needs correcting

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