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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Suppression of warm rain by aerosols in rain-shadow areas of India" *by* M. Konwar et al.

Anonymous Referee #1

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In the present work authors have presented aircraft observations of cloud condensation nuclei (CCN), aerosol concentrations, and cloud droplet size distribution conducted as a part of CAIPEEX program during the period of Indian summer pre-monsoon month (May) and during monsoon months (June to September) at different locations and altitudes over the southern part of India. The data set reported could be of potential importance as observational areas are of growing concern due to rapid industrialization and aerosol and CCN data are in sparse, and I congratulate authors for that. In point of fact, the data presented by authors are first of its kind involving aircraft measurements from Indian continental region. Deplorably, however, the manuscript is poorly written, scientific data presented is not achieving adequate standards of ACP, exhibits clear lack of consideration in texting the manuscript, and has some fatal flaws. Although may be of interest to ACP community and readers, this manuscript, at least in its present form

and for reasons mentioned below, should be precluded from publishing in ACP.

My detailed comments for the improvement, if authors wish to re-submit improved version to ACP, are included herewith, which I believe would be helpful for authors as CAIPEEX seems to be long-term project and more measurements may be carried out:

A methodical English editing and revision regarding formulations is necessary throughout the manuscript. I am aware that authors are not native English speakers, but there is still a huge scope for improvement. I have major concern the way manuscript is written. There is no experimental and method section. I believe/think explaining the instruments details in 3 – 4 sentences would be of great help for the readers. There are several other similar measurements from other parts of the world, which authors could wish to refer and expound how they are similar or different from CAIPEEX measurements (Roberts et al., 2010; Shinozuka et al., 2009; and references therein).

Another major and crucial concern, and main root of my criticism, which could raise several questions is about calibration of CCN counter (a Droplet Measurement Tenchologies' Cloud Condensation Nuclei Counter – DMT CCNC) and therefore about the CCN data reported. Did authors calibrate their instrument before each flight? if yes what was the method adopted and what are the uncertainties associated with effective supersaturations or did authors report supersaturion set in the instrument? Effective supersaturation is different from supersaturation set in the instrument (in addition please note that depending upon model/approximation/parameterization used for calibration the relative deviations at high effective supersaturation could be $\sim$ <10% and could be as high as >40% for effective supersaturations less than 0.1%). If authors did not experimentally calibrate their instrument they should at least specify the basis for their calculations and provide error/uncertainty estimates. The supersaturation generated in DMT CCNC is not only governed by temperature difference between the top and bottom of the flow column (ïĄĎT) as mentioned by the authors. In any case authors have not given these ïĄĎT values associated with the corresponding supersaturation measured, let alone the details like absolute temperature, pressure, and flow

of/through CCNC column, which also significantly affect the supersaturation generated in CCN counter (see Rose et al., 2008).

In Fig. 2 caption authors say "The CCN and aerosol concentrations are considered outside the cloud". It is hard to understand, did authors show/discuss/present measurements done outside cloud? If yes then why through out the text "measurement in clouds" has been addressed and if not then please remove this last sentence from the caption. Or did authors only measured cloud drop size distribution (DSDs) in clouds and rest parameters outside cloud?

Major comments are listed below:

1 Abstract

i. Page 17010 L1: Did authors mean cloud and aerosol properties? ii. Page 17010 L5: what is cloud drop condensation nuclei? Did authors mean Cloud Condensation Nuclei? If yes please say so. iii. Page 17010 L7 – L8: Please revise this line as there is no direct evidence from the data you have presented (please see detailed comment below)

2 Introduction

i. Page 17010 L22: Please replace cloud drop condensation nuclei by cloud condensation nuclei ii. Page 17011 L7: ". . .. . .despite being still. . ." What do authors mean by this statement? iii. Page 170111 L8: By what parameter the Fig. 1 is color coded? It is unclear from the text and figure caption. Please mention what Fig. 1 is representing. I assume authors are trying to present the topography of the area, if yes please explicitly mention that in text. iv. Please provide and cite appropriate references as some of the statements are too generalize and related to other similar studies (Roberts et al., 2010; Shinozuka et al., 2009 and other similar)

3 The aircraft measurement

i. What are the measurement locations? please specify them in this section (like

Nasik, Nanded, Raichur, Nalgonda, Anantpur, etc)? Western Ghat is too broad to mention that too it only mentioned in abstract. One of the fatal flaws is that authors keep jumping between measurement locations and dates from one figure to another (kindly see below) ii. Page 17011 L: 20: Authors mentioned CCN measurements were carried out at 0.35 – 04% but on the same page L: 26 authors mentioned it was set to SS cycle of 0.2, 0.4, and 0.6%, what supersaturation exactly the data is being presented??? What was 2 minute time, to shift to new supersaturation or the measurement time??? Please clarify all these details this is too confusing for a reader. iii. Page 17010 L24: if the aerosol size distribution was measured from 100 nm to 3 micron then why it was not shown/plotted in the manuscript? It would be nice contribution in the datasets and interesting to see how the aerosol size distribution looked like? iv. Page 17011 L10: Please provide the appropriate reference for the claim, as it is too general. What was the CCNC flow rate? Did constant pressure regulator was installed at the CCNC inlet? As mentioned above change in pressure with altitude could cause change in supersaturation. Or did authors only used the data of straight and level flights? If you have any other details requested above about CCN counter please add them in this section.

4 Aerosol radiative effects

i. Page 17011 L13: "Total eight cases. . .. . ." your table shows 9 flight details some of which are not at all discussed in the manuscript, please double check and correct the details. ii. Page 17011 L15: All of a sudden CCN concentrations at 0.4% are mentioned as a profile. Did authors keep the supersaturation at 0.4% during entire flight? If yes then why Fig. 2 (a, c, e) caption says CCN at 0.35 – 0.4%. If you kept changing the supersaturations (what ever range please check) please give the separate data for each supersaturation. iii. Again, there is no clear mention about measurement locations. It is very confusing: In Fig. 2 authors show Nanded, Raichur, and Nanded (heavy aerosol loading-21st June and 24th September, and low aerosol loading-22nd June). But in Fig. 4c authors have introduced Nasik or Nashik (and not Nassik as in

caption of Fig. 4c) data taken on 16th August 2009 which is not mentioned anywhere in the manuscript. Please explain the context of Fig. 4c. Same is for Fig. 6 along with Raichur, Nanded, and Nasik now authors have Nalagonda and Anantpur on three new dates and these details are nowhere to be found in manuscript. Please check thoroughly and discuss/present/compare only the data which is commonly available. Even if it is only for two stations that is fine, this will help to avoid confusion while reading and forcing guessing the content while reading. Please add the names of the measurement locations in Tab.1. iv. Page 17012 L22: While discussing Fig. 2c (and one point in Fig. 2 a as well) it is noticed that your CCN concentration is higher than total aerosol concentration. I believe, this seems to be absolutely wrong, as CCN are subsets of aerosol particles. Please double check the Fig. 2. v. Page 17012 L24: "...0.40 to 0.66..." your table shows 0.31 as minimum, which one is correct? Similarly Page 17013 L1: "....0.64 to 1.67....." your Tab. 1 shows minimum as 0.81 and Page 17013 L4: 0.12 is 0.15 in Tab. 1 which are correct? Please double check. vi. Page 17013 L11: "This inhibits the convection" Authors could consider giving NCEP-reanalysis interpretation to support this claim. As it is not clear from what authors have mentioned. vii. Fig 3 is absolutely insignificant, at least unless which point belong to which station is describe. Please provide information about which point belong to which station (same is for Fig. 5; again authors mention 8 flights, but there are 9 points in Fig. 3 & 5; please double check). In addition CAPE below 2000 J/Kg represents moderately unstable atmosphere, hence please provide valid reference for this claim. Just because AOD and AI was different on 21st June and 22nd June (heavy aerosol loading), and on 24th September (low aerosol loading) does not support that there was a warm rain suppression due to high aerosol concentration. Authors are requested to give the total aerosol number concentration measured on these specific dates during flights. Further, do authors have any explanation for Fig 4 (a, b, and d) that there is no significant difference at modal LWC and the tail of droplet diameter is at similar value at all locations under all the conditions. On the other hand during rainfall suppression, if any, the tail of the distribution on 21st June and 24th September would have been on

lower side. However, on average after 6000 meters there is no significant difference in tail value. Please explain. As I could only see the decrease in droplet size but not a clear suppression or rainfall. Also mention the average cloud base height in all measurement. This will help readers to assess which type of cloud authors were talking. Please refer to Tang and Chen, JGR, 2006 for more details about cloud types associated with summer monsoon. Some of the claims made in this section are very inexplicit and need appropriate references.

5 Aerosol microphysical properties

This section needs an "exhaustively complete and confirming exactly to fact" revision as it is really very hard to understand anything from this section. No co-ordination and link, whatsoever, between text and figures while writing the manuscript. i. Page 17014 L: 2: "....presented in (Fig. 4a-e).." where is Fig. 4e, not only that Fig. 4c, as mentioned above, is also out of context. ii. Page 17014 L: 8 "...the mean the maximum CDP....." what do authors mean by the mean the maximum, and what is CDP? Did authors mean cloud droplet concentration, as CDP is cloud droplet probe as mentioned on Page 17012 L: 3. please reformulate accordingly to avoid the confusion. iii. Page 17014 L: 10: "The CCN at 0.4% SS..." It is mentioned here that CCN at 0.4% was found out (did authors mean calculated?) from so-called CCN-SS relationship. What type of relationship? Is there any mathematical relation? Did authors mean it was calculated from classical power law? If yes, then try to compare the range of 'b' values from Tab. 1 with those available in literature. Again it is said at three different places within manuscript (Page 17011 L: 24; Page 17012 L: 15; Page 17014 L: 9) CCN at 0.4% were measured. I can understand that event though it is measured at 0.4%, it could also be calculated. But please specify that in a simple language so that reader can understand that without guessing. iv. Page 17014 L14: Can authors please explain what did they mean by super-continental clouds? v. Page 17014 L: 25: "....of the mean DSDs of the horizontal....." did authors mean vertical penetration? If not then please explain what you mean by horizontal penetration. vi. Page 17014 L:

28: Now this is where all of a sudden Nashik appears out of nothing. Nashik in text and Nassik in Fig. 4c caption. Please correct Nassik to either Nasik or Nashik and stick to one nomenclature, and appropriately explain the corresponding figure in text or please remove it. vii. Page 17015 L: 13: Please note that Lal and Pawar, 2009 have shown high correlation between lightning and rainfall during pre-monsoon. They have explicitly mentioned that during monsoon there is weak or no correlation between lightening and rainfall. They attribute low updrafts during monsoon season due to low cloud base height and low aerosol concentration for low correlations. Moreover they have presented the data analysis from 1998 – 2007. Hence, it is requested that authors should provide some experimental/analytical evidence for their claim about lightening or remove the statement.

6 Summary and conclusions

It is very hard to understand and clearly make out a "take home message" from the conclusions. For example, it is understood that over the Indian region in last few decades rainfall distribution has significantly changed, in spite of average rainfall over region as whole being unchanged (Goswami et al., Science, 2006). But I am wondering what this statement is supporting here, especially when authors do not have sufficient evidence to claim what they have intended for. This is too generalized statement without any evidence. I would rather suggest it to reframe without being too generalized or remove it.

Then there are countless typos in the text including figure captions and it is difficult to list all of them here, hence it is requested that authors should do a conscientious proof reading before re-sending the manuscript; figure legends except for Fig. 1 are hardly readable. I have one last concern; I would assume that rain-shadow areas are anyhow known to receive less rainfall how authors would quantify the suppression, if any, against the actual rainfall.

References:

Goswami, B. N., V. Venugopal, D. Sengupta, M. S. Madhusoodanan and P. K. Xavier: Increasing trend of extreme rain events over India in a warming environment, Science, 314, (5804), 1442-1445, 2006.

Roberts, G. C., D. A. Day, L. M. Russell, E. J. Dunlea, J. L. Jimenez, J. M. Tomlinson, D. R. Collins, Y. Shinozuka and A. D. Clarke: Characterization of particle cloud droplet activity and composition in the free troposphere and the boundary layer during INTEX-B, Atmos. Chem. Phys., 10, (14), 6627-6644, 2010.

Rose, D., S. S. Gunthe, E. Mikhailov, G. P. Frank, U. Dusek, M. O. Andreae and U. Pöschl: Calibration and measurement uncertainties of a continuous-flow cloud condensation nuclei counter (DMT-CCNC): CCN activation of ammonium sulfate and sodium chloride aerosol particles in theory and experiment, Atmospheric Chemistry and Physics, 8, (5), 1153-1179, 2008a.

Shinozuka, Y., A. D. Clarke, P. F. DeCarlo, J. L. Jimenez, E. J. Dunlea, G. C. Roberts, J. M. Tomlinson, D. R. Collins, S. G. Howell, V. N. Kapustin, C. S. McNaughton and J. Zhou: Aerosol optical properties relevant to regional remote sensing of CCN activity and links to their organic mass fraction: airborne observations over Central Mexico and the US West Coast during MILAGRO/INTEX-B, Atmos. Chem. Phys., 9, (18), 6727-6742, 2009.

Tang, X. and B. D. Chen: Cloud types associated with the Asian summer monsoons as determined from MODIS/TERRA measurements and a comparison with surface observations, Geophysical Research Letters, 33, (7), 2006.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 17009, 2010.