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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The authors thank the reviewer for the helpful comments. The paper underwent practically a complete rewrite with much greater attention to its substance, structure and language. Below are the reviewer's comments in *italics*, followed by our responses.

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).*

Response: We agree with the reviewer. When submitted, this paper was supposed to be one of three papers:

The first paper provides overview of CAIPEEX. In that paper the whole experimental design, the aircraft, instruments, flights and area that were covered is described.

The second paper concentrates on the relations between the vertical evolution of cloud drop size distribution and the height for initiation rain.

The third paper (the present manuscript) is applying it to understanding the relations between aerosols, cloud microphysics and precipitation in the rain shadow area.

Unfortunately, the other two papers were not submitted until now, so that the present paper was submitted and remained without the support of the other papers. This has obviously created a situation where much that was assumed to be handled to be taken care by the other papers was not available to the reviewer. We have included in the revised paper the minimum amount of information for being viable while not preempting the other papers.

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 <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 (ïA¸Dˇ T) as mentioned by the authors.

In any case authors have not given these ïA¸Dˇ T 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).

Response: The CCN counter was not experimentally calibrated during the field campaign. Pressure correction was applied with respect to CCN concentration and SS. The super saturation of the CCN counter was set at 0.2, 0.4 and 0.6 %. It takes about 1 min to switch over from one SS to another and another minute for the useful measurements. The inlet pressure of the CCN counter was at ~500 mb, for the actual CCN concentration, a correction factor equivalent to Ambient Pressure/ Inlet pressure was applied to the observed CCN concentrations. CCN concentrations were considered to be valid only after the temperature difference between the top and bottom of the column did not change more than 0.15ºC/sec for more than 10 consecutive seconds, and only after allowing at least 60 seconds for the instrument to equilibrate after each super-saturation change. The actual Super Saturation drops with respect to the ambient pressure by 0.07 % SS per 100 mb change in atmospheric pressure. The changes in SS due to pressure are taken into consideration.

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?

Response: We presented here the measurements of the CCN and aerosol concentrations outside the cloud to investigate the aerosol loading in the atmosphere. The cloud measurements are understandably inside the cloud. The CCN concentrations that were compared with the cloud drop concentrations were conducted by circling below cloud base.

Point to point response to major comments:

1 Abstract

i. *Page 17010 L1: Did authors mean cloud and aerosol properties?*

Response: Yes we meant microphysical cloud and aerosol properties by this sentence. For clarity this sentence is now rewritten.

ii. *Page 17010 L5: what is cloud drop condensation nuclei? Did authors mean Cloud Condensation Nuclei? If yes please say so.*

Response: We referred cloud drop condensation nuclei as CCN. Now it is modified to cloud condensation nuclei as suggested.

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)*

Response: We cannot agree to this comment. We have shown that there was no warm rain formation in the convective clouds over the rain shadow areas. Heavy aerosol loading found to suppress the formation of warm rain. Please see Figure 7 and the response here to the last query in the section **4** where we have demonstrated that there was no LWC contributed by the cloud droplets > 50 μm indicating insignificant coalescence process. The cloud image probe indicates that the cloud droplets transform directly into mixed phase precipitation.

2 Introduction

i. Page 17010 L22: Please replace cloud drop condensation nuclei by cloud condensation nuclei

Response: As suggested cloud drop condensation nuclei is replaced by cloud condensation nuclei.

ii. Page 17011 L7: "despite being still" What do authors mean by this statement?

Response: One of the major objectives of CAIPEEX mission was to study the end result of seeding operation, how much rain enhancement has taken place because of seeding. This issue is still unproven. During the CAIPEEX phase II campaign this issue will be addressed with the help of one instrumented microphysical aircraft, one seeder aircraft and ground based radar observations. This issue will be covered in the overview CAIPEEX paper.

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.

Response: Figure 1 is redrawn, now it provides the project topographic map, the names of the sites where flights were conducted, and the flight tracks with the flight levels. **Please see Figure 2.**

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)

Response: As suggested, in the revised manuscript a short discussion on other similar studies are includes with references.

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)

Response: As suggested the measurement locations are now described in this section and illustrated in Figure 2. We hope that the editor will not regard it as a fatal flow, but rather a correctable one, as we now corrected it.

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.

Response: The CCN counter was not experimentally calibrated during the field campaign. The super saturation of the CCN counter was set at 0.2, 0.4 and 0.6 %, it takes about 1 min to switch over from one SS to another and another minute for the useful measurements. The inlet pressure of the CCN counter was set at 500 mb, to obtain the actual CCN concentration a correction factor equivalent to Ambient Pressure/ 500 mb was applied to the observed CCN concentrations. . CCN concentrations were considered to be valid only after the temperature difference between the top and bottom of the column did not change more than 0.15ºC/sec for more than 10 consecutive seconds, and only after allowing at least 60 seconds for the instrument to equilibrate after each super-saturation change. The actual Super Saturation drops with respect to the ambient pressure by 0.07 % SS per 100 mb change in atmospheric pressure. Pressure correction was applied with respect to CCN concentration and SS.

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?

Response: The PCASP measured size distribution requires further post processing treatment before it is reported elsewhere. Therefore we did not use the aerosol size distribution, but just the cumulative aerosol number concentrations.

iv. Page 17012 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.

Response:

As suggested appropriate reference is given to the sentence in the revised manuscript.

With respect to the CCN, please see our response above to the comment on Page 17011 L: 20.

4 Aerosol radiative effects

i. Page 17012 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.

Response: In the revised manuscript this is corrected, now all the 9 cases are described and presented in the manuscript.

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.

Response: Thank you very much for pointing out the mistake, now it is corrected. The SS was set at 0.2%, 0.4% and 0.6% SS during these observations.

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.*

Response: As suggested, for simplicity we provided in detail only two contrasting cases on 21 and 22 June in the revised manuscript.

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.

Response: It is true that CCN are subset of total aerosols, however, the range of particles sampled by the Passive Cavity Aerosol Spectrometer Probe (PCASP) is limited between 0.1 to 3 μm. CCN aerosols with sizes of 0.05-0.1 micron that are below the detection limit of the PCASP can easily explain the difference.

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.

Response:

AOD should have been 0.12 to 0.81, with average value of 0.48. Thank you for the correction.

AI range is correct i.e. from 0.64 to 1.67.

Page 17013 L4: AOD on 22 June should have been 0.12. Thank you for pointing out the error.

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.

Response: All that we mean here is using references to indicate that when AOD exceeds 0.25 it can inhibit convection. We do not claim that this did actually take place in the study area. The text is corrected accordingly.

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.

Response: As suggested the statement "CAPE below 2000 J/Kg represents moderately unstable atmosphere" is included and referenced in the revised manuscript.

The comparison of AOD to CAPE was eliminated. However, in the revised manuscript we utilized the CAPE values obtained from radiosonde observations conducted by IITM from the IOP locations. In some places CAPE values are utilized obtained from http://weather.uwyo.edu/upperair/sounding.html, wherever nearby data are available closer to the flight observations.

Vertically integrated concentrations of mean aerosol concentrations sampled at 250m intervals are provided in table 1. Correlation coefficient of 0.49 is obtained between the AOD and aerosol concentrations.

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.*

Response:

With respect to the specific question of the reviewer, we offer here the LWC concentrations [g m^{-3} μm^{-1}] at the drop size of 40 micron for the four cases:

a. 21 June: 0.0001
b. 22 June: 0.03
c. 16 September: 0.004
d. 24 September: 0.00005

It is evident that there was 300 times more cloud water in 40 micron drops on the relatively clean day of 22 June than in the polluted case of 21 June. The contrast is even greater for 16 September. These are large contrasts that have great impacts on the rate of formation of warm rain.

We confirmed the onset of warm rain if occurred in the convective clouds with the help of cloud image probe which provides images of hydrometeors ranging from 25 to 1550 µm. The LWC contributed by the cloud droplets of the ranges 3-50 µm is quite significant as mentioned by the reviewer [clouds over the rain shadow areas]. However the LWC contributed by the droplets of the ranges 50-1550 µm, are insignificant depicting little contribution from warm rain. For example, please see the picture of the cloud images at 6.7 km (Figure 7) provided for the case on 21 June. The cloud droplets converted into ice hydrometeors without warm rain formation. This is where we have to rely on the analysis connecting cloud drop effective radius, cloud drop size distribution and the formation of warm rain. This will be extensively addressed in the companion paper that will be submitted next.

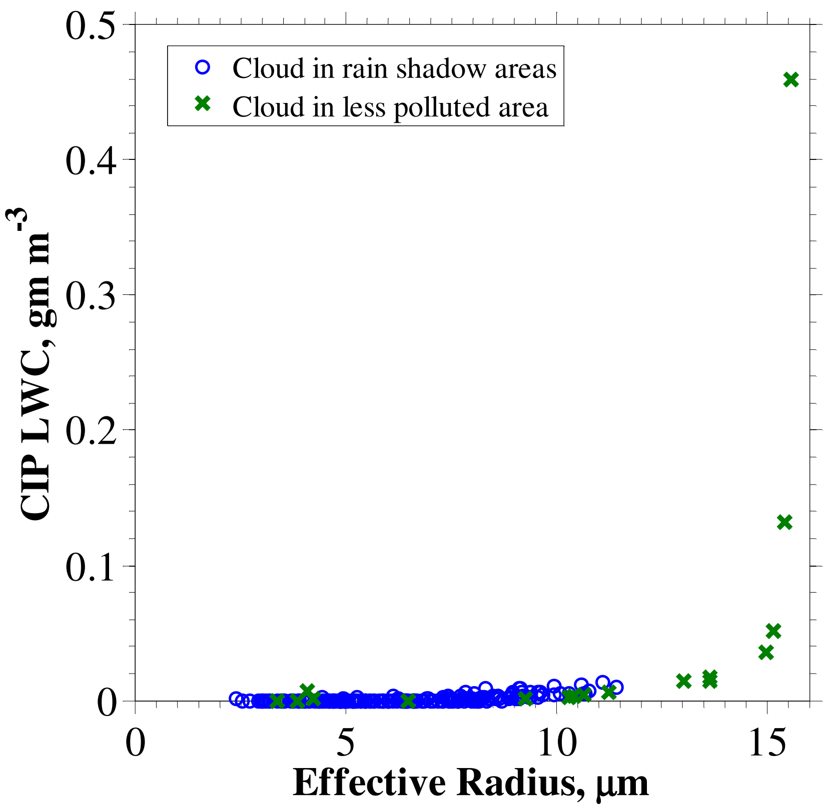

Fig. 7. Dependence of the CIP-measured rain liquid water content (CIP LWC) on the CDP-measured cloud drops effective radius, Re. Each point represents one cloud pass in the clouds. All the cloud passes in the polluted areas are indicated by the blue circles while the less polluted case is indicated by green crosses. Most of the clouds were profiled up to 7 km above the mean sea level. Significant rain (CIP LWC > 0.03 g m^{-3}) was observed only in the relatively clean case of Raichur, where Re exceeded the precipitation threshold of 12 – 14 µm.

In the revised manuscript the cloud base height is provided in table 1. The clouds that we profiled till ~8 km are convective clouds as described in the section 2.3 of the manuscript.

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.

Response: In the revised manuscript now it is corrected. Thanks for pointing out the mistakes.

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.

Response: In the revised manuscript this mistake is rectified. The cloud droplet concentration is now assigned as Nd.

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.

Response: The CCN spectra were calculated into a power law, and the supersaturation at 0.4% was calculated for that power law. The values of the power are relatively high, which are typical to microphysically continental environments (Cohard et al., 1998).

iv. Page 17014 L14: Can authors please explain what did they mean by super-continental clouds?

Response: Clouds that have very high drop concentrations and negligible warm rain processes even at large depth above cloud base. This definition was added to the text.

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.

Response: We obviously conducted horizontal penetrations at varying altitudes. It was clarified in the text.

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.

Response: Thanks for pointing out the mistake. Throughout the manuscript now only one nomenclature is kept for Nashik.

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.

Response: It has been hypothesized that early initiation of warm rain causes early rainout of the cloud water and depleting the supercooled water that is an essential ingredient in cloud electrification (Andreae et al., 2004). The paper of Lal and Pawar supports this hypothesis. If so, our study showing that the pollution aerosols suppress warm rain implies that lightning activity would be enhanced in clouds that become sufficiently deep. Indeed, intense lightning activity was recorded in the lightning detector of the aircraft from clouds that reached a cumulonimbus stature during the polluted conditions. The text was updated accordingly.

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.

Response: The take home message is as follows: The rain shadow area has small amount of rain primarily due to the dynamic causes being at the lee side of the Western Ghats. On top of that, clouds that manage to form there are frequently affected by large amounts of air pollution that suppresses the warm rain processes. This decreases or even completely shuts off the rain from clouds that do not reach the height of onset of rain, which is often higher than the freezing level. This means that rain from small clouds would be decreased by the air pollution, while rain from the very deep clouds will be less strongly affected. Radar studies indicated rain suppression from similar polluted clouds elsewhere, but the uncertainty is high, so that even the sign of the effect from the deep clouds is still in question. This is a matter of concern and further research for evaluating the magnitude of the impacts of aerosols on rainfall in this thirsty part of the world.

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.

Response: Proper care has been taken to provide correct English language throughout the manuscript.

As suggested we have increased the font sizes of the legends. For clarity the figures are redrawn and enlarged.

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.

Response: This is addressed in the response to the point before the previous one. We concur with the reviewer's comment that rain shadow areas of India do not get much rainfall. Through the aircraft observations we have shown that the rainfall may be affected detrimentally by the presence of large CCN concentrations where the warm rain process is suppressed. It was found in Thai cloud that the delay in the coalescence process can decrease the net rainfall amount from deep convective clouds with cool bases, as in the rain shadow area, by a factor of two (Rosenfeld and Woodley, 2003).

References:

M. O. Andreae, D. Rosenfeld, P. Artaxo, A. A. Costa, G. P. Frank, K. M. Longo, and M. A. F. Silva-Dias, 2004: Smoking rain clouds over the Amazon. *Science*, **303**, 1337-1342.

Cohard, Jean-Martial, Jean-Pierre Pinty, Carole Bedos, 1998: Extending Twomey's Analytical Estimate of Nucleated Cloud Droplet Concentrations from CCN Spectra. J. Atmos. Sci., 55, 3348-3357.

Rosenfeld D. and Woodley W. L.: Spaceborne Inferences of Cloud Microstructure and Precipitation Processes: Synthesis, Insights and Implications, A paper from the Symposium on Cloud Systems, Hurricanes and TRMM: Celebrations of Dr. Joanne Simpson's Career--- the First 50 years, 2003.