Responses to Anonymous Referee #1

We wish to express our thanks to Reviewer A for a thorough and constructive review. In what follows we provide detailed responses to the comments. We hope that the modifications brought to the text have improved the manuscript to make it suitable for publication.

This paper seeks to characterize the macrophysical and microphysical properties of ice clouds as a function of the large-scale cloud regimes derived from ISCCP, the amplitude and phase of the Madden-Julian Oscillation (MJO) and the large-scale atmospheric regime derived from a long-term record of radiosonde observations over Darwin. The paper uses both state-of-the-art retrievals of the cloud microphysical and macrophysical properties, as well as robust descriptors of the large-scale atmospheric regime. The paper improves upon past efforts of this nature by using a longer time period of observations (4 years). Further, there is a need for studies such as this in order to develop databases for comparisons with model studies to determine under what conditions the models do a good job at simulating cloud properties. This should aid in development of better process-oriented parameterizations for large-scale models. And, finally, the paper seems to be technically sound. Thus, I think the paper is appropriate for publication in ACP.

Thanks for this general assessment.

1. The length of the paper should be shortened and the quality of the writing should be improved. There is copious discussion throughout the manuscript. Instead of explaining every feature that is seen in all of the plots in the paper, the authors should concentrate on highlighting the most important points in the comparison. Otherwise, their message gets lost in a barrage of details and readers might lose interest before they come to the most salient points in the manuscript. Further, excessive speculative comments should be avoided (e.g., this is probably caused by that, appear to be fairly representative, etc.). Say what is important and stick to that.

In the new version of the manuscript, we have made every effort to remove all details that were diverting the reader from the main message, which is to highlight the most salient features in terms of variability. We totally agree with Reviewer#1 that this type of paper needs to be written carefully in order not to lose the reader along the way. However, in the present case, we have tried to use three different criteria, which need to be described. Then we have characterized several types of cloud properties, where most papers concentrate on one of them (cloud top for instance). We believe that it is interesting (and unique in a way) to go for the most comprehensive type of analysis instead of trying to publish 6 papers, one for each criterion used and one type of cloud properties only. It would make more attractive short papers, but we don't think it would give the whole picture and would result in weaker papers. But this choice has a cost in terms of number of figures and length of paper. We note that the reviewer probably agrees with the approach because he did not criticize the fact that we've used three criteria and a large number of cloud properties. In order to take this comment into account as best as we could we have therefore reviewed carefully each result and selected only the most important ones in order to reduce size. I think we have gained about 1.5 pages, which shows that it was worth the effort indeed. On another hand, we have had to account for other comments which resulted in few additional paragraphs as well. We hope the paper is now easier to read and more attractive. We have also modified the abstract significantly to better state upfront what the main objective of the paper is (this was also a comment from the main editor, B. Stevens) : "*The high complexity of cloud parameterizations now held in models puts more pressure on observational studies to provide useful means to evaluate them. One approach to the problem put forth in the modelling community is to evaluate under what atmospheric conditions the parameterizations fail to simulate the cloud properties and under what conditions they do a good job. It is the ambition of this paper to characterize the variability of the statistical properties of tropical ice clouds in different tropical "regimes" recently identified in the literature to aid the development of better process-oriented parameterizations in models*.". We have also been very careful to remove all speculative comments and hope there are none left.

2. There are a number of times in the manuscript where it is stated that differences in cloud properties between regimes are significantly different. However, as far as I can tell, no tests of statistical significance have been applied to the results. This would be a good idea to incorporate into the manuscript. Results of statistical significance could be summarized in a table in order to avoid the need for excessive text.

That's absolutely right, we need to apologize for that. This also directly relates to your comment 3 below. The reason is that we did not explain what we meant about "statistically significant". This has been now discussed thoroughly in the text. In the present case, there is no easy way to run classical statistical significance tests. However, we have used the simple following definition (which is also given in the manuscript now) : "*These error estimates on the cloud properties will be considered as a measure for statistical significance of the variability signatures in this paper, that is, if the variability measured is larger than these uncertainties, it will be loosely referred to as "statistically significant"* ". What we have done in the text is a better discussion about what we know about the error magnitudes in our radar-lidar retrievals (to account for comment 3 below), and then we state that we'll use these numbers to define what variability signature observed is statistically significant (when the variability is larger than the error on the parameter) and when it is not (when the variability signature is smaller than the error on the parameter).

3. There is little mention about what are the uncertainties in the retrieved cloud macro-physical and microphysical properties. I think this should be stated upfront (succinctly) when it is stated what cloud properties will be derived. This will aid in interpretation of whether differences between regimes are different in a statistically significant sense.

It is exactly the path we have chosen in the new version of the manuscript, keeping in mind not to do this at the expense of paper length too much. The expected uncertainties on these morphological properties are due essentially to the vertical resolution. Regarding the frequency of occurrence, the radar-lidar combination detects most clouds in the troposphere, except the thinnest cirrus clouds overlying low-level clouds (the radar won't be sensitive enough to detect them and the lidar will be extinguished by the liquid layer). It is however very hard to quantify if this is a significant effect or not, and I can't imagine an easy way to do that, because there are no other instruments to get a reference from. The expected uncertainties on the microphysical properties have been discussed in Protat et al. (2010) and DH08. The accuracy of the DH08 method has been estimated using synthetically-generated radar-lidar profiles following Hogan et al. (2006b), and to some extent in Heysmfield et al. (2008, JAMC). These estimated errors range from 10 to 20% for the radar-lidar part of the method, 20 to 40% for the radar part of the method, and can be larger than 50% for the lidar part of the method (except for extinction, for which the method is more accurate, DH08). These numbers do not include all possible sources of errors, but there are again clearly no easy ways to do that (although efforts are ongoing using radiative transfer codes and surface fluxes measurements, as well as intercomparison exercises with in-situ microphysical best estimates of the same parameters). These comments have been introduced in the text:
"*The expected uncertainties have been discussed in Protat et al. (2010), Heymsfield et al. (2008), and DH08. The accuracy of the DH08 method has been firstly estimated using synthetically-generated radar-lidar profiles following Hogan et al. (2006b). The estimated errors ranged from 10 to 20% for the radar-lidar part of the method, 20 to 40% for the radar part of the method, and could be larger than 50% for the lidar part of the method (except for extinction, for which the method is more accurate, DH08). Similar numbers were found in comparisons with selected in-situ microphysical profiles for which the closure with the bulk microphysical measurements was excellent (see Heymsfield et al. 2008 for further details). However, it must be stated clearly that these numbers do not include all possible sources of errors, but there are clearly not easy way to do that (although efforts are ongoing using radiative transfer codes and surface fluxes measurements, as well as intercomparison exercises with in-situ microphysical best estimates of the same parameters)*."

Responses to Anonymous Referee #2

We wish to express our thanks to Reviewer #2 for the support and a constructive review. In what follows we provide detailed responses to the comments. Please note that we have not understood the numbering you've used (or is it an error in the PDF conversion at Copernicus ?), so it has not been possible to track down the two last comments you gave. Please let us know where it is in the text and we'll address those.

This is a comprehensive study of ice clouds observed using ground-based radar and lidar observations from the Atmospheric Radiation Measurement Program at Darwin, AU. The properties of these clouds in different meteorological regimes are examined using a huge data set comprising four years of data. This study will be of considerable use for evaluating general circulation models in the Darwin area and I recommend that the article is accepted for publication in nearly its current form.

Many thanks for this nice comment on our work.

1. In Abstract, remove mention of terminal fallspeeds.

Done. Thanks.

2. 20071, line 20. Better understanding of microphysical processes and in-situ observations are also key. An example is ice concentrations. Shattering on the inlets of probes has resulted in overestimates of ice concentrations and extinction estimates. A second example is to better understand ice nucleation pathways.

We fully agree that the sentence was misleading and could make the reader think that the investigation of the statistical cloud properties is the only important one, which is definitely not true. This sentence has been modified accordingly : "*Further evaluations and improvements of model performances and development of new cloud parameterizations must now rely not only on a better understanding of cloud processes (using detailed in-situ microphysical observations of the nucleation and growth processes), but also on a better understanding of the statistical properties of clouds ...*". Hope this was the sentence you were referring to (because we did not understand the numbering you used for the comments, as explained previously).

3. 20073, lines 15-16. My most major comment is that this study has required the use of lidar data. Anytime cloud optical depths are higher than about 3, corresponding to somewhat thin and high ice cloud, the observations are not included in the analysis. This skews the observations to a certain yet relatively unknown subset of the ice clouds in the area. This point needs to be emphatically emphasized in the abstract and elsewhere.

There is a misunderstanding here, clearly. Both radar and lidar are used, but it does not mean that we need to have both, as was explained in the description of the Delanoe and Hogan method and in other places of the manuscript. Common radar-lidar samples are where the accuracy is best, but we also consider radar-only and lidar-only areas for the estimation of all cloud parameters. This is the beauty of the radar-lidar combination: the lidar will detect all the optically thinnest clouds (including those too thin to be detected by the cloud radar), basically most clouds of optical depth less than 3, while the radar will detect all optically-thick clouds, for which the lidar is completely extinguished. And there is an overlap between the two instruments. We fully exploit in this paper this complementarity. This confusion is probably due to the fact that we have not insisted enough on that complementarity. Sentences have been added to make this point even more clearly.

4. 20076, lines 24-25. This is not really ice cloud occurrence. See comment 3.

As discussed above, except few clouds that are missed in very special occasions, it is indeed ice cloud occurrence, and it includes most clouds of the total sample (a comment has been added to specify that we have no way to evaluate exactly how much we miss, but everybody agrees in the literature that it is a very small amount when you have both a radar and a lidar).

5. 20078, line 5. Not true cloud top heights, only when lidar and radar both detect cloud?

No. This is when lidar OR radar detects cloud top.

6. 20081, line 5. Unless you can demonstrate this it should be taken out.

Sorry, impossible to find out what part of the manuscript you are referring to. Please let us know where it is and we'll be happy to address this comment.

7. 20082, lines 1-2. It is not easy to believe this level of accuracy.

If you refer to the accuracy we were quoting with the unpublished results in page 14, line 1-2, we have taken out this comment, as there is a more complete discussion in section 2 now about the expected magnitude of the errors from published material.

8. 20083, line 2. This is not a plausible explanation as aggregation is inhibited by both small particles and temperature at these heights. A plausible explanation is sublimation although the IWCs increase downward so this is not a likely explanation either. Could it be a retrieval artifact. Could it be where lidar alone and lidar/radar together detect cloud?

Maybe we did not explain that part clearly enough, so these explanations have been partly rewritten. Aggregation is clearly expected in layer 2 (from 15 to 9 km), since a lot of in-situ microphysical observations at those heights do show the existence (and predominance for tropical ice anvils) of ice aggregates. Maybe the reader was left with the impression that we were talking about layer 1 ? In layer 1, as claimed by Reviewer 2, we don't believe that aggregation plays a role (we do agree with Reviewer 2). At these heights, deposition and homogeneous nucleation are generally found, with a predominance of pristine crystals, and the results we get from the radar-lidar retrievals agree with the in-situ observations from the literature.