Response to reviewers

Reviewer 1

There is a general lack of information relating to what the purpose of this paper is. For example,
1.1)Why is it important to compare the two methods of calculating albedo?
A) The paper is designed to validate the DE approximation of albedo using data from VOCALS. If shown to be suitable the DE method could be used to give an albedo value in simulations of stratocumulus decks without the need of a radiative transfer model.

1.2)What implications do the conclusions have?
A) The delta-Eddington approximation can be used as a method of calculating the albedo in a microphysical model and that assumptions within the DE method are valid.

1.3)How does this work compare to previous studies?
A) No other studies have to the authors knowledge performed analysis using two insturment sets on the same aircraft therefore comparison is difficult.

1.4)What is novel about the work done in this study?
First work using two independent methods on one platform and the first work to validate the DE method on clouds observed during VOCALS-REx

2) I have concerns about the method used to derive cloud optical depth, and hence the cloud albedo from the Cloud Droplet Probe (CDP). In equation (2) the effective radius should be representative of that near cloud top, whereas the authors use that derived from droplet size spectra measurements made lower down within the cloud layer, where the effective radius will presumably be smaller. I am also very dubious about the derived LWP measurements which are also used in equation (2). The LWP is assumed to be the LWC data measured at the aircraft flight level in the cloud layer multiplied by the cloud thickness below the aircraft. Even if one thinks of an idealised stratocumulus cloud that exhibits a triangular adiabatic LWC profile, then this assumption will only be correct if the aircraft is flying at a certain level within the cloud, which is almost certainly not the case. The authors could look at how the derived LWP compares to the integrated LWC made from aircraft profiles through the depth of the cloud layer e.g. at 1200 to 1400 seconds in Fig 1. The authors also assume a constant value of the asymmetry factor in equation (1). They could actually calculate this by including the measured drop spectra in Mie scattering calculations, although the same caveats about the data not being representative of that at cloud top would still exist. All of these factors lead me to suspect that there is a much larger uncertainty in the albedo and LWP derived from the cloud microphysical data than is indicated by the error bars shown in figures 3 and 4.
A) The LWP is now calculated using both methods to give the albedo using several approximations within the DE method. Furthermore uncertainties have been recalculated and an expanded section has been added to the manuscript.

2.2) In addition, I am also unsure of how the radiometric albedo measurements are actually made. Are these made by the aircraft overflying the same cloud layer that is analysed for the in-situ cloud microphysical measurements?
*A) The radiometric albedo is calculated using in situ radiometers see Slingo et al 1982. The calculation of the uncertainties in the radiometric albedo are also described in section **2.2***

3) For cloud segment 1, the authors show poor agreement in the albedo calculated from the two methods, and attribute this to the high solar zenith angle. It is also the case where the aircraft was flying lower down in the cloud layer for the cloud microphysics measurements, and so may be

subject to enhanced biases in the derived optical depth than the other cases for some of the reasons mentioned in point 2.
A) The cloud segment identified as segment 1 in the draft has been removed as part of the expansion to 9 clouds (originally 11 however two have been removed as the SZA in those cases is above 65 degrees).

4) The authors have used a very limited observational dataset. From the 13 research flights made with the FAAM BAe-146 aircraft only four cloud segments are analysed. The authors select these cloud segments based on requirements to have "good CDP and radiometric data with satellite coverage and contain little to no coastal pollution".

4.1)Firstly no satellite data is used in the paper so why is it a constraint?
A) This is a mistake and the reference to satellite data has been removed.

4.2)Secondly why do the authors only look at clean cases, where contrasting this with more polluted cases near the coast would be of interest?
A) As part of the expansion to 9 clouds several polluted cases are now used.

4.3)Thirdly why not use all research flights to increase the number of cases, so that any conclusions made are more robust?
A) Clouds are now analysed from six flights B408, B413, B414, B417, B418, B419. No suitable long in cloud legs exist in the other flights.

Reviewer 2

This article compares shortwave cloud albedo measured/derived from two different instruments making in-situ measurements on a research aircraft during the VOCALSREx field campaign, which sampled the stratocumulus clouds of the southeast Pacific. Its main contribution is to demonstrate that for solar zenith angles smaller than 65 degrees the delta-eddington method applied to aircraft measurements of effective radius and liquid water path has small errors compared to shortwave radiometric measurements, so microphysical aircraft measurements can be used to reasonably estimate cloud albedo. While this is a useful result, the paper needs major work on its clarity and specificity of its reasoning, justifications and purpose. For example, there is much emphasis placed on the result that the albedo derived from microphysical measurements works poorly for large solar zenith angles. But, this is a known consequence of using the delta-eddington method and not a novel finding. While these results possibly could be used to justify a more detailed look at albedo during VOCALS across more platforms, this work may serve better embedded within a more thorough paper. Nonetheless, with enough improvement to the argument flow and explanations other studies might cite this work, allowing them to justify their method succinctly. Cloud albedo is arguably one of the most important quantities to constrain with regard to stratocumulus, and is a key quantity in both model and observational studies. This paper needs to emphasize this more. I therefore recommend this paper be accepted with major revisions. More details regarding my concerns/suggestions are described below:

Although several VOCALS platforms are mentioned, this paper does not address which ones have shortwave radiometric measurements or CDP, so it is not clear whether albedo from microphysical measurements would add missing information to the VOCALS platforms.

1.1)It is not mentioned whether radiometric measurements are common on similar platforms.

A) The SW radiometric measurements are collected in cloud on the BAe-146 aircraft as described section 2.2

1.2)The paper states the ability to calculate albedo from microphysical measurements ‘enables a more detailed investigation of marine stratocumulus clouds,’ but offers no explanation as to what this means. The motivation for the work needs to be clarified.

A) The language used has been clarified and a redirection towards validation of the DE method using cloud from VOCALS has been done.

2) One of the main results of the paper is the range of solar zenith angles under which the cloud albedo derived from the Cloud Droplet Probe measurements using the deltaeddington method compares well with the shortwave radiometer measurements. This likely shouldn’t be the main focus of the paper because it is already known that the delta-eddington method is subject to large errors at large solar zenith angles and in fact the introduction of this paper explains that this method “breaks down” when the cosine of the solar zenith angle is smaller than 0.4, or 66 degrees. On the other hand, the R2 value between the measured and derived albedo within acceptable solar zenith angles is a useful quantity for future reference.

A) This has been done, as with comments from Reviewer 1, we now calculate the albedo from the DE method using two different assumptions and show the resultant R2 values.

3) Why are only clean clouds compared? The introduction refers to studies that found aerosol content affects cloud albedo, but in Section 2.2 it is not made clear why you would want to look at only clean clouds. In Section 2.3 it is mentioned that g=0.85 is used as it has been found in Twohy et al. (2005) to be appropriate for droplets in a clean environment. However in this paper, the ‘clean environment’ (phrase not used in Twohy et al., 2005) refers to the absence of significant amounts of absorbing soot particles which would affect the asymmetry parameter, not coastal sulfate.

A) Nine clouds from six flights are now analysed and these include both clean and sulphate polluted

environs.

4) The amount of sulfate aerosol that indicates ‘coastal pollution’ is not quantified clearly.
A) This restriction is no longer applied to the data.

5) State briefly in abstract why you compare albedo measurements– what is the motivation?
A) We compare albedo methods to validate the DE method in the case of the VOCALS-REX campaign.

6) The fact that the VOCALS-UK flew 13 research flights including investigations along 20S transects is good information for the introduction, but doesn’t seem necessary in the abstract, especially since all 13 flights are not used in the paper, and 20S is only referred to in the introduction.
A) We have tidied up this section, we use flights from both polluted and 20S sections.

7) Introduction, line 20-22: mention that the list of measurement platforms involved in the campaign are aircraft and ships where applicable. “The campaign involved the NCAR C-130, . . .etc.” may confuse a reader unfamiliar with the campaign.
A) We agree and have expanded this section giving descriptions of each platform.

8) Introduction, line 24-26: this appears to be an incomplete sentence. “Data from the . . .operated by the . . .” The abstract indicated that FAAM BAe-146 data is used, but this sentence fragment doesn’t make that clear in the introduction.
A) The phrasing is a requirement of use of the FAAM data. We have altered what we can to improve the flow of the paragraph

9) What is the basis for the assumption that uncertainty in liquid water path assumed to be 10%? Also, the sentence that states this assumption may be missing a phrase: “The uncertainty in the liquid water path, both from the liquid water path and the relative. . .”
A) The uncertainties have been recalculated for each value of the LWP. See section 2.3 for more details

10) The description of factors contributing to measurement uncertainties is thorough, though it isn’t clear in the paper how uncertainties from the various sources are combined. “The different uncertainties are combined using the standard methods for uncorrelated uncertainties based on Eqs. (1-3).” Although it isn’t necessary to show these methods in full detail, names and references of methods used would help readers who would like to reproduce your calculation of albedo from aircraft data.
A) A detailed description of the uncertainties has been added in section 2.3

11) Although not explained, it seems that the cloud albedo compared is the cloud albedo at the level of the aircraft, rather than the top of cloud albedo typically used in albedo investigations. If this is true, explaining this may help clarify the choice of methodology for computing liquid water path based on cloud thickness below the aircraft and the shortwave radiometric albedo computation.
A) The albedo is calculated from both methods in cloud and is now detailed in the manuscript.

12) The conclusions state cloud segments were chosen not only to be ‘clean,’ (with no reason given for this) but also to have ‘radiometric data with good satellite coverage,’ but nowhere in the paper is satellite coverage mentioned nor is satellite data utilized. A comparison with satellite data would require more assumptions and calculations to consider albedo at the top of the cloud rather than at the level of aircraft measurement.
A) The inclusion of satellite was a mistake and has been removed, as described above the clouds

are no longer required to be 'clean'.

13) While Figs. 2 and 3 are interesting, more explanation regarding why you would compare measurements in this way and what you learn from it is necessary. The relationship seen between effective radius and albedo, and liquid water path and albedo are similar. This is of course similar to the relationship between optical depth and albedo. What is learned from this comparison?
A) These comparisons were performed to validate the results with previous experiments. We have removed one result while retaining the effective radius plot which shows the variance in the effective radius in the flights observed.

Reviewer 3
General Comments:
Parkes et al. present a fairly straightforward study of the suitability of using aircraft in situ measurements of cloud microphysical properties as input into the delta-Eddington approximation to estimate the shortwave albedo of clean marine stratocumulus clouds. To test this technique they compare the shortwave cloud albedo derived from the microphysical measurements of the Cloud Droplet Probe on the aircraft to direct measurements of the shortwave cloud albedo measured by up- and down-looking shortwave radiometers on the aircraft. Based on this limited comparison (only 4 short flight segments) they conclude that the delta-Eddington approximation is suitable as long as the solar zenith angle is less than 65 degrees.
In the revised manuscript we now study 9 clouds all of which have a SZA of less than 65 degrees and are therefore appropriate for use with the DE approximation.

This paper mainly deals with the validation of a technique and is not really a description of the "albedo properties of ...stratocumulus...during VOCALS-REX..." as the title indicates. As a 'descriptive' type paper I would be concerned that it only looks at a limited number of case studies (only 4 short flight segments). But as a 'validation of a technique' type paper the number of cases is not as big of a concern (although more is always better). Also, as a 'validation of a technique' paper I would ordinarily recommend that it is more appropriate for the Journal of Atmospheric and Oceanic Technology. But since it is part of the VOCAL's special issue I feel it's appropriate for ACPD.
We have changed the name of the manuscript and agree that it is more suitable.

Specific Comments:
Title:
- For the reasons mentioned above, I think this paper is mis-titled. A more appropriate title may be something like, "Validation of the delta-Eddington Approximation to derive the albedo of clean stratocumulus clouds during the VOCALS-REX field campaign"
Abstract:
- The abstract is succint and to the point.
- Page 30021, Line 11: Recommend changing "below 65 degrees" to "less than 65 degrees" since "below" could be confused with lower in the sky, when really smaller solar zenith angles mean higher in the sky.
A) Corrections to the title and abstract have been made

1. Introduction:
Page 30021, Lines 19-20: Specify which country VOCALS-REX was based out of.
Specify off the coast of which country were the aircraft measurements made.
Page 30021, Line 22: It should be specified that the Ron Brown and IMARPE are research vessels.
Page 30021, Lines 23-24: Specify the country where Paranal and Iqueque are located.
Page 30021, Lines 24-26: The sentence "Data from the UK's ..." is incomplete
Page 30021, Line 25: "Facility" is misspelled.
Page 30022, Lines 1-6: Paragraph starting with "Much of the investigation...": Since this paper does not investigate the effects of cellular convection on cloud albedo there is no need for this paragraph.
A) Corrections to the title and abstract have been made

2.1 Instruments:
Page 30023, Lines 14-17: This description of how the uncertainties in the pyranometer measurements were derived is too cursory. It's not clear how you came up with 10Wm- 2 and this number appears arbitrary. Is this meant to be plus or minus 10 W/m2? How were they based on the

comparisons to UKMO standards? Since these measurements are central to the paper this should be elaborated more.
This has been done in section 2.3

Page 30023, Lines 14-17: Were the pyranometer measurements corrected at all for the attitude of the aircraft? This is more important for the up-looking radiometer which can be significantly affected by any tilting of the instrument due to the pitch and roll of the aircraft. This tilting can introduce offsets into the signal that can then throw off the albedo measurements. If you did do these corrections you should explain how you did them. And if you did not do these corrections you should explain why you did not.
A) These corrections are automatically applied by FAAM

Page 30023, Line 18: Remove "of" from "major of sources"
A) Done

2.2 Observed Data:
Page 30024, general comment: I'm curious why you only looked at 'clean' clouds? It seems like VOCALS provided an excellent dataset to investigate the use of the ïA¸d'- Eddington approximation for a wide range of marine stratocumulus clouds from 'clean' to 'polluted', from precipitating to non-preciptating. Investigating more and a wider range of cases would have provided a more valuable contribution.
A) Six flights and 9 clouds are now analysed

Page 30024, Line 10: Eliminate "and" from "...2D-S instruments and be free..."
This sentence has been changed.

Page 30024, Lines 10-19: In your description of the flight segments used in the analysis you describe climbing into the cloud, doing a straight and level leg in the cloud to obtain the microphysical measurements, then climbing out of the cloud.
But you don't describe how you measured the albedo above the clouds with the pyranometers?
Did the aircraft retrace the flight path of the in cloud flight leg?
How high above the cloud tops was the aircraft when it made the radiometer albedo measurements?
A) As described in Slingo et al (1982) the SW radiometers can be used in cloud and have been in this case. This has been made clear in the manuscript.

This is my main concern with this study: Were the radiometer measurements of the albedo made above the cloud? Or did you use the radiometer measurements of the leg in the cloud? If you used the radiometer measurements in the cloud then I'm not sure what your analysis means since you are just looking at the albedo inside the cloud, and not the top of the cloud albedo which is really what is required.
A) We do use the in cloud radiometric results as has been done in Slingo et al 1982. We agree that a top of cloud albedo is of more use however this is a limitation of trying to use a single platform in cloud. Further work analysing the change in cloud albedo through a direct transact of a cloud is an interesting concept which we would like to expand upon in a future publication.

If you are using radiometer measurements above the cloud, then you need to describe this in detail. How did you ensure that you were sampling the same area of the cloud so you could relate the in situ microphysical measurements to the above cloud radiometer measurements? Were there only clear skies above the aircraft for the albedo runs?
A) We have now described that the albedo measurements from the radioemeters were taken in cloud.

Conclusions:
Page 30027, Line 16: You mention that each of your cloud cases required satellite coverage, but you did not do anything with satellite data in this study so I'm not sure why this was a requirement. This phrase is not required.
A) The reference to satellite data has been removed.

Page 30027, Line 24-26: The final conclusion of this paper is perhaps a bit too strong given the limited number of cases investigated. A more appropriate conclusion may be something like "The results of this limited study show that for clean marine stratocumulus clouds the delta-Eddington approximation can be suitable for deriving the albedo of the clouds for solar zenith angles less than 65 degrees. This agrees with the theoretical work in Joseph et al. (1976)."
A) This change has been made

Page 30033 and 30034, Figures 2 and 3: Is the y-axis the albedo measured by the pyranometers or is it the albedo calculated using the given micropysical measurements (effective radius, LWP) as input to the delta-Eddington approx? If it's the pyranometer measured albedo, how did you relate each microphysical measurement to the pyranometer measured albedo?
This goes back to my comments above about how you measured the albedo with the pyranometers, in or above the cloud?
If above the cloud, how did you correlate each albedo measurement with the in situ microphysical measurement?
If this is the pyranometer measured albedo in the cloud, then I'm not sure if
there's any value to this analysis.
A) The albedo is the pyranometer albedo and is calculated using data which has been collected simultaneously with the microphysical data.