Review #1 of "Aerosol first indirect effect of African smoke in marine stratocumulus clouds over Ascension Island, south Atlantic Ocean" by M. de Graaf et al.

This study uses a single instrument to study aerosol first indirect effect with one month of data collected at the Ascension Island in the middle of the south Atlantic Ocean during the southern African biomass burning season. The manuscript is relatively well written, and the scope of the study is properly fitted for ACP, however, the current form of the manuscript reads too technical (more suited for AMT), and the significance of the results shown needs to be assessed before it can be considered for publication. Moreover, upon addressing the above mentioned points, the structure of the manuscript also needs to be revised for the ease of readership.

The reviewer is thanked for the thorough and careful review of the manuscript. Many issues were raised, and we have tried to answer them all satisfactorily below. The manuscript was completely revised, changing it from a technical reading to a geophysical article about the aerosol-cloud interactions over Ascension Island in the context of the stratocumulus to cumulus transition, which is important in this area. The suggestions and comments from the reviewer were very helpful to improve the manuscript to be more useful for the scientific community.

Below the comments are answered in detail. It is indicated when the manuscript was changed to comply to the raised issue.

Main concerns:

1. Statistical significances of the analyses (all 3 methods associated with Fig. 4-6) need to be included and discussed. Working with only one month of available can be quite challenging, but I believe one can still make valuable statements with proper significance assessments.
   Agreed. The statistical significance, comparative numbers from different other studies and a discussion on the meteorological conditions during the campaign, affecting the results, are now added to the manuscript.

2. attribution of the Twomey effect requires constant cloud macrophysical properties, e.g., cloud LWP, and environmental conditions, I understand this can be difficult with less than 40 sample size, but these limitations/assumptions need to be acknowledged when interpreting the results and making attributions (i.e., saying these indicate the Twomey effect).
   Agreed. A discussion section was added to discuss the limitations and the the manuscript was restructured to better describe the results and the uncertainties.

3. Section 5.2 confuses me, if one cannot validate the representativeness of

the retrieved cloud properties, how can one study the interactions of them with retrieved aerosol properties? Moreover, if the Radar retrievals of cloud properties is really problematic and biased (re retrieved using daily averaged or some assumed Nd values are certainly not suited for aerosol-cloud interactions studies), what's the point of validating Lidar Nd with Radar retrievals? Furthermore, it doesn't validate a retrieved variable when another variable retrieved using the same instrument is involved in the validation.
Radar can be used to estimate cloud droplet number density and effective radius using the well-known and often used methods described by Frisch *et al.* (1995) and Frisch *et al.* (2002). If only radar measurements are available, an estimate (assumption) on cloud number density can be used to determine effective radius. This is done in Frisch et al. (2002). However, an independent estimate of cloud droplet number density can be achieved using the lidar measurements. In this way, the complementary measurements should yield cloud parameters that are more accurate and less sensitive to assumptions. The section describing this in the manuscript was rewritten to show this more clearly.

4. The current form of the manuscript reads too technical, especially Sections 2.1-2.4 and 5.2. These technical details can be condensed and summarized in the main text, with details provided in an appendix or a supplement.
The manuscript was completely restructured, following the suggestions of the reviewers. The theory was moved to the Appendix, along with technical sections. The introduction was rewritten, to include literature references.

5. The validation section needs to be moved up before showing the results. How can a reader interpret these results without knowing the retrievals that these statements based on are validated?
The manuscript was restructured. First the campaign and the lidar measurement results are shown, with comparisons from other sources. Then the ACI are determined and described. The discussion section now shows the uncertainties that are associated with the lidar and radar cloud retrievals.

6. The current form of the Conclusions reads like a summary and repetitive of what have been stated. Emphasizing on the advantages (and caveats) and implications of the study would be very helpful.
The conclusions (and introduction and results) sections have been completely rewritten.

Minor comments:

The authors tend to state existing knowledges without providing references, for example:

1. P1 Line17 and P8 Line166, the typical thermodynamical structure of the MBL clouds over Ascension during the dry season can be found in Zhang & Zuidema 2019 ACP. The reference was added.

2. P2 Line33 and P8 Line173, Zuidema et al. 2018 GRL provides a more updated overview on LASIC and some first results. The reference was added.

3. When providing information on the transport, seasonality, and distribution of the smoke aerosols to set up the context on the complex environment within which smoke-cloud interactions manifest over the SE Atlantic during the southern African biomass burning season, Adebiyi & Zuidema 2016 QJRMS, Adebiyi et al. 2015 JClimate, and Zhang & Zuidema 2021 are suitable references. The introduction was rewritten and several reference were added to properly describe context of smoke in the African biomass burning season.

4. P11 L222, reference for the theoretically feasible values? McComiskey *et al.* (2009). This was added.

5. P13 L284, "from the literature", which one? L273: A typical value for Nd for marine, low-level stratocumulus clouds is 100 ± 70 cm^{-3} (Davidson et al., 1984; Martin et al., 1994). This was changed in the manuscript.

Since cloud properties retrieved by the lidar only represents cloud base values, and (as the authors also mentioned) cloud droplet size is highly dependent on height, I wonder if it's more appropriate to indicate that this study focuses on the aerosol indirect effect at cloud base in the title, with"... at cloud base of marine stratocumulus clouds ..."?
Fair enough. The title was changed, to reflect this and the concern by reviewer #2 about the cloud type.

The current introduction is too thin, introduction to existing knowledges on aerosol cloud interactions over the region is needed to set up the scientific question. The introduction to the Ascension Island and its environment, i.e., the smoky SE Atlantic during dry season, and the campaign info need to be moved up, preferrable to the introduction. When reading the current manuscript, a reader has no idea of the context (the condition under which these measurements were made) until P7 Section 3.
The introduction was rewritten

If the 2017 measurements were affected by alignment problems, why all your proof of concept exemplary figures show 27 Aug 2017? Why not use a day from 2016?
A day from 2016 is now used.

Why the sample sizes of the 3 methods not consistent? (37 in Fig. 4, 39 in Fig. 5, and 32 in Fig. 6). This needs to be justified.

The number of samples change, because for each method different criteria are used, as described in the manuscript. E.g. in the second method only clouds between -300m and +1000m from the cloud base are considered, whereas in method three only clouds are considered when a successful extinction profile was also retrieved from beneath the cloud. In the first method all successful cloud retrievals were used during the defined days. This is different between the methods, not necessarily inconsistent. However, care has to be taken when comparing different results, this is now better explained in the manuscript.

Is this correct that only the 2016 data is used for all you results? I think making this clear in a Data & Methods section would be nice.
Yes, only 2016 data was used to compute the IE. Validation was also done on 2017 data, due to the unavailability of radar data in 2016. This was made clearer in the manuscript.

Section 4.3, how is the two IEs calculated in this method? It seems you have retrievals (sample size ranging from 3 to 24) of cloud and corresponding aerosol properties for each cloudy period, from which IEs are derived? Making this clearer would be nice.
This is explained in the manuscript. Three to a maximum of 24 samples of 30 s intervals were averaged, from which the IEs (now ACI to be consistent with McComiskey *et al.*, 2009) are calculated. In Fig. 7, the sample size (time period length) is indicated by the color.

Check for spelling: P11 Line232: Ascension; Line 228: August or September? Fig. 5: daily.
Done
P11 L220, what does "cloud inversion" mean?
Lidar inversions, this was changed
Define abbreviations at first use: SNR, ATB
SNR was defined, ATB was not found
Fig. 7-9, validations are better illustrated with scatter plots with R2 values provided, similarly as in Fig. 11.
Done. Fig. 7 (now 3) includes a scatter plot of the measurements, and Fig 8 and 9 were revisited and now show temporal plots and scatter plots of the retrievals, including linear fits to the comparisons.
Fig. 6, x-axis font needs to be adjusted.
Done, the figure was updated and improved.
Fig. 8-9, the range of y-axis needs to be adjusted to remove the empty space above and to see the variability in the variables better, and why filling in no-retrieval period with straight lines, if you don't have values for that time period?
Done. The figures were updated, see above.
Data doi needs to be provided.
Done.

References:

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