Response to Referee #1

I thank the authors for their excellent response to the majority of my points and the extra analysis they have done. My previous points have been largely addressed, although a few small points remain that I think would be good to clarify before publication.

The authors are grateful to Referee #1 for the valuable time spent on thorough reading of our revised manuscript and responses. The constructive comments guided us to further improve the manuscript. We have taken notice of all comments to the revised version, listed below in black, with our responses in red. Associated changes in the revised manuscript (using "track changes") are also indicated, copied in the responses in "quotes" and line numbers are provided.

Main points

1. Many thanks for this additional explanation of the q-factors. I have one further question about the interpretation. With the new text 'Explanatory power of combined effects is larger...' (L44), this seems to imply that the impact of aerosol and meteorological factors are combining to generate a larger effect. Could it not be some other factor that causes the meteorology and aerosol to co-vary along with the clouds, such that it appears there is a combined effect, when it is actually a confounder?

Answer: Yes, this could indeed be the case. The larger correlation and high explanatory power of combined pairs of aerosol and a meteorological parameter, it is not clear whether there is a combined or a confounding effect on cloud properties.

The text in lines 46-50: "The results from the GDM analysis show that the explanatory power of the combined effects of aerosol and a meteorological parameter is larger than that of each parameter alone." has been changed to "The results from the GDM analysis show that cloud parameters are more sensitive to the combination of aerosol and a meteorological parameter than to each parameter alone but confounding effects due to co-variation of both parameters cannot be excluded." The sentence "Thus, the GDM provides an alternative way to obtain information on confounding effects of different parameters." has been removed.

2. It is good to have a justification for using the CER instead of the N_d, but some of the points should be referenced or removed. The studies that used CER alone were not really looking at the Twomey effect in isolation, such that they were not really studying the RF_{aci} either (McComiskey and Feingold, 2012). While N_d is affected by biases in the CER retrieval, these are different to the CER biases alone (and in some cases may offset each other; Painemal and Zuidema, JGR, 2011). For marine stratocumulus clouds, the N_d retrieval appears to be surprisingly accurate (Gryspeerdt et al, ACP, 2022). I would also note that while it is clear there is a relationship between CER and LWP, given that LWP is calculated from CER and cloud optical depth (as is N_d), neither of these is a "retrieval error" as such. All the properties could be retrieved perfectly and you could still find a relationship between CER and LWP (particularly for adiabatic clouds).

Answer: Thank you for this comment. The text in the Introduction (lines 181-190) has been reorganized as "It is noted that RF_{aci} is formulated in terms of N_d, whereas studies on the Twomey effects often use CER alone instead of N_d, such that they were not really looking at the Twomey effect in isolation and not really studying the RF_{aci} either (McComiskey and Feingold, 2012). CER is readily available as a satellite retrieval product, although in particular

over land the reliability is questioned (Grandey and Stier, 2010), whereas N_d is derived from CER and the cloud optical thickness (COT) (e.g., Grandey and Stier, 2010; Arola et al., 2022). While N_d is affected by biases in the CER retrieval, these are different to the CER biases alone (and in some cases may offset each other; Painemal and Zuidema, JGR, 2011). For marine stratocumulus clouds, the N_d retrieval appears to be surprisingly accurate (Gryspeerdt et al, 2022)."

3. I think it would be fine just to state that you are stratifying by LWP and focussing on CER sensitivity (without a focus on the RFaci). I would still suggest you consider N_d for future studies. There are easily available N_d products that might be useful (such doi:10.5285/864a46cc65054008857ee5bb772a2a2b and https://catalogue.ceda.ac.uk/uuid/cf97ccc802d348ec8a3b6f2995dfbbff).

Answer: Thank you for this valuable comment. We will consider N_d for future studies. In this paper we keep the sentence "the current study focuses on understanding effects of different parameters on CER sensitivity to aerosol rather than the application to determine RF_{aci}" (Line 193-194) as it is.

4. Also, it seems like if the GDM doesn't detect significant relationships for regions smaller than 9x9. When compared to the 4x4 region recommended by Grandey and Stier (2010), might this suggest that the results in this work are due to a misleading spatial covariation? Naturally, the GDM depends on spatial variability, so cannot easily operate on small regions (as I understand it) and there are other benefits to using it. I am not suggesting that this invalidates your results or that you have to do a lot of extra work, but I think this is an important aspect that should be discussed in the paper so it is clear to further potential users of this method.

Answer: Thank you for this comment. We have added the following text at the end of Discussion (lines 811-816): "As regards large regions: Grandey and Stier (2010) recommend 4° x 4° as the largest size and "if data exist at higher gridded resolution the possibility of analyzing data at this higher resolution should be seriously considered." In this study the resolution of MYD08 data used is 1° x 1°, the GDM doesn't detect significant relationships for regions smaller than 9° x 9° due to insufficient samples. In the future, higher resolution data can be used for GDM by controlling the size of the study area to be less than 4° x 4°."

Minor points

1. L68 - RF_{ari} is defined before RF

Answer: We have removed the definition of RFari before RF.

The text in the Introduction (lines 69-71): "Aerosol particles affect climate by their interaction with radiation (aerosol radiation interaction, ari) which exerts a radiative forcing (RFari) on the Earth'energy budget which results in rapid adjustments of global mean atmospheric quantities such as temperature." has been changed to "Aerosol particles affect climate by their interaction with radiation (aerosol radiation interaction, ari) which exerts a radiative forcing on the Earth energy budget, which results in rapid adjustments of global mean atmospheric quantities such as temperature."

2. L89 - The 'cloud lifetime effect' is less used today, as it is not clear a cloud lifetime is really involved (see the IPCC AR5 chapter on clouds and aerosols). I think you could just say 'are sometimes referred to', if you want to keep the terms in.

Answer: done. The text in the Introduction (lines 91-93): "These two effects of aci are also referred to as the cloud albedo and cloud lifetime effects (Quaas et al., 2008)." has been changed to "These two effects of aci are sometimes referred to as the cloud albedo and cloud lifetime effects (Quaas et al., 2008)."

3. L254 - Christensen - this paper also doesn't seem to show up in the references.

Answer: We apologize for the misspelling the name. The paper by Christensen et al. is included in the references and the name Christensen has been corrected in the text (lines 260-263): "… is based on reports by Christensen et al. (2017) and Varnái and Marshak (2009), rather than 0.6 used by Brendan et al. (20062005), who used MOD06 Collection 04 products. Christensen et al. (2017) used MOD06 C6 data (1km x1km) and reported that…".

Christensen, M. W., Neubauer, D., Poulsen, C. A., Thomas, G. E., McGarragh, G. R., Povey, A. C., Proud, S. R., Grainger, R. G.: Unveiling aerosol-cloud interactions - Part 1: Cloud contamination in satellite products enhances the aerosol indirect forcing estimate, Atmos. Chem. Phys., 17, 13151-13164, 2017.

4. L274 - I don't think Liu, 2002b is the usual ERA5 reference

Answer: Done. We have deleted the reference.

5. L323 - I am not clear what 'dot files' are. I assume it is some kind of 2D(?)array representation?

Answer: The sentence has been corrected. The text (lines 325-327): "The data in the raster grid is transformed into dot files, each dot containing a value for the CER and for one of the influencing parameters x." has been changed to "The data in the raster grid is transformed into 2D point vector files, each point containing a value for the CER and for one of the influencing parameters x.".

6. L420 - Aerosol is likely a secondary control on CF, rather than the primary cause. CER is similar, with cloud depth (and hence LWP) being a more important factor in determining CER, rather than aerosol.

Answer: Yes, a large part of the correlation between aerosol and CF is thought to be due to effects other than aerosol-cloud interaction such as aerosol humidification. The text in lines 424-429: "The high values of the CER and CF over the ECS could be due to the dominance sea spray aerosol, the high hygroscopicity of which makes these particles very efficient CCN." has been changed to "The high values of the CER over the ECS could be due to the dominance of sea spray aerosol, the high hygroscopicity of which makes these particles very efficient CCN, which in this environment over ocean with high water vapor concentrations, results in larger CER."

7. L552 - While there is a strong correlation between AOD, CF and CTP, that is not good evidence of an aerosol effect (Quaas et al., ACP, 2010; Gryspeerdt et al., ACP, 2014c).

Answer: Yes, we agree. We have added the following text in the Sect 4.4 (lines 562-565): "Although there is a strong correlation between AOD, CF and CTP, this does not imply evidence of an aerosol effect (Quaas et al., ACP, 2010; Gryspeerdt et al., ACP, 2014)."

8. Results section - I would be a little careful in attributing causality to these results, as they show a correlation, but not evidence that the aerosol variation caused some change in cloud properties. This doesn't require much change, but I would watch out for cases talking about the 'effect' of AOD on cloud properties (e.g. L689) or 'influence' (e.g. L787), as it is not clear there is actually an impact of aerosol on cloud properties from these results.

Answer: Following these comments, we have reorganized the following two sentences and added some words in the Discussion.

The text in lines 698-702: "The data in Fig. 10 also show that the explanatory power is largest for the combined influence of AOD together with other factors, and is somewhat larger than the influence of AOD alone (Table 6) for all 5 cloud parameters." has been changed to "The data in Fig. 10 also show that cloud parameters are more sensitive to the combination of AOD and a meteorological parameter than to AOD alone (Table 6)."

The text in lines 796-798: "The factor detector analysis (Section 4.6.1) shows that over the ECS, AOD has the largest influence on cloud parameters, as indicated by the large and statistically significant q values." has been changed to "The factor detector analysis (Section 4.6.1) shows that over the ECS, cloud parameters are most sensitive to AOD, as indicated by the large and statistically significant q values."

The text in lines 809-811: "Moreover, it should be noted that although the results show correlations, they do not provide evidence that the aerosol variation indeed causes some change in cloud properties." has been added in the Discussion.