

Interactive
Comment

Interactive comment on “The contribution of extratropical cyclones to observed cloud–aerosol relationships” by B. S. Grandey et al.

B. S. Grandey et al.

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We would like to thank all three referees and the editor for their helpful comments. We have made changes following their comments, improving the quality of the manuscript. We have also conducted further analysis, and corrected two errors in our previous analysis. Below, we explain the correction to the previous analysis, introduce the motivation for the further analysis, respond to the comments (*italicised*) in turn and provide a summary of the changes we have made to the manuscript.

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Corrections to previous analysis

While conducting further analysis in response to the comments, two errors were found in the original analysis:

1. The storm-centric and all-conditions data were erroneously duplicated. This does not appear to have affected the composites, slopes or correlations. However, the one-sigma standard errors and t-test p-values would have been smaller than they should have been due to the erroneous doubling of the effective sample size. This has now been corrected.

In the process of conducting a corrected version of the analysis, the corrected non-duplicated data were reshuffled. This shuffling is a random process. Therefore, the shuffled dataset used in the updated manuscript is different from the one used previously.

2. Despite the data duplication, it appears that the one-sigma standard errors for the $T_{top-\tau}$ slopes were miscalculated to be much larger than they should have been. Repeating the analysis using updated software has revealed smaller standard errors. This causes most of the all-conditions and storm-centric non-shuffled slopes to become significant where they were previously insignificant (Fig. 5a,e). The $f_{c-\ln \tau}$ standard errors are affected to a much smaller extent, impacting the error weighted means slightly. The updated standard errors and p-values appear to be in agreement for the following software combinations:

Operating System	Python SciPy	Python NumPy
Ubuntu 12.04.3	0.9.0	1.6.1
Mac OSX 10.8.4	0.10.1	1.6.2

A t-test two-tailed p-value threshold of 0.05 is now used for significance testing rather than the two-sigma threshold used previously. The t-test has the advantage that the sample size is taken into account as degrees of freedom.

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The manuscript has been updated to reflect the corrected analysis. The only major change is the statistical significance and standard errors of the $T_{top-\tau}$ slopes.

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Further analysis

Following the comments, we have conducted further analysis. The question asked in the introduction can be approached from at least two perspectives:

1. First, the contribution of ω to observed cloud-aerosol relationships can be investigated at each individual grid box in the storm-centric domain. This is the approach we took in our original analysis. This has been now clarified in the revised manuscript.
2. Second, the combined contribution of ω and storm spatial structure to observed cloud-aerosol relationships across the domain can be investigated. We have performed further analysis to investigate this, and the results have been included in the revised manuscript. Tables 2 and 3 have been added.

The second approach supplements the first. The conclusions, namely that only a small component of the relationships observed in the MODIS data can be explained, are somewhat similar for the two approaches.

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Response to Referee #1

This study investigates the relationship between satellite-retrieved aerosol optical depth (AOD) and cloud properties, namely cloud fraction (CF) and cloud top height (CTH). Previous studies have found a relation between e.g. satellite-retrieved AOD and CF as well as with CTH, which could be either due to retrieval artifacts, co-variation between AOD, cloud fraction and other meteorological variables (such as wind speed) or genuine aerosol-cloud interactions. More precisely, the authors examine if there is an influence of the strength of extratropical cyclones, and the location relative to the storm center, on the relation between AOD and CF/CTH. As a measure of extratropical cyclone strength and structure, the 850 hPa relative vorticity from ECMWF re-analysis is used.

The manuscript is in general well-written and the topic should be of high interest for ACP readers. Although the conclusion of the study is more or less a “non-finding” - there is no strong relation between the strength of the relative vorticity, AOD, CF and CTH – I still think the results are worth publishing, as they give further insights into the reasons behind the observed correlation between AOD and CF/CTH. However, there are a few clarifications, especially regarding the method, that need to be done before the manuscript is ready for publication.

Thank you for your comments. We have added further clarifications to the method as you suggest. Below, we outline our response to each of your comments.

General comments:

*- Title: I find the title imprecise. Maybe something like “The contribution of *the strength and structure* of extratropical cyclones to observed cloud–aerosol relationships” would be better?*

This is a good suggestion. We have changed the title accordingly.

- Abstract: Similar as the title. Shouldn't it be something more like “it seems plausible

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*to hypothesise that *the strength and structure* of extratropical cyclones may drive relationships between cloud-related properties and AOD”?*

Thank you for the suggestion. However, we have decided to leave this sentence as it is because this sentence is general (rather than referring to our specific method which focuses on strength and structure). We have added more mention of strength and structure after this in the updated abstract, in addition to changing the manuscript title.

- Introduction, p. 11974, line 2: It would be nice to clarify already here that you are looking at CF and CTH as cloud variables.

We have changed ‘between aerosols and clouds’ to ‘between τ and f_c and between τ and cloud top temperature (T_{top}), a measure of cloud top height’.

- Introduction, p. 11974, lines: 5-7: I think the motivation to this question could be a bit clearer. Why would the position relative to the storm centre be important? Because of the wind velocity? But the (local) cloud cover, and other cloud properties are not really governed by the wind speed? Why 850 hPa relative vorticity? “Many studies have shown that extratropical cyclones and fronts are major drivers of large-scale cloud-related properties.” How good is the (re-analyzed) relative vorticity as an indicator of extratropical cyclones and fronts?

Thank you for encouraging us to think about these questions. At 11974.5 we have added ‘The storm relative vorticity is used as a measure of storm strength, while the position relative to the storm centre allows storm spatial structure to be accounted for.’

TRACK has been used successfully on many occasions to perform objective feature tracking of extratropical cyclones using 850 hPa relative vorticity (see publications by Hodges). Further information, including mention of uncertainties and a new reference to Hodges et al (2011), has been added to the method section.

- Method, p. 11974, line 20-22: Did you do any filtering of the 850 hPa relative vorticity to remove weak, stationary or short-lived features?

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Yes. Only storms that persist for at least two days and move a distance of at least 1000 km are considered. This information has now been incorporated into an expanded discussion of the tracking and gridding methodology (see next point below).

- Method, p. 11974, line 23: storm-centric gridding methodology. Could be worthwhile to say a few more words about the method (so that you don't have to look up Grandey et al., 2011).

We have now added further details as you suggest.

- Method, p. 11975, lines: 18-20: Same here, a bit more explanation of the storm track regridding would be nice (so you don't have to look up Grandey et al., 2011).

Moved the 'all-conditions' paragraph earlier, so it immediately follows storm-centric gridding paragraph. Aided by the inclusion of more information about the storm-centric gridding methodology (see previous point above), the 'all-conditions' paragraph should now be understandable without the reader needing to refer to Grandey et al. (2011).

- Method, p. 11976, lines 3-8: Why $1 \times 10^{-5} \text{ s}^{-1}$ bins? If I understand things correctly, this "shuffling" will pair an AOD with a CF or CTH from a different grid box and/or time point, but with a similar relative vorticity? I think this should be clarified.

The bins are chosen to distinguish in an easy to understand way storm strengths varying between $1 \times 10^{-5} \text{ s}^{-1}$ and $15 \times 10^{-5} \text{ s}^{-1}$. The bins are deliberately narrow compared to the full range.

The shuffling pairs τ data with cloud property data from the *same* storm-centric grid box and a similar storm ω . Thank you for pointing out the potential misunderstanding. We have added further clarification that 'The shuffling occurs independently for each grid box, so the shuffled data remain functions of position in the storm-centric domain.'

- Method, general: What is the time resolution of the data? How are the MODIS and ERA-Interim data matched in time?

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Thank you for pointing out the omission of temporal resolution information. We have now added that the MODIS level 2 are at 5 minute resolution and the ERA-Interim at 6 hourly. The MODIS and ERA-Interim data are not directly matched in time. Instead, they are matched to interpolated storms (which have been tracked using ERA-Interim relative vorticity). This should now be clearer to the reader due to the addition of further discussion of the gridding methodology.

- *Results, p. 11977, lines 11-16: Isn't the decrease also due to (strong) precipitation?*

That is indeed a strong possibility. We have now added further clarification by changing 'frontal clearance' to 'frontal clearance via wet scavenging'.

- *Results, p. 11979, lines 3-9: Isn't it a bit odd that relative vorticity and CF are so poorly correlated? Does this really show that vorticity is a good variable to use as a measure of extra-tropical storm (cloud) characteristics?*

For the scales presented here, the variability in f_c does indeed appear to be larger than the influence of storm strength. (See also next point below.)

- *Results, p. 11979, lines 10-13: The dependency of CF and AOD on storm strength is however weak, especially for AOD. I think this should be acknowledged.*

We have now added 'However, it has also been demonstrated that the variability of these variables is large, as shown by the differences between the quartile composites.'

- *Results, p. 11981, lines 5-7: "They provide further support or the conclusion that the simplified description of the large-scale synoptics can explain relationships between AOD and f_c ." To me, it looks like large-scale synoptics (or rather relative vorticity) cannot explain the relationship between AOD and CF.*

We have now rewritten parts of this results section in an attempt to make things clearer. This sentence has been deleted.

- *Conclusions, p. 11985, lines 22-23: "This suggests that large-scale synoptic con-*

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ditions in the mid-latitudes are not a major driver of Ttop-AOD relationships.” This is a rather strong statement. It assumes that relative vorticity is a good indicator of the large-scale synoptic conditions.

This sentence has now been deleted. The paragraph has been rewritten following the corrected analysis.

Minor comments:

- P. 11977, line 20: “This storm-centric enhancement...” The enhancement is not really storm-centric, is it?

We have deleted ‘storm-centric’.

- P. 11978, line 5: I suggest changing “They are similar to those for the NA. . .” to “The results are similar to those obtained for the NA. . .”

We have adopted this change.

- P. 11979, line 22: I suggest changing “. . . is very slightly smaller” to “. . . is only slightly smaller”.

Again, we have adopted this change.

- P. 11981, lines 14-15: “Relatively cold cloud.” should probably be “Relatively cold clouds. . .”?

Thank you for the suggestion. Either option would be valid because, as a noun, ‘cloud’ can be either countable (count noun) or uncountable (mass noun). However, we were inconsistent due to the use of ‘warmer clouds’ in the following sentence (11981.18). We have decided to use the uncountable option, consistent with the rest of the paragraph. Hence we have now changed ‘warmer clouds’ to ‘warmer cloud’.

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Response to Referee #2

The study investigates the relationship between aerosol optical depth (AOD), cloud fraction (CF), cloud top height (CTH) and strength of extratropical cyclones. AOD has previously been found to correlate well with both CF and CTH. However, there is still an ongoing debate as to whether this correlation represents a physical process (a true aerosol-cloud interaction) or if it is simply symptomatic of other processes (e.g. large scale or local meteorology), or retrieval biases in the satellite products used to obtain estimates of these variables. The underlying question motivating this study is: can relationships between aerosol and cloud related properties be simply explained by the relative vorticity of extratropical cyclones. The rationale is that extratropical cyclones may drive both high AOD, CF and CTH values and the correlation observed between these variables can then simply be due to cyclone activity.

Overall, the manuscript is well written, easy to follow and will be of significant interest to the readers of ACP. I do however have some comments that I think the authors need to discuss before I recommend the paper of publication.

Thank you for your comments. We address your specific comments below.

Major comments:

1) In the conclusions, the authors state that large scale synoptic conditions are not the driver of observed CTH and AOD relationships but can explain a fraction of the CF and AOD correlation. These conclusions are therefore rather inconclusive in some way. When reporting a null-result like this, how certain can you be that your method would capture a relationship if there was one? I think some acknowledgment of this fact would be in place.

Thank you for pointing out our potential over-generalisation. We have deleted the sentence stating that large-scale synoptic conditions are not a major driver of $T_{top-\tau}$ relationships. In redrafting the manuscript, we have been more careful to talk about ω and

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position in the storm-centric domain (the description we use) rather than large-scale synoptic conditions in general.

2) In relation to comment 1), I miss a discussion on the uncertainties of the re-analysis product. All atmospheric state variables will include some degree of uncertainty. How could this uncertainty affect the null result? Also, what is the time lag between the MODIS overpass and the time of the analysis product? The temporal resolution of the ERA-Interim data is every 6 hours so in some regions there could be a significant temporal difference between the re-analysis and the satellite overpass? A discussion regarding this would benefit the paper I think.

Thanks for the suggestions. We have added more information about the storm location and reanalysis field uncertainties in an updated extended explanation of the tracking and gridding methodology (see next point below). We have now also included information about the temporal resolution of the datasets and have mentioned that the tracked storms are interpolated to match the MODIS overpass times.

3) Having read Grandey et al. (2011) it is easier to follow the Method section of the paper. But perhaps you could be a bit more detailed in the description of the method so that paper is more easily followed without looking up that paper? I got stuck a few times on the gridding methodology.

We have now added further details as you suggest.

4) I think the authors need to acknowledge that the approach can only be used in the mid-latitudes. So even if the cyclone activity could explain the observed relationship, it could not explain the correlation in the sub-tropics and the tropics, where the correlation between AOD, CF, and CTH have been found to be high as well? That we have to use different plausible mechanism (meteorology, satellite biases, etc.) to discard the perhaps true aerosol-cloud interaction, at different parts of the globe I think is a bit troublesome. Perhaps it is not a problem since the paper does not report cyclone activity as a major explanatory factor of the observed correlations, but still, perhaps

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you can discuss the implications of this a bit more.

Thanks for the suggestion. We have added an addition paragraph to the introduction: 'It is possible that the causal mechanisms driving cloud–aerosol relationships may vary between different regions. This paper focused on relationships in the midlatitude storm tracks.'

5) Of the amount of original data used (2002-2007), how much is left after processing and the storm-centric approach? There must be some limitation to the number of cyclones passing during the period of study and the number of matching MODIS overpasses as well? Is it really 5 years of continuous data used in the paper?

There is indeed a limitation here. Table 1 lists the number of cloud– τ pairs per storm-centric 200 km \times 200 km grid box for different 850 hPa relative vorticity (ω) ranges over the NA.

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Response to Referee #3

Review: “The contribution of extratropical cyclones to observed aerosol-cloud relationships” by Benjamin Grandey and coauthors Stier, Grainger and Wagner.

The paper describes an analysis that seeks to explain the relationship between aerosol optical depth (AOD) and cloud properties (fraction and cloud top height) in the vicinity of midlatitude cyclones as being due to meteorological covariation. To do this they use cyclone compositing approach whereby many meteorological, cloud and aerosol fields are composited based on distance and direction from storm center. I was quite confused on reading the paper about what exactly the authors have done and what hypothesis they are really trying to test. I think that the manuscript will be useful if the authors can make some efforts to improve clarity in the description of the methodology. As they will see below, I am confused about practically every aspect of their analysis, and I hope they can mitigate this in a revised version.

Thank you for your comments and for highlighting that the manuscript had the potential to be confusing. We have endeavoured to improve this in our updated manuscript. We have added clarification to the method, reduced imprecision when discussing the results and conclusions, and have added further subtitles to aid the reader.

I thought that they might test whether the mean/median fields of aerosol and cloud fields are spatially correlated (they are as far as I can tell), since this would in my view be evidence that meteorology is partly driving the correlations. It appears that is not the case. The authors are looking at aerosol-cloud correlations between storms at a given location from the center as a function of the cyclone strength. So as far as I can tell, the authors are testing whether cyclone strength may help drive correlations, not whether the existence or otherwise of a cyclone can produce AOD-cloud covariation. Thus the title is incorrect and should reflect what is being tested, which is not what I first thought was being done.

To illustrate, imagine I have a ground site and I want to examine whether cyclones are

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helping to explain the correlation between AOD and cloud cover I see. I notice that when the winds are strong there is more AOD, the cloud tops are higher and there is more cloud because a cyclone is passing. This “effect” is not what the authors are testing here, unless I’m mistaken. That needs to be carefully spelled out.

Thank you for the suggestion to perform further analysis of the spatial structure. We have now performed further analysis.

That said, after the third re-read I think I understand the key question they are setting out to test (Line 5, P 11974): “Can relationships between aerosol and cloud-related properties be explained by considering simply the relative vorticity of extratropical cyclones and position relative to the storm centre?”. I think they mean “does the correlation between cloud fraction [or temperature] and AOD at a given location relative to the cyclone center change with cyclone strength?”

So I am stumped when it comes to understanding what is plotted in Figure 3. Shouldn’t there be one figure for each of the vorticity bins used? Or does this figure not take into account the cyclone strength? Everything was headed toward me expecting to see correlations as a function of vorticity but they never came. I have to admit that I really lost track of what the authors are doing at this point. As such, the paper needs some serious work to improve readability and precision about what’s been done.

In light of your comments, which highlight the potential lack of clarity in our original manuscript, we have endeavoured to improve the readability of the manuscript, as mentioned above. We hope that the updated manuscript is a lot more clear.

Other comments:

There are problems with the abstract. From its reading, it is actually impossible to figure out what the authors did that led to their conclusion that large scale synoptic conditions do not drive cloud top temperature-AOD relationships. Indeed, I’m not even sure I understand what such a statement means. The language is not specific enough

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and this leads to confusion. Just stating that a “storm centered approach” is used is not enough. I can use a storm centered approach to conclude the opposite: for example, if one looks at the median fields of AOD and cloud top temperature, they are correlated. High AOD values are found in the high winds surrounding the cyclone, and this is where the highest cloud fractions and coldest cloud tops are found. Based on this, one would conclude precisely the opposite of what is stated in the abstract, namely that synoptic conditions so drive relationships between cloud top temperature, cloud cover and AOD.

Thank you for your suggestion. Our updated conclusions are more precise. We have also edited the abstract to be more precise and to include more methodological information.

How much more data is in the “all conditions” composites as opposed the ot the “storm-centric” ones? This would appear to be important if the cyclone domains take up more than 50% of the area. In a recent study [Bodas-Salcedo, personal communication] the storm-centric dataset is approximately half of the total. It would be better to take the storm-centric data and the REMAINING data as the two datasets to make Figs. 3 and 5.

Thank you for the suggestion. However, in our case, we have deliberately constructed the all-conditions composites to represent average conditions (i.e. they are blind as to the large-scale synoptics) rather than storm-free conditions. The volume of all-conditions is very similar to the volume of storm-centric data.

Abstract: How does covariation drive a relationship? What exactly does this mean? I don't really know what the authors are setting out to do.

Our updated abstract now starts ‘Meteorological conditions may drive...’.

What is a “statistically-robust explanation”. Do you mean a significant correlation?

Thank you for seeking clarification. We did indeed mean statically significant. This sentence has been deleted in our updated abstract.

Response to Editor

The editor provided some comments when we submitted our initial manuscript to ACPD. We acted on the minor comments before the manuscript was published in ACPD, so they will be omitted here. However, we decided to consider the science comments more fully when we had received the referee comments.

Storm-centric composites of aerosol optical depth, cloud fraction, and cloud-top temperature obtained from MODIS and ERA-Interim data are displayed for the North Atlantic sector. Supplemental material includes the South Atlantic sector. The composites are stratified by central vorticity, with roughly equal numbers of moderate and strong storms. Also, the upper and lower quartiles are displayed alongside the median composite.

The study has been done carefully, the text reads well, and the figures are colorful and high-quality. While the results demonstrate a clear role for extratropical cyclones in organizing clouds and aerosol over the ocean, it is unclear from the results shown that the paper's objective has been achieved, viz., to elucidate how these weather systems modulate cloud-aerosol relationships. This question is addressed by constructing storm-centric maps of regression slope & correlation, and comparing to maps of "all conditions" data.

*The impression from Figures 3 and 5 is that, while enhancements of slope and correlation in the storm-centric composites are modest at best, there's clearly a **pattern** obtained in the storm-centric composite that is not seen in the "all conditions" composite. It would be worthwhile to explore the underlying question more deeply. These results suggest that modulation of cloud-aerosol relationship might be found.*

The paper is of modest length, and can easily accommodate more text and figures. Suggestions for additional graphics include (i) map of storm centers, (ii) composite map of meteorological variables from ERA-Interim relevant to aerosol formation and cloud-aerosol relationship, such as relative humidity, wind speed, vorticity (for frontal

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boundary) and ozone (as a passive tracer in LT), (iii) schematic of MODIS pixels and analysis grid boxes, showing how calculation of box-average quantities is done.

Please address minor comments below, whereupon the paper will be acceptable for publication in ACPD. Optional science comments are appended for consideration by the authors at any stage of the review process.

[Minor comments omitted here.]

SCIENCE COMMENTS (optional)

The paper alludes to several ways that extratropical cyclones might modulate cloud-aerosol relationships, but does not delve into any of them. The intent of the paper evidently is not to address processes in extratropical cyclones, but simply to show how storms organize the observed properties and relationships. This goal is too modest. There are two ways that the paper can be improved:

SC.1: Expand the scope to address how the meteorological processes affect the observed properties and relationships. Separate questions relating to the observability of aerosols (cloud obscuration & retrieval error) from genuine meteorological effects (hygroscopic growth, precipitation scavenging, surface wind enhancement, large-scale subsidence, air-mass origin). Take a subset of these processes where observability of aerosols is not an issue, and investigate how the meteorological variable affects the observed properties and relationships.

SC.2: Alternatively, focus in greater detail on the statistical aspects of how the extratropical cyclone contributes to the observed properties and relationships. A good way to do this would be to begin with a cloud-aerosol relationship from a previous study, or as you might obtain from the "all conditions" composite, then dissect this relationship by showing where, within the meteorological disturbance, the data points are located that describe the relationship. If, for example, the observed relationship is stronger in one region of the storm than another, this should become apparent when the data points

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unique to that region are highlighted.

Of these two suggestions, the second is more amenable to display; additional graphics could be created from the analysis already done.

Thanks for your interest in ACP, and for submission of a high-quality manuscript.

Thank you for suggesting that we perform further analysis, and for your specific suggestions about how we could do that. Our further analysis has investigated the role of the spatial structure ('pattern') in more detail. Although we have not directly adopted SC.1 or SC.2 in the manuscript, we think that they are good suggestions and should be recorded here. SC.1: We have not looked directly at local meteorological processes, and consideration of local meteorological variables is mentioned as a basis for possible future work in the conclusions. We encourage other researchers to look at this in more detail if they interested. SC.2: Figures 3 and 5 (updated) show that statistically significant relationships for the shuffled data are sometimes found near the centre of the storm. This is mentioned in the results section of the updated manuscript.

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Summary of changes to revised manuscript

Title and authors

Title changed to 'The contribution of the strength and structure of extratropical cyclones to observed cloud–aerosol relationships'.

BSG's additional affiliation: deleted 'now at:'.

Abstract

Substantially edited. More information about the method has been included.

Introduction

11973.22: added 'It is possible that the causal mechanisms driving cloud–aerosol relationships may vary between different regions. This paper focuses on relationships in the midlatitude storm tracks.'

11974.2: 'between aerosols and clouds' -> 'between τ and f_c and between τ and cloud top temperature (T_{top}), a measure of cloud top height'.

11974.5: added 'The storm relative vorticity is used as a measure of storm strength, while the position relative to the storm centre allows storm spatial structure to be accounted for.'

Method

Adding subsection titles to aid reader.

11974.12: 'MTDATML2' -> 'MYDATML2'.

11974.13: added 'These level 2 data have a temporal resolution of 5 minutes.'

11974.20: '1.5° × 1.5° ERA-Interim' -> '6 hourly 1.5° × 1.5° ERA-Interim'.

11974.21: deleted '(ω)'. Limiting ω to refer to the relative vorticity of the storm, rather than 850 hPa relative vorticity in general.

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11974.25: added further details of tracking and gridding methodology.

11975.11: ‘median value’ to ‘median of the positive data’.

11975.18: moved paragraph earlier, to 11975.7. Added inverted commas around first mention of ‘all-conditions’.

11975.24: added ‘The question mentioned in Sect. 1 is considered from two different perspectives in this paper. First, the contribution of ω to observed cloud-aerosol relationships is investigated at each individual grid box in the storm-centric domain. Second, the combined contribution of ω and storm spatial structure to observed cloud-aerosol relationships across the domain is investigated. The method used to investigate the former is outlined below; the method for the latter is discussed in the next section.’

11975.24: merged sentences to ‘Regression slopes and correlations of the cloud-related properties versus τ are calculated at each $200\text{ km} \times 200\text{ km}$ grid box in the domain for the all-conditions data and for the storm-centric data.’

11975.27: added ‘Only positive aerosol and cloud data greater than zero are used.’

11976.1: ‘ τ ’ -> ‘ $\ln \tau$ ’.

11976.11: ‘Because this shuffling occurs within narrow ω ranges, the shuffled cloud and τ data remain functions of ω . So the calculated relationships between shuffled τ and the cloud properties represent the synoptic component which can be explained by ω and position in the storm-centric domain.’ -> ‘The shuffling occurs within narrow ω ranges, so the shuffled cloud and τ data remain functions of ω . The shuffling occurs independently for each grid box, so the shuffled data remain functions of position in the storm-centric domain. Therefore the calculated relationships between shuffled τ and the cloud properties represent the synoptic component which can be explained by ω for a given position in the storm-centric domain’

11976.19: moved paragraph earlier, to 11975.7.

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11976.23: adding section explaining the further analysis to investigate the combined contribution of storm strength and storm spatial structure to cloud–aerosol relationships across the storm-centric domain.

Results and discussion

Adding subsection titles to aid reader.

Results and discussion: aerosol optical depth

11977.13: ‘frontal clearance’ -> ‘frontal clearance via wet scavenging’.

11977.20: deleted ‘storm-centric’.

Results and discussion: cloud Fraction

11978.5: ‘They are similar to those for the NA’ - > ‘The results are similar to those obtained for the NA’.

11979.12: added ‘However, it has also been demonstrated that the variability of these variables is large, as shown by the differences between the quartile composites.’

11979.22: ‘is very slightly’ -> ‘is only slightly’.

11979.22: ‘0.163 compared to 0.165’ -> ‘0.156 compared to 0.159’.

11979.24: ‘data for each grid box are next shuffled within each vorticity range’ -> ‘data are next shuffled within each grid box and vorticity range’.

11979.25: ‘slopes are’ -> ‘slopes for each grid box are’.

11980.3: ‘The magnitude of the EWM is larger than the one-sigma error. However, it is two orders of magnitude smaller than the all-conditions EWM for the non-shuffled data and is also opposite in sign, -0.001 compared to 0.165 .’ -> ‘The magnitude of the EWM is smaller than the one-sigma standard error. Furthermore, it is more than two orders of magnitude smaller than the all-conditions EWM for the non-shuffled data.’

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11980.11: ‘which is also larger than the all-conditions EWM of -0.001 ’ -> ‘which is an order of magnitude larger than the shuffled all-conditions EWM’.

11980.13: ‘0.163’ -> ‘0.156’.

11980.14: deleted sentence starting ‘Although the simplified...’.

11980.17: ‘Similar conclusions can’ -> ‘Similar observations can’.

11980.21: ‘ -0.002 ’ -> ‘ -0.001 ’.

11980.24: ‘ -0.011 ’ -> ‘ -0.010 ’.

11980.26: deleted sentence starting ‘Again, this suggests...’.

11981.5: deleted sentence starting ‘They provide further support...’.

11981.1: moved sentence to 11979.9.

11981.7: added ‘In this section, the contribution of ω , a measure of storm strength, to f_c - $\ln \tau$ relationships at each location in the storm-centric domain has been investigated. For the shuffled storm-centric data, statistically significant EWM regression slopes have been found, suggesting that storm strength may, to some extent, drive relationships between f_c and τ via meteorology. This effect appears to be stronger near the centre of the storm. However, the relationships for the shuffled data are much weaker than those for the non-shuffled data, demonstrating that the contribution explained by storm strength is only a small component of the observed relationships.’

11981.7: added section discussion results of further analysis, looking at combined contribution of storm strength and storm spatial structure.

Results and discussion: cloud top temperature

11981.9: ‘show’ -> ‘shows’.

11981.18: ‘warmer clouds’ -> ‘warmer cloud’.

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11982.18: sentence starting ‘As mentioned...’ moved to 11979.19.

11983.15: moved sentence to 11982.11.

11982.12–11983.26: edited following correction to previous analysis.

11983.26: added section discussion results of further analysis, looking at combined contribution of storm strength and storm spatial structure.

Conclusions

11984.22: ‘cause’ -> ‘drive’.

11985.6: added ‘This question has been considered from two perspectives. First, the contribution of ω to observed cloud-aerosol relationships has been investigated at each individual grid box in the storm-centric domain. Second, the combined contribution of ω and storm spatial structure to observed cloud-aerosol relationships across the domain has been investigated.’

11985.7-17: edited to encompass further analysis.

11985.18-25: edited following correction to previous analysis.

11986.3: ‘by position’ -> ‘by both position’.

Acknowledgements

Added ‘BSG acknowledges funding from the Singapore National Research Foundation (NRF) through the Singapore-MIT Alliance for Research and Technology (SMART) Center for Environmental Sensing and Modeling (CENSAM).’

‘Thanks are due to Joanna Eberhardt who commented on an earlier draft of this paper.’ -> ‘Thanks are due to Joanna Eberhardt, three anonymous referees and the editor for providing helpful comments.’

References

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Added Grandey and Stier (2010).

Added Hodges et al (2011).

Tables

Table 1: updated following correction to previous analysis. Also changed to number of f_c - τ data pairs specifically used in the linear regression analysis. The regression analysis requires both f_c and τ to be positive, so $f_c = 0$. and $\tau = 0$. are excluded.

Table 2: added.

Table 3: added.

Figures 1–5: updated following correction to previous analysis.

Figures 3 and 5: changed significance threshold to a t-test two-tailed p-value threshold of 0.05.

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