Response to Reviewer #1

The authors evaluate the semi-direct effect of dust aerosol over low-levels clouds in the North Atlantic Ocean with CloudSat radar and CALIOP lidar observations. They show that the summer free-tropospheric dust layer overlying low clouds induces a shortwave heating response which strengths the boundary layer inversion, consistent with previous studies of biomass burning aerosols (positive semi-direct effect). But also, due to the large dust particle sizes, a significant longwave warming response. The dust-induced longwave warming dominates the heat budget and leads to overall less cloud-top cooling (~10%) which reduces cloud cover (novel negative semi-direct effect).

The paper presents an interesting new finding about the longwave semi-direct effect of aerosols on low-level clouds. The authors discuss first the response of cloud cover, and then the response of heating rates, to dust optical depth, dust layer geometric thickness, and dust-layer base altitude. They use a radiative transfer model (SBDART) to quantify the cloud sensitivity to perturbations in each of these aspects, as well as meteorological quantities.

Overall, the results are interesting. However, I have some confusion about the methods used for the analysis, particularly the approach taken to remove confounding effects of meteorology and how to separate the impact of each metric of the dust layer.

We thank the reviewer for showing interest in our manuscript and for their thoughtful and constructive comments. We have responded to the points raised and incorporated the valuable suggestions, which have significantly improved our manuscript. We address (in blue font) each of the reviewer's comments (in black font), along with suggested changes in the manuscript, which are shown in italics.

Major Comments

1. Mainly, why have you chosen to split up the data into so many "categories" and compute the partial derivatives of interest as differences between categories instead of computing them with a multiple linear regression?

Thanks for the comment. Our primary goal was to isolate the sensitivity of cloud-top radiative cooling to different dust-layer properties (optical depth, geometric thickness, base height), cloud properties, and thermodynamic structure. We explored multilinear regression in the early stages of our analysis. However, because some of the dust-layer properties are correlated, accounting for the interaction terms (that is, cross-correlation terms between the correlating variables) significantly affects our interpretations. In addition, there are likely nonlinearities in the relationship between low-level cloud response and dust properties that are beyond the scope of this study, as well as the negative impacts of potential outliers, making it unsuitable to use a multilinear regression approach. Therefore, to avoid these problems and to better control the co-variability in the dust-layer properties, we binned the data into categories. Thus, our categorical method allowed us to systematically vary one parameter set at a time, while holding the others fixed, enabling us to directly quantify their related cloud response. In addition, this also allows us to carry out corresponding radiative transfer calculations by varying combinations of aerosol, cloud, and thermodynamic structure and to quantify their sensitivities. To clarify, we have added the following text in the manuscript.

"We used this categorical method to account for potential cross-correlation between predictors (such as dust geometric thickness, base height, and optical depth), which would otherwise require calculating interaction terms in a multilinear regression, and to avoid potential nonlinearities and negative impacts of outliers, allowing us to isolate the impact of each parameter while maintaining statistical robustness through sample size criteria."

2. For the meteorology, why do you choose to analyze each 5×5° box separately rather than e.g. extending a cloud controlling factor analysis to include the dust properties of interest? Wouldn't this better remove any confounding meteorological factors, rather than just looking at some "small area"? Because surely there will be some variability still within 5×5° boxes.

As done in previous studies, we chose 5 ° × 5 ° grid boxes to minimize the influence of cloud-controlling factors on our analysis. This is because our study focuses on instantaneous interactions between clouds and aerosols, and as such, this approach minimizes any potential memory that may be introduced into our statistical relationship due to the influence of large-scale meteorology (Mauger and Norris 2007). In addition, choosing a larger domain of analysis implies comparison of cloud/dust characteristics of entirely different locations. For example, the eastern parts of the domain, often dominated by dust that is relatively near to source regions, compared to those which are dominated by long-range transport, far from sources, are the western part of the domain. Furthermore, underlying meteorological conditions such as sea-surface temperature, wind speed, etc., significantly differ from east to west. Therefore, confining our analysis within 5°×5° boxes helps reduce such stark changes. Finally, for meteorology variables in Section 3.5.2, we also adopt a similar approach to ensure consistency with other parts of our analysis

3. For section 2.2.2 why do you split just into "low" and "high" dust optical depth? Why not have a continuous variable for the DOD?

Thank you for the suggestions. We use these broad categories, in part, because DOD distribution often skews towards low DOD (indicating that clean-to-low DOD occur far more frequently than high DOD cases), and to have relatively sizable and equitable sample sizes to compare, we use low and high DOD as representatives of broad aerosol conditions. In addition, and similar to our response above, this approach also aids the interpretability of our results and avoids the potential outlier effect. Yes, we acknowledge that binning into "low" and "high" optical depth reduces the number of available datasets significantly. However, it was necessary to compare the influence of a dust layer of the same geometric thickness, having differing optical depth, so we can quantify the influence of geometric thickness independent of optical depth. Ideally, we would have taken more DOD bins; however, due to limited profiles, it was not possible to have enough samples available for each category and DOD bin to make a statistically significant average and comparison across categories. In addition, to quantify the influence of DOD. That said, the reviewer may be interested in Fig. S8, which used DOD from all available profiles for each geometric-thickness and dust-base combination to calculate the relationship between cloud fraction and DOD (see Fig. S8).

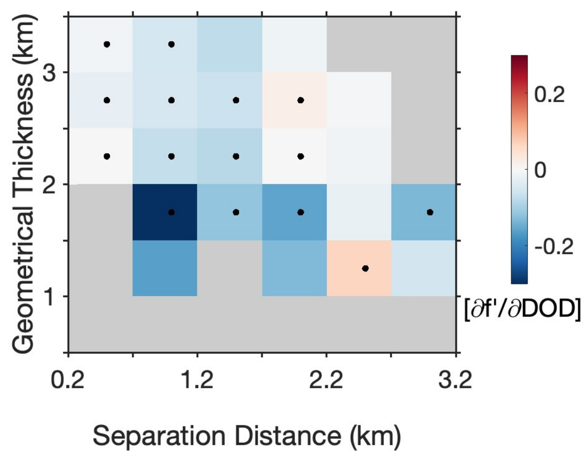

Figure S8: Change in *f'* per unit change in DOD as a function of dust base height and geometric thickness

4. If instead of making these artificial categories and thresholds you used the continuous variables, you probably would be able to retain more data and have more statistical significance (instead of throwing out so many profiles that don't meet the minimum 180 per category). If you are concerned that some of these relationships may be quite nonlinear, so a linear regression wouldn't be suitable, maybe you could consider something like ln (DOD) instead of DOD directly.

We appreciate this suggestion. Part of our motivation for categorization was to avoid the inherent cross-correlation among predictors such as dust geometric thickness, dust layer height, and optical depth. In addition, while previous studies have indeed used ln (DOD), such characterization is currently not clear for dust geometric thickness and dust layer height. In addition, we used a minimum sample threshold for each category to ensure statistical significance and representativeness, although some profiles were excluded. That said, we also consider other minimum sample thresholds and our selection of at least 180 per category does not significantly impact the conclusion presented in the paper. In addition, this methodology also simplified the associated radiative transfer simulations using SBDART, as the corresponding values of thermodynamic profiles, dust-layer properties, and cloud properties could be directly used to perform the sensitivity analysis.

Minor Comments

1: L101/102: I think you have swapped reference to Fig 1c and 1d. The figure caption says that 1c is showing change in cloud cover when all aerosols are present and 1d is for when only dust is present.

Thank you for pointing this out. We have corrected the figure references accordingly.

2: L170: How can the dust "coexist with" but also be "typically above" the cloud layer?

Thank you for pointing this out. We have revised the sentence to read: "frequently overlies." Our original use of "coexist" referred to co-occurrence within the same atmospheric column, not mixing of dust and cloud in the same layers. This has now been clarified and modified sentence reads

"We focused our analysis on the North Atlantic Ocean between May and August (2007-2017) when the SAL dust ***frequently overlies*** *low-level cloud layers"*

3: L548: Is this a good estimate? What if the clouds are not stratiform and pushing through the inversion layer?

Thank you for pointing this out. We agree that identifying the inversion layer based solely on cloud-top height has limitations, particularly if clouds are not strictly stratiform. However, mentioned, we defined the inversion top as the layer immediately above the cloud top, as our interest here is not in diagnosing the detailed inversion structure but in capturing the humidity that could entrained into the cloud layer and change cloud parameters. For this purpose, the simplified definition of the inversion top as the layer just above the cloud top (following Eastman and Wood, 2018) consistent. We have clarified this

"Though this is a simplified definition of the inversion top and may not fully apply to clouds with cumulus characteristics, here, we are mainly interested in moisture available for entrainment into cloud."

4: L1059/1065/1066: "LST" should be "LTS" for lower-tropospheric stability.

Thank you for pointing this out. We have corrected this.

Reference

Mauger, G. S. and Norris, J. R.: Meteorological bias in satellite estimates of aerosol-cloud relationships, Geophysical Research Letters, 34, 1–5, https://doi.org/10.1029/2007GL029952, 2007.

Eastman, R. and Wood, R.: The competing effects of stability and humidity on subtropical stratocumulus entrainment and cloud evolution from a Lagrangian perspective, J Atmos Sci, 75, 2563–2578, https://doi.org/10.1175/JAS-D-18-0030.1, 2018.