Reply to Anonymous Referee #2 review of the ACPD paper "Satellite observations of aerosols and clouds over southern China from 2006 to 2015: analysis of changes and possible interaction mechanisms" by N. Benas et al. The reviewer comments are included in italic.

Interactive comment on "Satellite observations of aerosols and clouds over southern China from 2006 to 2015: analysis of changes and possible interaction mechanisms" by N. Benas et al.

Anonymous Referee #2 Received and published: 6 November 2018

This paper synthesizes monthly-averaged satellite aerosol data from MODIS and CALIPSO, biomass burning emissions data from GFED, and cloud data from MODIS and CLARA-A2 to examine annual and seasonal trends for the South China region over the past decades. The purported goal of the study is two-fold (as stated on Pg. 2, Lines 9-13): 1) to analyze aerosol and cloud characteristics and changes, and 2) to investigate the possibilities and limitations of the synergistic use of this multitude of data for assessing aerosol and cloud interaction mechanisms.

Only three aerosol types are investigated from the CALIPSO dataset: Dust, Smoke, and Polluted Dust. This substantially limits the conclusions that can be drawn with regard to aerosol source attribution; although, the manuscript forges ahead and attributes changes in the decadal timeseries of aerosol optical depth large to changes in biomass burning, particularly residential energy sources (Pg. 5, Lines 14 and 18).

We demonstrate that CALIPSO observes significant negative changes in smoke and polluted dust AOD (which are consistent with MODIS observed changes) and that GFED carbon emission estimates also have strong decreases. These are two independent pieces of information suggesting that AOD changes are related to changes in carbon emissions from burning, which is furthermore supported by a number of studies referenced on page 5, line 19-20. Furthermore, we acknowledge the limitation in the CALIPSO level 3 aerosol data set that the reviewer mentions (page 2, lines 31-33) and the underlying reasons (page 2, lines 33-34), as explained in the discussion of the relevant paper. The validation of this data set and its use in other studies (see page 3, lines 35-38) shows that this limitation does not necessarily invalidate our conclusions. It rather limits the scope of the conclusions that can be drawn regarding the sources of the total aerosol load over the region, something that we have already acknowledged.

The authors then average the decadal cloud microphysical data by month to look at seasonal trends, and find no clear seasonal trend in aerosol optical depth even as there are pronounced increases in liquid water path, cloud fraction, cloud optical thickness, and effective radius during the November-December time period.

The reviewer has apparently not read the paper correctly: there is a clear (and strong) seasonal decrease in AOD in these months (see Fig. 3).

From these relationships, they conclude that the observed seasonal trends are inconsistent with the first and second aerosol-cloud indirect effects, but possibly consistent with the semi-direct effect (Section 3.3.2). Such strong conclusions are not supported by the underlying data, which are themselves highly averaged in both space and time. The highly-averaged nature of the data makes it hard to draw conclusions other than to say that the seasonal trend of one variable appears to *correlate with the trend of another variable or that one or more variables trend up/down slightly over time – such apparent correlations are neither causal nor attributive.*

Any study of long-term changes will have to use averaged data. We have used standard monthly-averaged satellite products, which are publicly available and have been used in many other studies as well. The statement of the reviewer implies that these monthly-averaged products make no sense and studies using these data are flawed. In our case, we do not find 'variables trending slightly up/down over time' but very strong changes over the decade studied. These strong changes cannot possibly be caused by the particular ways in which the data have been averaged.

No statistical tests are presented to quantify the robustness or strength of such correlations.

How can the reviewer state this? As written on page 4, line 14: 'Statistical significance of all calculated changes was estimated using the two-sided t-test.' The results of these tests appear in many parts of the manuscript: e.g., page 5 line 6 (AOD), page 6 lines 4-5 (liquid CFC and LWP), page 6 lines 33-34 (seasonal cloud property changes), page 7 lines 8-10 (geopotential height and surface pressure: no significant changes), page 8 line 9 (seasonal AOD), page 8 lines 21-22 (seasonal smoke/polluted dust).

Indeed, I find all of the authors' conclusions regarding the attribution of aerosol sources to biomass burning and their effects on clouds to be highly speculative. To quote the authors (Pg. 9,Lines19-21): "These results do not constitute evidence of any cause and effect mechanism, which cannot be proved based on observations only. They rather represent a contribution to the observational approaches in aerosol-cloud-radiation interaction studies, highlighting both the possibilities and limitations of these approaches." From reading this paper, I don't know what approaches are being referred to here. The approach employed seems to have been to take a bunch of Level 3 temporally-averaged and gridded satellite data products, plot them up, and draw strong, unsupported conclusions about aerosol-cloud interactions based on perceived annual or seasonal trends.

We have analysed observed changes in aerosols and clouds over southern China, and outlined possible aerosol-cloud interaction mechanisms that can explain these changes: not more but also not less. We have made a careful attempt to indicate limitations of an observation-based study. However, the reviewer characterizes the study as "highly speculative", then quotes a part where we acknowledge the limitation of our analysis in drawing strong conclusions, and then criticizes the study for drawing "strong conclusions". This is an obviously inadequate comment, that does not do justice to the study.

In my opinion, this paper does not represent a substantial contribution to scientific progress, which is the minimum criterion for the ACP scientific significance review criteria. As I do not see a path forward by which this manuscript could be revised to be a significant contribution, I recommend to the editor that this manuscript be rejected.

This review does not mention any concrete and specific flaw in our study. In contrast, it seems that the reviewer only glanced through the manuscript to reach an unsupported negative judgement. It is unfortunate that the lack of even a general suggestion for a possible way forward that could lead to any improvement in this study only corroborates this conclusion.