We thank the referee for the constructive review. Following are our point by point replies with the referee comments in italic.

General comments:

There are many (admitted) observational limitations, especially with respect the interpretation of aerosol type thus I recommend also to look at the fine-mode AOD of the MISR retrieval, which I placed on ftp ftp://ftp-projects.zmaw.de/aerocom/satellite/mis_v23/

We thank the referee for this suggestion and for providing the data. Using fine mode AOD as a better CCN indicator (compared to total AOD) will add robustness to our findings. We intend to include this analysis in a revised version of the manuscript.

It seems very promising to examine cloud property changes in regions where large changes in aerosol loading have occurred. However for that region an opportunity is missed by just exploring periods of a recent AOD decline (especially since 2012), whereas it was contrasted by a strong AOD increase before 2008. Thus opposing cloud property changes should have been observed, if there was an aerosol impact on clouds. Unfortunately even with these AOD changes the aerosol loading was quite high so that CCN concentrations may been already saturated with respect to droplet formation which in part explain the largely 'missing' first indirect effect. Overall, I like the paper but I am sometimes dismayed at the recognition of shortfalls without going into further detail. The data-consistency (e.g. CLARA) also is often a major handicap so that despite of significant data-analysis often there remain relatively little useful information to work.

We acknowledge the value of performing the same analysis for larger periods of time, in view of the known alternating signs of aerosol changes over this region. However, reliable satellite-based aerosol observations are not available before the 2000s. Additionally, to minimize the effects of interannual variability on the detected changes, the latter should be computed for relatively long periods (here we required at least 10 years). These factors render the combined examination of aerosol and cloud changes over larger periods practically unfeasible. The strong AOD decrease before 2008 that the referee mentions is not apparent in our analysis (Fig. 2), probably because the latter includes only two years prior to this switching point. However, we took the opportunity to examine clouds changes over a larger time period, offered by CLARA-A2. As the referee mentions, issues of long-term consistency in this data set reduce the availability of useful information. The remaining information, however, shows that the changes reported during the more recent years are considerably different (and in some cases opposing) than those based on earlier time periods (as shown in Fig. 6), suggesting a connection with the switch in the sign of aerosol changes in recent years.

The referee's remark on possible saturation in CCN concentrations is an important one that we plan to include in the relevant discussion.

The use of reanalysis data is an interesting aspect, and I just wonder if they the MODIS data assimilation in MACC actual changes in cloud-properties in the examined China regions are simulated. I suggest also not to look just at biofuel but also fossil fuel emission (trends) as alternate aerosol change indicator although working with actual AODf is probably best.

Comparisons of satellite-based aerosol and cloud data with corresponding CAMS data show consistent changes. However, in the case of CAMS this is due to data assimilation, and does not prove a successful model reproduction of aerosol-cloud interactions that occurred in reality.

Working with fine mode AOD is a constructive suggestion that we intend to incorporate.

Minor comments:

figures

Figure 1 biomass burning emissions are at a maximum in spring but AOD/ AODf reductions (of MISR see below) are largest in fall. I would also add seasonalities of MISR data (AOD, AODfine and AODnonsphere[dust])

From MODIS and CALIPSO we also get largest AOD reductions in fall and early winter (see Fig. 3). We plan to include MISR data in the revised manuscript.

Figure 2 MODIS data over land have limited accuracy and MISR data are there usually more accurate … although the reduced AOD in that region is not questioned. The component AOD type assignments of CALIPSO involve many assumptions and should not be overinterpreted.

We intend to include MISR data in the analysis. CALIPSO assumptions and issues are acknowledged, and they will be further emphasized in the framework of a more conservative interpretation.

Figure 3 the AOD reductions by MODIS and CALIPSO are max in October, for which GEFD changes can be hardly used to explain. Also consider that the CALIPSO 'smoke' in fact could be 'pollution'

As we explain in the manuscript, GFED does not provide a full picture of aerosol emissions (page 4, lines 37-38). Hence it should not be expected to explain all changes in AOD found from MODIS or CALIPSO. CALIPSO uncertainties will also be further highlighted, although possible misclassifications of the "smoke" subtype are already mentioned (page 5, lines 29-33).

Figure 4 cloud fraction is relatively high (ca 80%) so that the number of successful pixels in MODIS retrievals maybe very small and that CALIPSO will miss cases with strong pollution below clouds. Why is the water cloud phase dominant in the coldest seasons (that is not very intuitive). Also I would assume the largest cloud-fraction, COT and LWP during the monsoon season in summer, which the seasonality does not show.

Because of the high cloud fraction, there are indeed relatively few successful MODIS AOD retrievals. Therefore, we required only 10 daily AOD values to be present (compared to 18 for cloud properties) to accept a MODIS monthly-mean AOD for further analysis. On the relatively coarse grid scale of the MODIS products (1 x 1 degree) this is usually fulfilled. Similarly, the even much coarser (2 x 5 degrees) spatial resolution of the CALIPSO level-3 products also yields sufficient retrievals for the monthly means.

The monsoon season in summer is characterized by a larger fraction of high clouds with ice near the top, in particular convective clouds. In winter, low stratus/stratocumulus clouds prevail. In both seasons the cloud fraction is rather high, but one can imagine that persistent stratus will give a larger mean cloud fraction than intermittent convective systems. The seasonal cycle of cloud properties is retrieved very consistently from two independent satellite records, CLARA-A2 and MODIS, so we think it can be trusted.

Figure 5 the increase to LWP and cloud cover change have a strong local signature so I wonder if working with large regional changes is too simple. Even these annual averages display strong variations over time, so that I wonder if the identical 'trends' are significant. Also COT an REF retrievals are only possible for overcast conditions – so in that context tendencies in overcast cloud conditions would be of interest as well.

The LWP and cloud cover changes do show spatial variations, in particular with larger (positive) changes over land, and even slightly negative changes over sea. These patterns are consistent between CLARA-A2 and MODIS. We do not want to 'cherry-pick' exactly the grid boxes where we see the largest changes, and therefore we analyze time series of monthly-mean cloud properties averaged over the full region from both satellites, even if this might give somewhat weaker effects, because some parts with weaker trends are included. COT and REF retrievals are performed for cloudy pixels under the assumption that these are overcast. This will not always be the case, but we do not have sub-pixel information to determine that.

Figure 6 AVHRR over the years have different overpass time (trends may reflect in part daily cycles) and different sensors, so CLARA may not have the homogeneity needed for long-term trends. On the other hand the long time series is of advantage as (industrial) fine-mode AOD likely increased until 2008 and likely decreased since 2008 over China. So if there is a cloud property response I would expect a clear bi-polar response, which I do not see.

Orbital drift in NOAA satellites is important especially in the 80s and 90s. For the 10-year period examined in this study, data from NOAA-18 and NOAA-19 were used. Specifically, only the “primary” satellite was used in each month, meaning that when NOAA-19 data became available, NOAA-18 was not used any more. In this way any possible drift would be minor. The very good agreement with MODIS ensures that drift is not an issue for the 10-year period examined. For longer periods, depicted in Fig. 6, the effects of drifts are identified and acknowledged.

Figure 7 the largest cloud property changes (increases to all examined cloud properties!) are in December – not the month with the largest AOD changes (October). Also the largest AOD and AODf values of MISR in that region are in spring (consistent with your Figure 1)

Not all cloud properties have the largest change in December: liquid CFC has the largest increase in November. Apart from that, we agree that the largest aerosol and cloud changes don’t coincide, but we also claim that such a coincidence is not a prerequisite for possible interactions. Aerosol type and height relative to clouds are also important factors, and constituted the main reason for CALIPSO data to be included in our analysis. We also don’t see why the largest changes in AOD should coincide with the largest AOD concentrations. Large changes in autumn and large amounts in spring are not inconsistent.

Figure 8 the changes in October are likely pollution rather than smoke and the category of dust mixtures is too imprecise for me to be useful.

The most relevant part of aerosol type in this case is the optical properties and how these might affect interactions with clouds. We agree that attributing sources of aerosols based on their types is uncertain, but it is also of secondary importance in our case. The two main important points are (i) that the aerosols are absorbing and (ii) that they reside at a certain height, which is different in these two months. We will try to clarify this even more carefully in the revised version of the manuscript.

Figure 9 Wouldn’t it be better to show the histogram just for Oct and Nov when you focus on Oct and Nov changes. Actually I am bit uncomfortable just to focus on two individual months rather than on an entire season – in the context of meteorological variability

Yes, this suggestion is reasonable and we will incorporate it. The focus on two separate months is due to the different behavior in CALIPSO aerosol subtypes (Fig. 3) and corresponding differences in vertical changes in these months (Fig. 8). Our results suggest that these changes affected clouds, based on the same explanatory mechanism which manifested in two different ways depending on the relative positions of aerosols and clouds. These effects would be indiscernible, had we averaged these months or examined the season as a whole.

text

page 2 / 20 there are some issues with the total AOD, since the it is contaminated by larger dust particles, which do not contribute as smaller particles from wildfire and pollution to particle concentrations and potential CCNs. MODIS only offers to total AOD and there are issues with the accuracy over land (for which the data are applied here) and the opportunity to retrieve aerosol maybe be limited by the relatively high low cloud cover. And the aerosol typing of CALIPSO is only quantitative – especially for 'mixtures' and it is extremely difficult to distinguish between pollution and biomass aerosol types. I think the use of MISR data also should be included in order to include AOD trends, especially since MISR also addressed the AODf.

We acknowledge the issues in using AOD as proxy to CCN. Possible reduced retrievals due to high low cloud cover are treated by applying all the requirement thresholds in the estimation of monthly means, described in Section 2.4. Regarding the quantitative aspects of AOD from CALIPSO, our focus is primarily on their optical properties and how they can affect interactions with clouds. Inferring sources of aerosols based on their types is secondary and we will try to clarify this in the revised version.

page 2 / 40 the mentioning of the GFEDv4 data is an interesting concept to justify aerosol changes due to biomass burning, but I would argue that industrial emission reductions (talk to Steve Smith to provide his industrial emission data for that region) are much more likely to explain satellite observed AODf reductions in recent years.

Reductions in aerosols from biomass burning activities over this area in recent years is well documented in the literature, which we include in our study, and the GFED analysis confirms these previous findings in terms of both seasonal distribution and changes. We claim that the change in these activities explain only part of the reduction in total AOD, as is also obvious e.g. from Fig. 3 and the relevant discussion (page 5). We agree that reductions in industrial emissions may also play a role in explaining the overall AOD reduction, and it is a constructive idea to include some relevant analysis as the referee suggests. It should be noted, however, and will be emphasized in the revised manuscript version, that the main goal of this study is to examine possible aerosol-cloud-radiation interaction mechanisms manifested due to the AOD decrease, rather than explain in detail every aspect in this decrease over the region. These interactions should not be expected to correlate with the overall aerosol change reported, but depend on specific aerosol types, relative positions and overall conditions.

page 3 / 10 the MODIS data for (liquid) cloud cover is the most reliable property (though possibly biased by the overpass time and also dependent on the non-obscurance by higher altitude clouds). Details (LWC, Reff and COT) only refer to an overcast cloud subsample – so I wonder if also changes for overcast low cloud pixels can be addressed as well

It is true that MODIS COT, REFF and consequently LWP are retrieved for a subsample of cloud-detected pixels (e.g. cloud edges are excluded). In that sense, analyzing the fraction of "overcast"-only cloudy pixels is a useful addition.

page 3 / 15 I wonder how stable the CLARA data-set is, especially involving AVHRR data with drifting overpass time and different sensors.

The issues that the referee mentions are more pronounced during the first two decades of CLARA-A2 data. In fact it has been shown that CLARA-A2 data are stable from the 2000s onwards (see also Karlsson et al., 2017). We use data from the 10 most recent years of the data set, from afternoon satellites NOAA-18 and NOAA-19. For every month, the most recent satellite available was used. Hence, the rapid succession of satellites ensures that when their orbital drift could start compromising the stability of the time series, their data were already not being used in the analysis. This point will be included in the revised manuscript to clarify the issue.

page 3 / 30 consistent changes of different data is certainly a help but for clouds I wonder how independent the cloud retrievals (other than cloud cover) are.

We understand the referee's point. Since both CLARA-A2 and MODIS use the Nakajima-King principle to retrieve cloud optical thickness and effective radius, their retrievals are not completely independent. We will rephrase accordingly.

page 4 / 12 what is a pixel? (I assume a 1x1 lat-lon region), thus there are 50 pixels in the regions investigated region

Correct, it will be clarified.

page 4 / 20 domestic burning may be largest in winter … but what about industrial emissions as I also expect that mitigations (e.g. to electric vehicles) have lowered emissions?

Yes, industrial emissions may also have been decreased, and their seasonality pattern will be different. A relevant description will be included in the revision for completeness. However, these changes are not depicted in the GFED analysis, since they originate in a different aerosol source.

page 4 / 35 the aerosol typing of calipso should be taken with care. What does 'polluted dust' really tell you on fractions of dust and pollution? without a good distinction between pollution and smoke how to argue via biofuel (GFED) rather than fossil fuel (IPCC6) changes?

As we mention in other parts, the primary goal of the study is not to attribute sources on different aerosol types, especially given the uncertainty that characterizes the CALIPSO aerosol subtypes. We argue, however, that consistency in seasonal characteristics and changes in an aerosol subtype with GFED constraints the possibilities of the origin of this subtype. In this sense, we claim that the "polluted dust" subtype is probably of local origin. For its possible effects on interactions with clouds, however, fractions of dust and pollution are of less importance.

page 4 / 38 given the potential of advection it would be useful also to look at trends including surrounding regions (at least for the aerosol data)

While this would support conclusions on the specifics of the origins of these changes, this analysis would exceed the scope of the study, since attributing specific reasons of changes in aerosol concentrations is not the primary goal.

page 5 / 2 MODIS pixels for AOD are usually provided at 10x10km so the use of 'pixel' here for 1x1 deg is a bit misleading

The term here refers to level 3 "grid cells". We will use this term consistently for more clarity.

page 5 / 16 please also add industrial emissions in Figure 3 (contact S.Smith, who provides industrial emissions over time – also for China – for IPCC6 simulations)

This is a constructive idea adding to the completeness of the analysis regarding sources of aerosol change and will be considered in the revised manuscript.

page 5 / 42 it does not seem too intuitive why there should be so much less liquid water clouds the monsoon season: is this an artifact since there are more ice clouds so that the lower altitude clouds cannot be seen?

Our results are verified by two independent studies cited in this part of the analysis (Pan et al., 2015; Cai et al., 2017). Following the referee's question, the same analysis for ice clouds will be included. Please refer also to our reply on the referee's previous comment on Figure 4.

page 6 / 5 the 1x1 region based changes to water cloud cover and water content show a lot of sub-regional signals … so maybe also aerosol data (e.g. AODf data) should be examined on that basis…?

Following the referee's suggestion also in other comments regarding analysis of fine mode AOD, corresponding data from MISR will be included in the revised study.

page 6 / 20 in figure 6 changes in the y-axis direction are relevant, so that the Reff results are rather meaningless – other than could drops were relatively large in 2003 … which however was identified in the text as an artifact. Although Figure 6 is a nice analysis the results may not be robust enough to make good cases for cloud property changes.

In Fig. 6 the x-axis position is also important since it determines the start year of the period examined each time. Hence in the R_{eff} case, apart from start and end years around 2000, which are known to be artifacts, the plot shows that there was a relatively constant decrease in the period before 2000 and a relatively constant increase afterwards. We consider that even after identification of the artifacts, this analysis still contains useful information, and it is a relatively robust way to exploit the long time series available from CLARA-A2 and highlight the importance of changes during the past 10 years compared to earlier periods.

page 6 / 20 the changes to aerosol (earlier) and cloud (later) do not have the quite same seasonality … and the admittance of potential meteorological causes is good though limiting to the goals of the study.

The seasonality in changes is indeed different. However, it would be oversimplified for ACI to manifest through common seasonality in changes of total aerosol and cloud properties. But in cases where strong changes in clouds and specific aerosols occur simultaneously, possible interactions playing a role are worth investigating. We consider however a prerequisite in such a study to "admit" potential meteorological causes and examine if these can explain the changes reported or if they can be excluded as explanatory mechanisms. Our analysis indicates that the latter is true.

page 7 / 5 unfortunately no renalaysis data are shown (in that context it would be interesting if reanalysis sees the same 'trends' for aerosol and cloud properties as the observations. The reanalysis also could address if there were changes to the monsoon (strength and location) … e.g. were there trends in precipitation and are there temporal shifts (e.g. associated potentially with temporal delay with more fine-mode aerosol)? Also do the reanalysis data see shifts in fine-mode AOD altitude as indicated by CALIPSO?

All the analyses suggested by the referee would be interesting and possibly strengthen some of our conclusions, although there is always a question when reanalysis agrees with observations: does this agreement verify an underlying mechanism or is it just due to assimilation? In the case of CAMS data, probably the latter holds, since the aerosol-cloud-radiation interactions mechanisms are not included in the model (Flemming et al. 2017). Such an analysis would extend considerably the present study, of which the purpose is to examine what can be possibly inferred using observations only, minimizing the need for analyzing reanalysis data.

page 7 / 25 to exclude monsoon impacts the focus is on changes to aerosol and clouds in the dry season … so maybe this can be even part of the title.

The season of largest aerosol and cloud changes is prominently mentioned in the abstract and elsewhere. We think that including this in the title would make it excessively long.

page 7 / 37 add 'rapid' after 'more'

Ok.

page 8 / 1 also aerosol from pollution is absorbing (ssa ~ 0.92) – although usually not so strong as from fresh biomass aerosol (ssa ~ 0.85).

This shows that even if part of the aerosol change is from pollution rather than biomass burning, there is still considerable absorption. We will include this detail in the revised version.

page 8 / 13 if there I so much low altitude cloud cover in Oct and Nov, how reliable is the CALIPSO statistics (other than the strong trend changes within 1 month seem highly suspicious)

In October and November the cloud fraction is around 70% (Fig. 4). Therefore, there are still considerable cloud-free periods, allowing to retrieve aerosol information from CALIPSO for the full atmospheric column. The aggregation of CALIPSO data over rather coarse (2 x 5 degree) grid boxes further increases the number of valid aerosol profiles. In addition, when low clouds are present, potential aerosols above these clouds can still be identified with CALIPSO.

page 8 / 35 interesting but speculative, although possible at least to some degree

This mechanism is indeed mentioned as a possibility, with no claim of proof based on the present analysis.

page 9 / 10 to make the stated case on absorbing aerosol decrease in that region (other than aerosol in that region is absorbing to start with) it would be useful to demonstrate that these aerosol reductions are associated with the fine-mode AOD (at this point there is a reliance on Calipso typing which is quite *general and its association with biomass burning is not very convincing as also urban/industrial (fossil fuel) emissions were reduced and may be even more relevant.*

The association with biomass burning was based on the good agreement with the GFED, rather than reliance on CALIPSO aerosol subtypes only. However, fine mode AOD analysis will be included to add robustness to our conclusions.

page 9 / 20 the disclaimer is warranted (though disappointing)

We agree with the referee's comment. However, one should acknowledge these limitations, which are inherent to observations, and recommend the most promising way forward, which includes modelling simulations in order to overcome these limitations.