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Atmospheric Chemistry and Physics Discussions

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 #1

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Review of paper:

Satellite observations of aerosol and clouds over southern China from 2006 and 2015: analysis of changes and possible interaction meachanisms. by N.Benas et al.

Positives - exploring co-located changes in aerosol and cloud retrievals for insights - picking a (S.China) region, where aerosol loads have been changing on decadal scales - looking for consistency by exploring different aerosol and cloud products

Concerns - AOD is a poor indicator for CCN concentrations – AODf is much better (I suggest MISR) - CALIPSO typing cannot really distinguish between pollution and wildfire (and wildfire is not so effective for CCN as pollution) thus looking as GFED

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(biomass, van der Werf) emissions rather than also industrial emissions (IPCC6, Smith) is not convincing - the examined region is relatively small - the opportunity to contrast impacts with aerosol increase (before 2008) and aerosol decrease (after 2012) is missed - many data limitation / inconsistencies are recognized but not further explored . . . so the value of the paper is limited and the suggested links remain speculative.

General comments:

This study examines co-located 'observational data' based on satellite retrievals for aerosol and clouds over time. Here, a relatively small region over China is picked since over the larger China region the (fine-mode) aerosol loads after many decades on continued increase have been decreasing over the last decade. Co-located cloud properties over the same period were examined. The associations suggest that for aerosol impacts on low water clouds the first indirect effects (Twomey) seem unimportant (which is not completely surprising as baseline CCN concentrations are already very high), while cloud lifetime impacts (aerosol solar absorption and heating associated evaporation for less cloud cover) seem more relevant. Hereby it is suggested that the reduced aerosol load and absorption is recent years might explain increases to low altitude cloud cover and to liquid water content. 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/ 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. 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.

details in the supplement

Please also note the supplement to this comment:
https://www.atmos-chem-phys-discuss.net/acp-2018-982/acp-2018-982-RC1-supplement.pdf

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