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Interactive comment on "Spatial and seasonal variations of aerosols over China from two decades of multi-satellite observations. Part II: AOD time series for 1995–2017 combined from ATSR ADV and MODIS C6.1 for AOD tendencies estimation" *by* Larisa Sogacheva et al.

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We thank the reviewer Dr. A. M. Sayer for his positive statement and very constructive comments. We much appreciate his thoughts and suggestions that reflected in the improvement of this manuscript. Detailed answers are below.

Summary:

I am writing this review under my own name (Andrew Sayer) as I have previously discussed this research with the authors, and am on the team responsible for the MODIS aerosol data products being used in the study. I also reviewed the paper de Leeuw et al (2018), which is in some sense a predecessor to this study, and the Part I of this paper also by Sogacheva et al and also currently in ACPD. I feel I am able to provide an impartial review, but am signing the review in the interests of transparency.

The goal of this pair of papers is to look at spatial and temporal (seasonal/interannual) variations of AOD over China. This is accomplished mainly by using two satellite data sets: the ADV algorithm applied to the combined ATSR2/AATSR record (1995- 2012), and the combined Deep Blue/Dark Target algorithms applied to the MODIS Terra record (2000 onwards) from the latest Collection 6.1. Part I contains some validation results and an initial look at the time series, while this Part II focuses on trends ("tendencies" in the authors' terminology) during several time periods where emissions policies may have influenced the aerosol loading. These papers are linked so I will summarize my review of Part I first (which be found on the ACPD page at https://www.atmos-chem-phys-discuss.net/acp-2018-287/), since this Part II requires Part I to stand on. For Part I, I have recommended revisions and re-review. The two main technical threads of my review of Part I were that (1) more needed to be done to establish the validity of treating ATSR2 and AATSR as a single record (which is the underlying but untested assumption), and (2) some of the time series analysis in Part I should be moved to Part II to keep the flow of both papers better and avoid some redundancy. So this review should be read with that in mind.

My overall recommendation for this Part II is also for major revisions and re-review. It's an interesting and important topic, but I don't think it is ready for publication in current form. I would like to review the revision; I would prefer if Part I can be revised and eventually accepted for publication first, if possible, so that we have that as a stable version to refer back to when reviewing a revision of this Part II, since the papers are quite closely linked. This is an interesting study but I think (see below) that the ATSR/MODIS merging technique requires some more examination, and also the conclusions would be better supported by including additional meteorological and/or geophysical data products in the analysis (so we can see whether AOD changes are likely to be the result of policy, or whether weather patterns may be an influence here).

Uncertainties in the method and results also need better quantification. Note I am not an expert on policy or emissions, so my comments mostly focus on the statistics and AOD data. Hopefully another reviewer can comment on policy/emissions in more detail – my lack of comments is due to a lack of expertise to judge in those areas.

The quality of language is overall good and any issues can probably be dealt with by Copernicus' copy-editing and typesetting process. Therefore my review mainly concentrates on technical abstracts. I have tried to separate each main comment into its own paragraph to respond to. Here, PXLY refers to page X, line Y.

Specific comments:

Abstract: I would condense this into one paragraph if possible and shorten it to highlight the main findings. For example I think the authors can cut out the discussion on linear trends across the whole period, since one of the main points of the study is that it should not be considered one period due to the changes in emissions policy. I'd also cut out the discussion of annual trends/tendencies since I think (as discussed before, and below) only seasonal trends are meaningful due to the seasonal differences in aerosol loading, type, and retrieval coverage (from e.g. cloud cover, snow) aliasing into the annual means in a complicated way. Also, the papers cited in the abstract can be removed – these citations are in the body text, they are just adding length here; traditionally one doesn't need to provide citations to back up statements in the abstract because that's what the rest of the paper is for.

The Abstract is shortened. The citation is removed.

P3L2-3: I would avoid giving urls like this as citations here, particularly since the latter is an opinion piece. Urls are not always stable and one can't be sure the content is going to change or is valid. It would be better to cite something with a DOI or official publication number. For example the first link is for the World Bank so there must be some report or something which can be used.

URL is replaced with a reference to the similar publication.

Figure 1: Likewise, I would not give an url here for the population data used. If you click through the url, it gives a citation for the data set which should be used instead.

The DOI for the data is provided.

A couple of other things jump out at me from this figure. First, it seems that the largest population change in this region is not in fact China, but India. If population acts as a driver for anthropogenic aerosol emissions, one might expect that observed aerosol changes in China may be influenced by changes in transported aerosols from India. If this contribution cannot be quantified, it means that one cannot state that observed changes in China are a result of changes in Chinese policy. (The fact that aerosols don't follow

national borders is one reason why in general I prefer regional studies to national studies – you have to be able to account for the broader context of regional emissions/meteorology changes.)

We agree with the hypothesis that aerosol measured in China might be transported from some other areas (India, Russia, etc.). However, on averaged AOD maps, AOD level over SW China is low, since the AOD transport from India and Bangladesh, which are the strong source of aerosol particles of different origin, is highly blocked in eastern direction by the Himalayas. Whereas highly populated and industrial areas are often recognized in China by the local elevated aerosol concentration. The increase in the population usually follows the growth of industry and, in case of China, is resulted in the increase of pollutants. In the current ms, we consider different areas, where the economic growth was not equal during the last decades. Difference between the areas in the aerosol tendencies proves that changes in local emissions play stronger role than the change in the aerosol transportation to China from outside the country. We mention later in the text other reasons for changes in the aerosol concentration, but the impacts of each factor are not considered.

Secondly, it looks like the population in the Sichuan Basin area (30 N, 105 E) has dropped somewhat since 2000, while the rest of China has been flat or steadily increasing. Is this right? I did a quick search online and it looks like the Chengdu metropolitan area population is increasing (http://worldpopulationreview.com/world-cities/chengdu-population/ - perhaps this is the red dot on the map here – but the overall population of Sichuan province is fairly stable (http://population.city/china/adm/sichuan/). However I'm not sure of the reliability of these sources, and it is difficult to estimate the total population from these maps of population density because the colour scales seem to saturate and we don't have grid size information. So perhaps people in Sichuan province are becoming more concentrated in Chengdu, I don't know. The point is, I think both of these aspects should be discussed in more detail in the manuscript.

You are right that overall the population in the Sichuan province has not changed much. However, people were moving to the Chengdu metropolitan area (current color scale, which was chosen to be the same for all three maps for comparison may not show that clearly) from other regions of the Sichuan province. Thus, we discuss in the text also the changes in population in megacities. We added some clarification to the text.

Section 2: This is largely a repetition of Part I, in that it is introducing the satellite data used. I understand a recap of the data is needed, but I think that this could be shortened. For example we don't need to know the spectral and spatial resolution of the ATSR and MODIS instruments, or provide the validation summary table. I think it's enough to say you're using the level 3 monthly products at 1 degree, and refer back to Part I for more details. Particularly since the number of 3-way matchups (AERONET, ATSR, and MODIS all together) was low and largely confined to cities in Eastern China, I think discussing these statistics in detail gives a (possibly false) impression that we can be confident that these are representative of relative performance across the whole of China. Plus, instantaneous validation differences will not necessarily reflect differences in the monthly or longer means, and the method in Section 4 is meant to show and reconcile these differences. So I think Section 2 can be shortened to a couple of paragraphs.

The section is modified. The note about the limited number of validation points in the northwest of China is added. We keep the summary for validation results, on which the method introduced here is based.

Section 4, general: the large number of long subscripts on variable names makes things cumbersome to read (hard to visually follow the equations). I suggest replacing "ADV" subscripts with "A" and "MODIS" or "MOD" (both are used, but I think mean the same thing) subscripts with "M". Other subscript shortenings could include "y" for "year", "c" for "comb", or "rc" for "rel_corr", for example. This will make it easier to follow the

equations.

Some of the subscripts are shortened, as suggested. Others are kept as originally introduced since with further shortening the visualization makes reading more complicated.

My understanding of the method is that the overlapping ATSR/MODIS period (2000- 2011) just averages the two time series. Then the size of this adjustment in the overlapping period is used to scale the pre-2000 ATSR data, and the post-2011 MODIS data, to generate the "merged" time series. This is done twice: once for an "annual" correction, used for the merged multiannual time series, and once for a set of four "seasonal" corrections, used for the merged seasonal time series. Further, the calculation is performed separately for each 1 degree grid cell. Is that a correct description? If so I would include a couple of sentences to that effect somewhere up the top, before the equations, in case the reader gets lost.

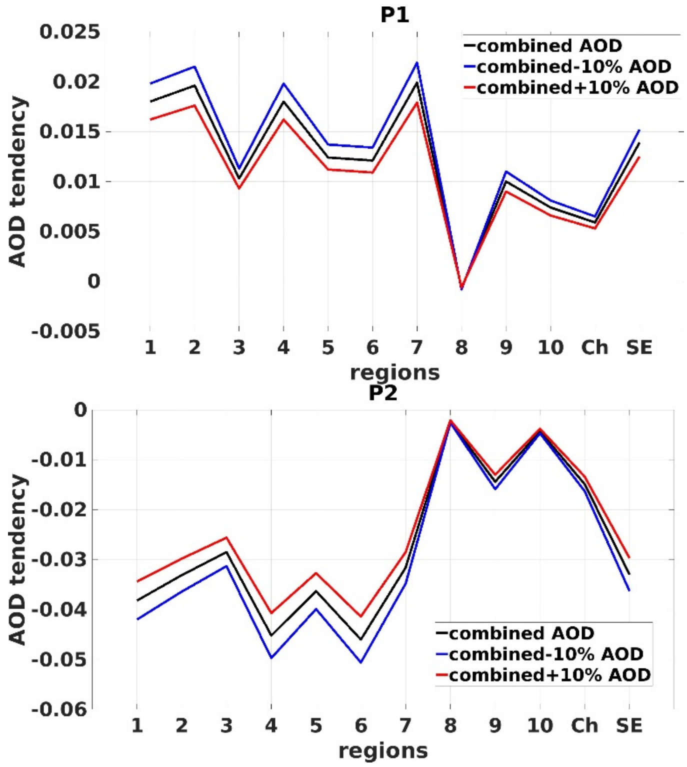
The text was modified. In the current version, we first introduce the method and then say that it is applied to L3 annual and seasonal aggregates.

P10L27-28: the authors mention Bourassa et al (2014) as an example of this technique being used elsewhere. However I'm not sure that is necessarily a good justification. Bourassa et al were looking at stratospheric ozone, which has (to my knowledge) a somewhat longer lifetime and smoother spatial distribution than AOD, as well as fewer contextual (i.e. surface cover or type-dependent) uncertainties. These are very different error characteristics. Looking through the Bourassa paper, the relative differences between the ozone data sets used were in many cases a lot smaller than the differences seen for AOD here. So the method which is justifiable for one geophysical data set is not necessarily a good choice for another one. Are there other example applications, or justifications which can be made?

We agree that the approach for combining ozone data sets is different (https://www.atmos-chem-phys.net/17/12533/2017/acp-17-12533-2017.html). We removed the reference to Bourassa et al. (2014). We could not find any reference where a similar method to combine data from different satellites is introduced and discussed.

I have some more general concerns about this method for combining the ATSR and MODIS records, which become more severe because the purpose is trend analysis, which becomes particularly sensitive to values at the start and end points. Both the start and end points of the combined record are being adjusted by this method, and the "combined" to "adjusted MODIS" changeover also takes place at the same time (year 2011) as the start of the trend analysis period "P2" (2011-2017). So uncertainties in the merging process will unfortunately affect the trend analysis at the points at which a trend calculation is most susceptible to artefacts.

We indeed did not find the way to estimate the uncertainties related to the merging. However, we estimated the difference in the tendencies, assuming that the combined AOD is over- or underestimated by 10%. In the figure below, we show the annual AOD tendencies for P1 (upper plot) and P2 (lower plot) estimated for each region from the combined dataset (black line) and for two "tested" AOD datasets. In the first tested dataset we assume that our combined dataset is overestimated and "real" AOD is 10% lower (blue line). In the second dataset, we assume that our combined dataset is overestimated and "real" AOD is 10% higher (red line). For both P1 and P2, the difference in the AOD tendencies for the tested datasets was less than 10% from the tendency estimated of the combined dataset.



However, we decided not to include those checks into the manuscript, since, as it was mention before, the length of the periods, where we estimate tendencies, is short and the results might have uncertainties due to the short length of the periods (thus, called tendencies).

Instead, to give an estimation of the quality of the combined AOD datasets, in the revised version we added a chapter (Sect. 3.3) on the ADV, MODIS and combined AOD seasonal/yearly comparison with AERONET. This is not validation but more a comparison, since all the AERONET available data were used to calculate the seasonal aggregates and the satellites temporal coverage is lower. It means that some of the events might be missed by the satellites which are included in the AERONET AOD seasonal/annual aggregates. The comparison has been performed for four periods:

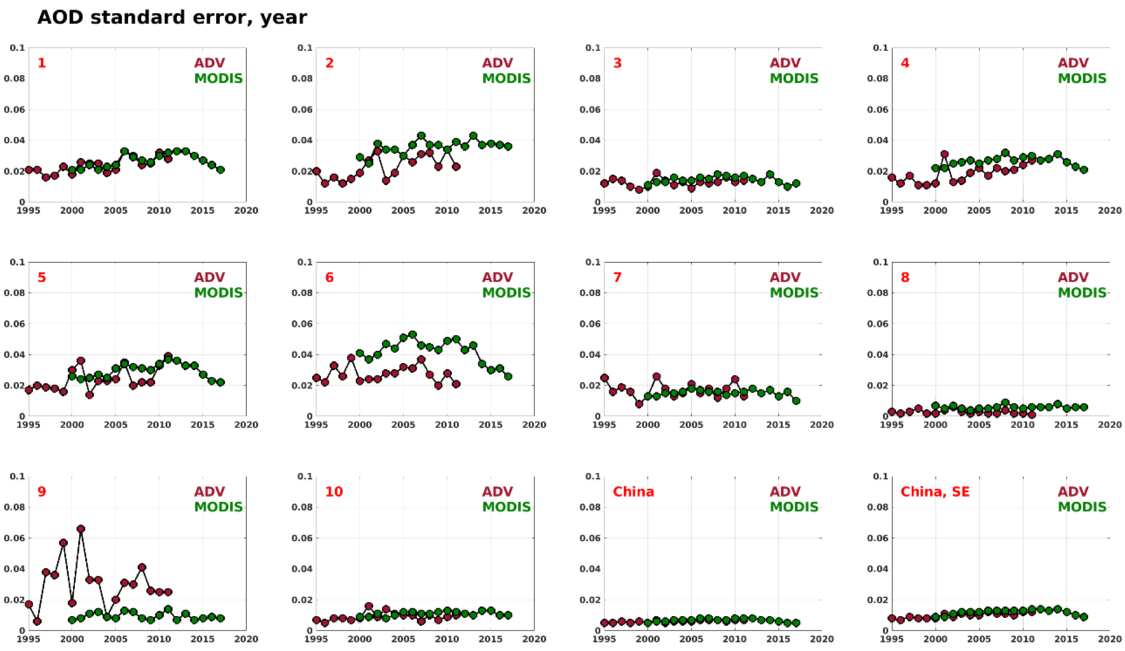
- T2 (2000-2011, overlapping)
- T3 (2011-2017)
- ADV period (1998-2012)
- MODIS period (2000-2017)

For T1 (1995-2000) there was not enough AERONET data for comparison.

The comparison shows that the statistics for the combined dataset are slightly better than for MODIS and ADV separately.

I appreciate the authors' efforts to merge the ATSR and MODIS records here, and I don't know that there is any well- defined most appropriate way to do so. So this is one attempt which seems like a reasonable thing to try, and I am not sure what other way to harmonise the AOD record to suggest. However the fact remains that this method will introduce uncertainties, which may be systematic, and influence calculated trends.

We calculated the AOD standard errors for each year/season for both ADV and MODIS but, we decided to not include it to the manuscript. Here is an example for annual AOD :



For China and SE China, the AOD standard error is low (~0.01). For other areas, the AOD standard error is higher, but what is important here is that the ADV and MODIS AOD standard error time series have similar patterns overall. This gives us the confidence to conclude that the AOD standard errors do not influence the AOD time series significantly.

So, at a minimum, the possible influence of these effects must be quantified. Here are some thoughts about how to do that. The decision to do a simple average for the overlapping (2000-2011) period seems to have been made on the fact that the mean bias of the two data sets vs. AERONET, based on the limited available number of samples and limited available locations of samples, was roughly equal and opposite (so averaging might be expected to cancel out the bias on an average basis). Despite the fact that this was presented as a difference in ABSOLUTE AOD bias, the correction term is applied as a RELATIVE AOD correction. So that feels somewhat inconsistent.

We scaled the correction to avoid the situation, when the correction itself is higher than the AOD.

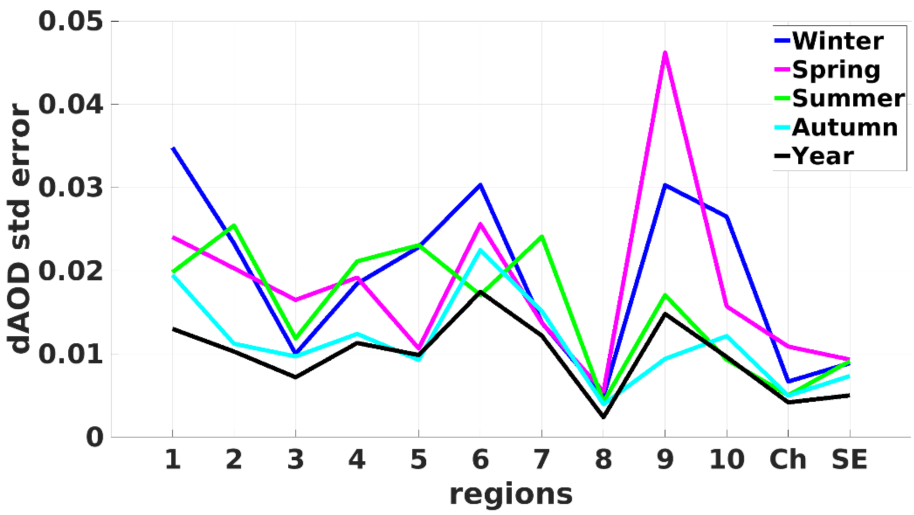
The authors could make maps of MODIS-ATSR on a monthly basis, both in absolute and relative terms, and see what it looks like. If the bias vs. AERONET of roughly 0.06 (for MODIS) and -0.07 (for ADV) is representative everywhere, then I would expect these maps of absolute difference to hover around values of 0.13 and have little spatial or temporal variability. If they don't, then it tells you that the AERONET matchups aren't representative of the bigger picture. In contrast if maps of relative AOD show small variability, it suggests that a relative scaling is more appropriate.

We disagree that the absolute difference of ~0.13 should be representative everywhere. This number depends on the sampling. For fine-dominated aerosols, the validation results show a bias of -0.09 and 0.08 (for ADV and MODIS, respectively); for coarse-dominated aerosols, the bias is -0.11 for ADV and 0.10 for MODIS. The conclusion we draw is that biases for ADV and MODIS are opposite in sign but similar in amplitude. We modified the conclusions in Sect. 3.3.

For the period with both ATSR and MODIS data (2000-2011), my understanding is that the two time series are averaged, and it is the correction across this 11-year period which is the basis for correction periods T1 and T3. So one other thing which could be tested is to see what the variability in the MODIS-ATSR difference is over 2000- 2011, e.g. what the standard deviation of the difference is. If it is 0 then MODIS and ATSR are always offset the same amount. In reality it will be nonzero, and this additional variability should be propagated into the trend uncertainties discussed later in the paper. Although you might get a trend with apparently low noise, if you know that part of your time series may

have uncertainties which aren't captured by this error model, then those errors (in this case, the interannual variability in MODIS-ATSR AOD in 2001-2011) should be added on when estimating the total uncertainty on a trend. A similar point can be made (see my review of Part I) when considering the combination of ATSR2 and AATSR to give the single combined ATSR record used as the basis here.

We estimated the standard error for MODIS-ATSR difference over 2000-2011. See the figure below.



For the annual AOD, the dAOD standard error is around 0.01; for the seasonal AOD, the error is somewhat more significant (ca. 0.02 with some deviation for different seasons and regions). We consider that it is low value and will not change the tendencies significantly (as shown above, where the difference between AOD from the combined data set and the tendencies for the combined ± 10%AOD are presented).

In Sofieva et al., 2017 the uncertainties are not considered (https://www.atmos-chem-phys.net/17/12533/2017/acp-17-12533-2017.html), where trends in ozone are estimated. They make a statement that "different amounts of data available over time result in varying uncertainties over time, which might improperly weight the time series. In our regression, all data points are considered with equal weights, and the uncertainty of the fitted parameters is estimated from the regression residuals". We are following Sofieva et al. method, and also added uncertainties for the estimated tendencies to the table with the statistics for tendencies.

My remaining comments are more general, because I think that the above comments and discussion, plus the review of Part I, may necessitate a rewrite of some of the later sections of this study. Some of the points which I think need to be discussed here in more detail include:

1. Trend terminology. The authors say "tendency" rather than "trend" throughout. I prefer the more standard "trend". From previous discussions with the authors, my understanding is that they preferred the term "tendency" because they feel that "trend" should refer to a longer time period than considered here. My personal feeling is that "trend" is clearer for the reader, so long as it is made explicit that one should not extrapolate out of the time period under consideration.

Indeed, we say "tendency" rather than "trend" because the length of the periods when AOD is more less steadily changing is short (11 and 6 years) to estimate trends (Weatherhead et al., 1998).

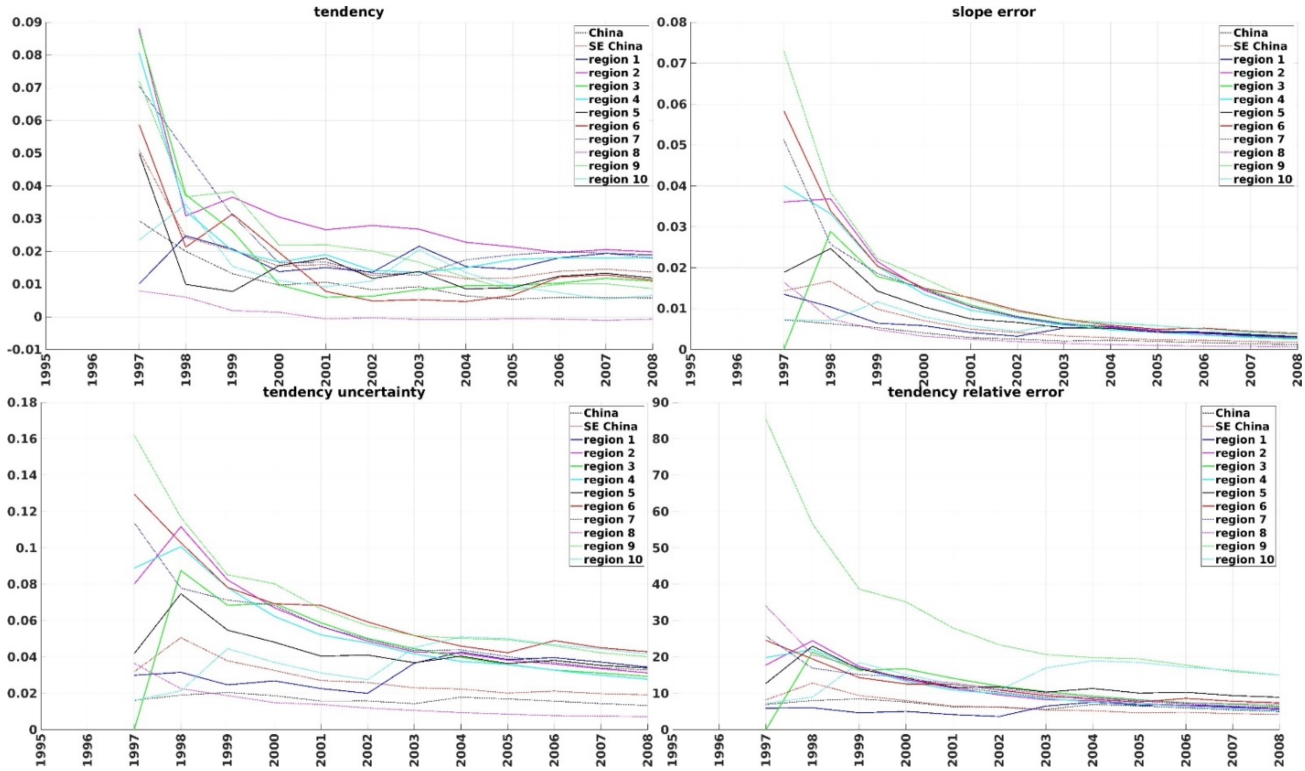
2. Annual and whole period trends. I continue to think that, since the time series show seasonal variation and are not linear, it is not sensible to do whole period (WP) trends, or annual trends. I think it's best to show only the piecewise trends.

The results and discussion on the WP AOD tendencies were removed. We included those into the version submitted to ACPD thinking that they could have been used for the comparison with other studies (e.g., Wang et al., 2017). However, we keep the results and discussion on the annual AOD tendencies, since this general information is useful for e.g., modelers.

3. Trend breakpoints. The authors split the trend analysis into WP, an early P1, and late P2. These split times are informed by times where policy changes may have had an influence. However there also exist established statistical methods to estimate whether there are breakpoints in trends, and when these breakpoints are. It would be good to use these methods to see whether in fact any such breakpoints are detectable at the point the authors assume they are there.

In the current manuscript, we study the possible connection between the emission policy and the aerosol concentration. Other factors, e.g., meteorological conditions, dust transport, biomass burning are discussed in Part 1 and Part 2 briefly.

Besides binding the choice for the periods to the Five-Years Plans and emission reduction policy, we performed statistical tests, where we looked at the AOD tendencies, uncertainties of the tendencies, the error of the slopes and tendencies relative error annually and for all seasons all regions. The example for the statistics for annual tendencies for periods of different length starting in 1995 is shown below (number at, e.g., 1999 show statistics for the period 1995-1999).



Similar test was performed for the second breakpoint in ~2011. We added a paragraph to the manuscript (Sect. 5.3), summarizing the results.

4. Trend significance. The actual fitting mechanism and uncertainty estimation is not discussed in detail. P21L5 says it is linear regression (I assume ordinary least squares) and they define p<0.05 as statistically significant (I assume here this is de- fined as the estimated trend is at least twice as large as the estimated uncertainty in that trend, i.e. 2 sigma). Is autocorrelation considered? Often in trend studies lag 1 autocorrelation is estimated and used to correct the uncertainty estimate (because many geophysical data fields can be autocorrelated on time scales of months, seasons, or years). This gives a more realistic, and generally larger, estimate of the uncertainty on the trend because

autocorrelation tends to be positive. See e.g. Weatherhead et al (JGR 1998, https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1029/98JD00995). If this is not done then there should be some evidence given why this model is reasonable.

We have not include autocorrelation into our tendency analysis. The reason for that was that we do not expect any autocorrelation between yearly AOD (as, an example, one exists for other variables dependent on, e.g., 11-years solar cycle). Our annual time series are built from annual values only. Seasonal time series are built from correspondent seasonal aggregates, thus there is a one-year lag between neighboring values.

Further, as noted above, the uncertainty from the ATSR/MODIS merging exercise should ideally be considered.

As mention before, we did not have a possibility to estimate the uncertainties. The results for seasonal/interannual comparison with AERONET, which are included in the revised version, gives a rough estimation on the combined AOD quality.

Additionally, there's the problem that since so many comparisons are being performed there are likely to be a number of false positives (trends which appear significant but are coincidental). See e.g. Wilks (JAMC 2006, https://journals.ametsoc.org/doi/10.1175/JAM2404.1) for more on the false discovery rate and how to deal with this sort of thing.

Thank you for the suggestion. In the current manuscript, the AOD *tendencies* were discussed; we apply the statistical significance test for both L3 AOD time series fitting and the averaged over the selected areas AOD. We will consider the method introduced in Wilks when the length of the period will be long enough to discuss *trends*.

5. Presentation of trend uncertainties. In the later Figures and discussion, trends are given in terms of both absolute and relative AOD (where I guess relative AOD is defined with respect to some base year – this is unclear – since as the AOD changes by some absolute amount per year, the relative trend would change while the absolute trend would not). For clarity and comparability between regions, I would rather just see absolute AOD trends.

We think that the maps with the AOD relative tendencies contain information that is complementary to the AOD absolute tendencies. With those, we can discriminate the areas where the AOD had the higher changes compared with the AOD local values. This information can be potentially compared with relative changes in the emissions (currently we do not have access to that information on the emissions).

I would also like to have the uncertainties estimated on the trends be presented and discussed (the Appendix tables say they have absolute error in percent, but it's not clear to me what exactly that means, and it is more relatable to have in absolute units like the trend itself). For example at face value the sign of a trend may have changed between two time periods, but it may be that the difference is within the uncertainties of the individual trends, in which case it is more accurate to say that a change in behaviour cannot be identified. Thresholds on p-value are somewhat arbitrary and so I think it is generally more useful to present trend uncertainties instead (or in addition) in the text and tables in the Appendix.

We added the results for tendencies uncertainties to the Table A1 and discussed them in the text.

6. Evidence of attribution for trends. It makes sense that changes in policy could affect emissions and change the AOD. However another key factor, which is examined in several other studies in China and elsewhere, is changes in meteorology. For example if there have been changes in air stagnation frequency, or aerosol transport pathways (for sources outside China as well as those inside China), then these might be magnifying or masking any trends resulting from policy changes. Additional data sets that might be able to support this would include emissions data bases, other satellite products (e.g. SO2 and NOx, which are briefly discussed), and meteorological reanalyses. Some of this has been

done by other studies, and I'd like the discussion to go into more detail on those. But some of this may not have been and the authors might need to do these analyses themselves. Otherwise it is premature to try to state the reason for the trends.

In de Leeuw et al. (2018) and Part 1, we briefly discussed the potential impact of the meteorological conditions and the dust transport on the AOD in China. In the introduction to Part 2, we also shortly mention the results from other studies on the contribution of the meteorological conditions on the AOD in China. E.g., Gu et al. (2018) are referred, who shows that the variation of AOD over Beijing was significantly affected by the anthropogenic aerosol emissions and less affected by the wind and temperature inversion. In the current manuscript, we show that the AOD tendencies have a good agreement with the 5-years plan related emission policies. We discuss the changes in the emission policy in China and briefly discuss the changes in NOx and SO2 emissions in China published by van der A (2017). The detailed analysis on the decoupling of the contribution from changes in the emissions and the meteorology, including dust transport, and their influence on NOx, SO2, and AOD over the whole China, which implies a detailed analysis of the reanalysis data, is a topic for another manuscript.

In summary, the main thread is I feel it is important to be thorough and give reasonable uncertainties – say what we can – than to make a conclusion which isn't fully supported. Particularly since this is an inherently political topic. Maybe the data we have are not enough to be conclusive yet, in which case it is even more important to say as much as we can but no more and be clear about what the biggest uncertainties which we need to reduce to be able to answer the question are. The last sentences of the paper (P26L26-27: "Thus, in the current study the effect of the changes in the emission regulations policy in China is evident in AOD decrease after 2011. The effect is more visible in the highly populated and industrialized regions in SE China.") are very strong statements, and this might indeed be the case. But I don't think the discussion of uncertainties is quite thorough enough, or other explanations and their contributions examined in enough detail, to make this case.

We agree that the statement is strong, but we are not claiming that the AOD decrease is caused by the changes in emission policy only (we discuss other factors which may influence AOD in the introduction). "Evident", in the current sentence, means that the AOD decrease is noticeable after 2011, when a new regulation plan for emissions was accepted. Those two events (new police and the decrease of the AOD) are in good agreement. The contribution of the anthropogenic emissions to the AOD is critical in China (however, might be comparable with episodic dust or biomass burning transport). Such a strong decrease cannot be caused by e.g., meteorological factors only, which contribute less to the AOD, as compared to the anthropogenic emission in China, as shown by Gu et al. (2018).

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