We thank the referee #2 for giving valuable comments. We think that the paper has been improved based on the suggestions from the referee. We respond to each specific comment below. The comments and questions from referee #2 are in italic font.

In the introduction, the authors cite van der A (2006) regarding the strong NO2 increase over China over the past two decades. I think it would be good scientific practice to acknowledge the earlier scientific literature on this subject (Irie et al., Richter et al. both from 2005).

We agree with this suggestion. We add the reference of Richter et al., 2005 in the introduction (Page 6339) by changing line 9-10 to "For instance, satellite measurements showed that NO_2 column concentrations increased about 50 % from 1996 to 2005 (Irie et al. 2005; Richter et al, 2005; van der A et al., 2006)."

On p. 6343, the authors describe that they extend the modelled NO_2 fields above the CHIMERE upper boundary at 500hPa using a climatological partial column. This gives rise to the following questions:

– How is this column derived? – How is the AK applied to this partial column?

We use the same method as Mijling and van der A (2012). So we didn't give detail information for these two questions. We add the reference on Page 6343 line 5: "As mentioned in the paper of Mijling and van der A (2012), to compare…"

Also on p. 6343, the authors describe an updated version of DECSO. It is not clear to the reader, why some parts of the description of the DECSO improvements are here, i.e., in section 2.1, and others are in the later chapter 3.

The description of the updated version in this part shows the difference of the default version we use in this study and the original version from the paper of Mijling and van der A (2012). We refer to this version as DECSO v3a. More detail information on the improvements leading to DECSO v3a will be published in another paper. However, we add some details in this paper to clarify this to the readers. During the research, we found some problems over the study domain East Asia. We solve the problems by replacing the CTM model and adding a satellite observation filter, This improved version is referred to as DECSO v3b.

On the same subject: The authors fail to give either details or reference regarding the sector-dependent injection height in the model.

We give more details about the sector-dependent emission injection height.

We change this paragraph on Page 6343.

“In this study, we used an updated version of DECSO, which is referred to as DECSO v3a. In particular, the calculation speed has been improved in this update. DECSO does not distinguish between biogenic emissions and the anthropogenic sectorial emissions. Emission differences are attributed to anthropogenic contribution only, i.e. the biogenic emissions are assumed to be modelled correctly by the CTM. Emission updates are distributed by ratio over the sectors (power, industry, transport, domestic) as described by the apriori emission inventory. If a grid cell is dominated by power plant emissions, however, emission updates are attributed to the power sector only. The locations of power plants are provided to the algorithm as additional a priori information. In DECSO v3a, the emission injection height has been made sector-dependent. Emissions are injected in the lowest three model layers of the CTM; each sector having its characteristic vertical emission distribution. For example, transport emissions are released at the surface, while power plant emissions are fully released in the third model layer corresponding at a typical smokestack height.”

On the same subject: The authors fail to give either details or reference regarding the used backward trajectory calculations.

We add the following explanation in this part of the text.

“Trajectory calculations of the observed species are crucial in the determination of the source-receptor relations. The DECSO algorithm uses meteorological wind fields (the same as used in the CTM) to calculate how the content of a tropospheric column is advected over the model domain. Here, the injection heights are distributed according the modelled vertical NO_x distribution. In DECSO, the forward trajectory calculation is changed to a backward trajectory calculation, i.e. the source-receptor relations are calculated backward in time, based on the height distribution of NO_x modelled at satellite overpass time.”

On the same subject: The authors’ re-definition of the observation error Eobs seems arbitrary. Specifically,

– the authors fail to justify this error tuning; they simply describe the effects, but not why it should be a valid assumption to give more weight to larger columns.

Assuming the relative error of observation will be more or less equal. Then during the data assimilation process in DECSO, the error of high observations will be relatively high, thus the weight of this high observation is low. But the low NO_2 observation value with low error has more weight. This will favor the low observations and thus, the emission updates cannot easily capture new emission points or high emission episodes.

We add the reason after the description of the effects by tuning the satellite error on Page 6343 (line 21). "[…absolute error for low values (typically around 0.5 10^{15} molecules cm^{-2}). <u>In this way, DECSO captures better new emission points or high emission episodes."</u>

– the wording the error [. . .] is recalculated is misleading, as it suggests that Eobs is a physically meaningful error estimate. I suggest re-wording this along the lines of DESCO v3 uses tuned synthetic error estimates derived from the original satellite uncertainties via [. . .].

We agree this comment. The sentence on Page 6342 line 14-15 is changed as following:

"In DECSO v3a, tuned synthetic error estimates E_{obs} are used, derived from the original satellite observation via:"

– I fail to see how, using typical Csat of 5E15, f can be anything different than zero, which according to equation (1) just leads to a 50% reduction of the observational error

The C_{sat} is a normalized value from 10^{15}. We add the following explanation in the text (Page 6343 line 18).

"[…]the satellite observations. The unit in this formula is 10^{15} molecules cm^{-2}. The modified […]".

This aim of this formula is to keep the original observation error for low NO_2 observation values and half the original error for high NO_2 observation values. So the high observation values will have more weight during the data assimilation process.

On l. 27 on p. 6344, the authors should justify why they filter out the outmost 4 pixels on either side of the scan.

Considering this comment, we add the following explanation on line 27 Page6344.

"because the size of these pixels is 3 times larger than the model grid cell. After the filtering, the largest footprint is about 75x21 km^2".

In l. 1 on p. 6345, the authors should detail which surface albedo dataset they use for the filtering.

The surface albedo is given in the retrieval data product and this filter is suggested by the DOMINO Product Specification Document on www.temis.nl. We use this surface albedo criterion only to remove the observations over snow and ice.

We modify the sentence on line1 page 6345 into:

“To reduce the influence of bright surface scenes on the quality of the retrieval product, we use only observations having a surface albedo lower than 20% to remove observations over snow and ice (Product Specification Document of DOMINO v2 on www.temis.nl).”

In ll. 8–10 on p. 6345, the authors (quite inspecifically) describe that the selected data is still of sufficient quality; however, they should also discuss the potential influence / additional uncertainties this modified cloud filter criterion has on / adds to the satellite measurements.

We have checked the distribution of the selected satellite data, which remained unchanged. Indeed, the error on the monthly mean data changes. Therefore, we have replaced line 9-11 on page 6345 with:

“From our analysis of the satellite data we conclude that as a result of this new limit on the cloud fraction the error on the measurements increases with less than 20% and without introducing biases. Yet this effect is compensated by the advantage that more data becomes available. The number of observations increases with about 37 % over the whole domain”

On the same subject: The authors claim that the number of observations increases [by] about 37%, however, an increase of 37% on a total number of zero measurements (as given earlier) is still zero measurements.

The emission of Nanjing can be affected by the observations over the whole east Asian domain due to the transport. The transport process of NO_2 concentrations is considered in our DECSO algorithm. The 37% increase of measurements is over the whole domain. This is added in the paper.

In l. 18 on p. 6345, the authors should give the exact URL where the conversion table from the Technicsl Regulation on Ambient Air Quality Index in China is available.

We add the link of Technical Regulation on Ambient Air Quality Index in China in line 18 on page 6345.

http://kjs.mep.gov.cn/hjbhbz/bzwb/dqhjbh/jcgfffbz/201203/W020120410332725219541.pdf

On the same subject: It is not entirely clear which in-situ NO_2 measurements the authors actually use. From what is described in the text, it seems that they do not use the original in-situ measurements but rather calculate the in-situ measurements from the AQI values. However, air quality indices are usually derived from a number of different air quality indicators; the aqicn.org website lists PM, O_3, NO_2, SO_2, CO. Mathematically, the calculation of the AQI is therefore a mapping from an $n-$dimensional state space to a $1-$dimensional value. Therefore, it is not clear how the authors can derive the NO_2 concentrations which lead to a given AQI value from the AQI (and the mapping table) alone.

The aqicn.org team publishes the hourly Air Quality Index (AQI) of specific air pollutants, such as NO_2, SO_2, and particulate matter (PM10 and PM2.5). Thus, we calculate the NO_2 concentration by converting the AQI of NO_2.

To clarify this we replace the words "different air pollutants" by "specific air pollutants" on line16 page 6345.

In l. 11 on p. 6346, the authors write that NO emissions cannot be negligible. This sounds incorrect to me; maybe the authors meant to write can be non-negligible?

Yes, we agree and we change the word "*negligible"* to "neglected".

The new sentence in line 11 on page 6346 is:

"The added biogenic emissions can affect the emissions estimated for rural areas as biogenic NO emissions in rural areas cannot be neglected in summertime."

In l. 19 on the same p. 6346, the authors write that land use may have large differences in 15 years. Why don't they just show the land use maps for the Nanjing region from both datasets? Also, the authors should reference their Fig.8 in this context.

Thanks for the valuable suggestion. We moved the Figure 8 to Figure 1 and add the new figure of the land use (GLCF 1994) used in CHIMERE 2006.

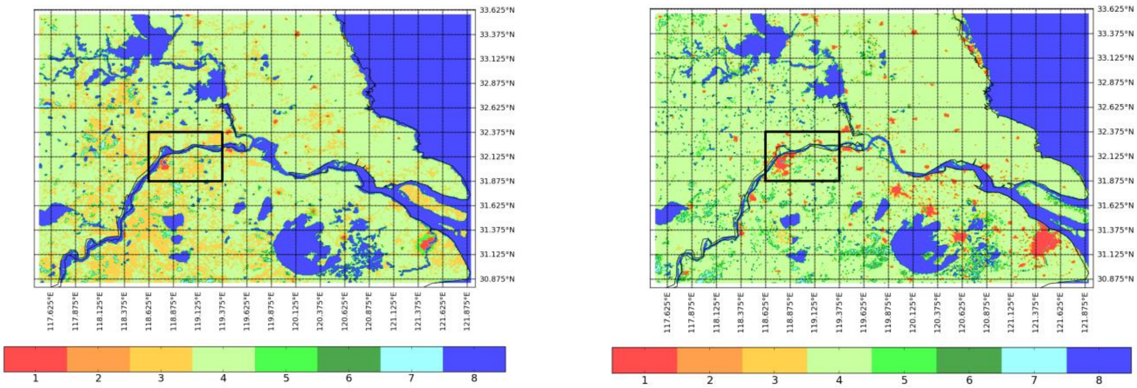

Figure 1. Land use over the Jiangsu Province from Global Land Cover Facility (1994) (left) and the GlobCover Land Cover (2009) (right) and as used in CHIMERE v2006 and CHIMERE v2013. The 8 categories are: 1. Urban, 2. Barren land, 3. Grassland, 4. Agricultural land, 5. Shrubs, 6. Needleleaf forest, 7. Broadleaf forest, 8. Water. The solid rectangle (about 50 x 90 km^2) indicates the 6 grid cells that cover the Nanjing area.

In l. 22 on the same page 6346, the authors should write the differences between DECSO versions 3a and 3b. In the earlier chapter 2.1, the authors only talked about v3, and now there are two sub-versions, and the authors do not explicitly state their differences.

DECSO v3a uses the old model CHIMERE v2006 and DECSO v3b uses the new version CHIMERE v2013b. The DECSO algorithm we described in part 2.1 is actually DECSO v3a. We change the version name in that part to make it more clear.

In ll. 5f on p. 6347, the authors state that CHIMERE v2013 improves the NO_2 concentrations during night, while improvements during daytime are rather small. However, it is unclear with respect to which reference the concentrations improve. For nighttime, the authors have stated that CHIMERE v2013 gets rid of unrealistically low boundary layer heights which previously lead to unrealistically high NO_2 concentrations, but for the dayime, it is unclear which reference the authors refer to when they notice a small improvement, especially, since they did not point towards any deficiencies of CHIMERE v2006 during daytime.

To make it clear, we modify the sentence in line 5 page 6347 into:

“As expected, v2013 improves the surface concentration simulation at nighttime, while differences during daytime are rather small compared to the in-situ observations.”

Again on the matter of improvements of CHIMERE v2013: In l. 16 the authors claim to see some improvements for averaged NO_2 concentrations; but given their own reference to Blond et al. (2007), and given that the surface concentrations used in the present study are unreliable (as previously stated by the authors), maybe the conclusion of noticable improvements is not justified well enough.

We think the 8 month in-situ measurement have enough statistics for the diurnal cycle. We agree with the conclusion of Blond et al. (2007) and that is why we don't use the daily and hourly in-situ observations to validate the model results.

We add explanation in line 16:

"Blond et al. (2007) concluded […] In spite of this, by using the 8-month average of the diurnal cycle to reduce the noise of the in-situ measurements, we see some improvements for these averaged NO_2 concentrations in CHIMERE v2013. "

In l. 25 on the same page 6347, the authors see indication of a better performance of CHIMERE v2013 in summertime, based on the OmF they calculate. However, this conclusion would only be valid if the satellite measurements were reflecting the true NO_2 concentrations in the atmosphere. Given that the NO_2 measurements are subject to measurement uncertainties, the statistical significance of the conclusion CHIMERE v2013 is better than CHIMERE v2006 depends on the magnitude of the measurement uncertainties. At least theoretically, it would be possible that the CHIMERE v2006 forecast were more accurate than the CHIMERE v2013 forecast; if now the NO_2 measurements were strongly biased towards the v2013 forecast, this could lead to the observed OmF values. Therefore, I do not see justification for the authors' conclusion that v2013 is better than v2006. I would appreciate at least a comment from the authors on this matter.

The comparison of CHIMERE v2006 and v2013 are done with the same set of NO_2 values. If, as the reviewer suggests the NO_2 observations are biased, the data assimilation will adapt the emissions (i.e. the free parameter) to remove these biases. This means that the analysis in the data assimilation will in general be close to the observations. In the next assimilation step the model error will grow and the OmF values can be used to judge the performance of the model over a single time step. In addition, the OmF distributions of both CHIMERE v2006 and V2013 are checked and did not show any biases. Since this is a common method for data assimilation schemes (see e.g. Data Assimilation: Making Sense of Observations, W. Lahoz, B. Khattatov, R. Menard(Eds), 2010, page 357), we prefer not to add an additional explanation to the text.

In lines 6 f. on p. 6348, the authors write [. . .] the NO_x emissions are almost entirely removed [. . .]. This statement is not understandable to a reader who is not familiar with the DECSO algorithm. I suggest the authors elaborate a bit on this statement so that readers not familiar with DECSO can understand it.

We re-elaborate the sentence: “At these dates the derived NO_x emissions drop to zero in one day and then slowly increase again to the previous emission levels in the following days.”

In line 8 on the same page 6348, the authors say that the unrealistic emission updates are related to large OmF values. However, correlation is not causation, so I would appreciate if the authors could modify their statement from a relation to a concurrence.

We agree with this comment.

“These unrealistic emission updates concurred with extreme OmF values (lower than -5 or higher than 10 10^{15} molecules cm^{-2}) with relative small OmF variances[…]”

In line 13 on the same page 6348, the authors should give reference to the PM data set they used for this observation.

Following the reviewer’s suggestion, we add the reference of PM data set here.

“ […] the in-situ observations of PM10 from CNEMC (see section 2.3) show […]”

In line 17 on the same page 6348, the authors write about an underestimation of cloud fraction [. . .] from OMI. However, this statement is only admissible if it were already established that the MODIS cloud fraction is more correct / of higher quality than the OMI cloud fraction. As the authors do not give reference to any study allowing this conclusion, I believe it is not adequate to speak of an underestimation.

We agree with the referee that it is too premature to state that MODIS is giving better cloud fraction values under these circumstances, especially since the relation between aerosols and cloud fraction is rather complex. Therefore we have changed “The underestimation of cloud fraction” into “ “The deviating cloud fraction”.

In line 19 on the same page 6348, the authors should give proper reference to the cloud retrieval algorithm.

We have added the proper reference for the cloud algorithm product that is part of DOMINO2:

Acarreta, J. R., J. F. De Haan, and P. Stammes (2004), Cloud pressure retrieval using the O2–O2 absorption band at 477 nm, J. Geophys. Res., 109, D05204, doi:10.1029/2003JD003915.

Stammes, P., M. Sneep, J. F. de Haan, J. P. Veefkind, P. Wang, and P. F. Levelt (2008), Effective cloud fractions from the Ozone Monitoring Instrument: Theoretical framework and validation, J. Geophys. Res., 113, D16S38, doi:10.1029/2007JD008820.

In line 22 on the same page 6348, the authors give reference to Lin et al. 2014. However, in the reference list of the article, there is no such reference to be found; a proper study to cite in this context would be Leitao et al. 2011.

Thank you for checking the reference and giving another good study to cite. We also noticed that it is missing in the references just after the paper was accepted for ACPD. We add the reference and also cite the paper of Leitao et al. 2011.

In line 24 on the same page 6348 the authors should remind the reader that the observational uncertainty they are talking about to actually refers to the redefined obsertational error E_{obs} from Eq. 1.

Thanks for the advice. We make a reference here to Equation 1.

In lines 3 f. on page 6349, the authors should explain why haze around the Bohai Bay [. . .] indicates that the high aerosol concentrations are near the surface.

We change the sentence to “The RGB image of MODIS shows haze around the Bohai Bay, which indicates that high aerosol concentrations are present in that area.” The height of the aerosols is in fact not important, we just want to stress that high aerosol concentration may have affected the NO_2 retrieval products.

In line 12 on the same page 6349, the authors should explain why they chose the filter criterion they mention, and how they arrived at this criterion.

An OmF filter is a common method in data assimilation to filter out the outliers. In lines 13-27 on page 6349, we explained why we use this particular criterion.

In line 17 on the same page 6349, the authors should explain why the longer NO_2 lifetime can lead to larger OmF values in winter.

The lifetime of NO_2 is much longer in winter than in summer. Therefore, the NO_2 concentration is higher than in summer. Assuming the relative error of the observation is the same in winter and summer, this leads to larger OmF values in winter time. We add a short explanation in the paper.

In lines 13-19 on the same page 6349, the authors should discuss what it means for their conclusions that the distribution of the OmF in winter is clearly not Gaussian (see the heavy tail to the right in Fig. 6b), given that they explicitly state on line 7 of the same page 6349 that /In the data assimilation it is assumed that the OmF distribution is Gaussian.

In data assimilation, the OmF is assumed to be Gaussian distribution. The figure shows that the tails of distribution on both side do not follow the good Gaussian distribution. Therefore we use this OmF criterion to filter out those heavy tails. The distribution after applying the filter show a better Gaussian shape. In the paper, we add that the tails are not Gaussian and filtered.

In lines 9 f. on page 6350, the authors write that /when the pixel size of the satellite is twice that of the model grid cell, the updates of emissions in that grid [cell] will even be doubled/. This statement again is not understandable to a reader who is not familiar with DECSO, so the authors should explain why this is the case. As a side note, to a reader not familiar with DECSO, this sounds like a serious flaw of the DECSO algorithm, so I do see the necessity to explain.

This is probably not explained very well by us, since this statement is completely unrelated to the DECSO algorithm. It is simply that the back-of-the-envelope calculation is done for emissions and concentrations in a single grid cell. If the satellite observation measures an average concentration for twice this area, the total amount of NO_2 will be twice as high and therefore the emissions have to be twice as high to explain this amount of NO_2.

On second thought, we decided to remove this remark in the paper to avoid confusion for the reader.

In line 13 on page 6351, the authors write that they include this in the SD. This statement is not understandable. What is SD? Why do they chose to include the trend in the SD? What does this even mean?

It is the rule of ACP that Standard Deviation is SD and it is automatically changed. We saw that the small trend of NO_2 can be neglected compared to the standard deviation of the NO_2 concentration from year to year.

We replace the previous sentence with: "Although a small increasing trend from 2005 to 2011 is visible in the satellite data, it is negligible compared to the SD of the natural variability."

In line 16 on the same page 6351, the authors speak of a small decrease in [. . .] February. However, I do not see any decrease in Fig. 9 in February.

Compared to the NO_2 concentration in Jan. and Mar., the NO_2 concentration in Feb. is lower than in these two months.

In line 19 on the same page 6351, the unit molec cm-2 cannot be correct with a number 6.6.

Yes, the referee is correct. We change it into 6.6 10^{15} molecules cm^{-2}.

In lines 20 f. on the same page 6351, I the authors write that consequently [. . .] NO_2 concentrations of the following months are also lower than in previous years. Given the short lifetime of NO_2 in the atmosphere, I do not understand the causal connection implied by the authors' use of the word consequently. It would be nice to hear the authors' interpretation of this: Were the pollution control measures prolonged by the authorities? Were they voluntarily continued by the population? Is this a mystery? Also, the authors should define the following months, given that they never clearly stated their study period.

Considering the comments of the referee, we remove the word "consequently". We change the previous sentence into "Due to the effect of the continuous air quality regulations during the YOG and afterwards, the NO_2 concentrations of the following months are also lower than for previous years."

Several measures taken by government were continued, especially related to NO_2. In Table 1 we have underlined the regulations with a permanent character. Also some less well documented technical improvements have been implemented. At the end of page 6339 we added:

“In addition, several technical improvements have been implemented to reduce pollution from heavy industry and power plants.”

In line 27 on the same page 6351, the authors should cite previous studies showing that differences [. . .] can be attributed to the meteorological conditions.

We cite a previous study here.

Lin, W., Xu, X., Ge, B., and Liu, X.: Gaseous pollutants in Beijing urban area during the heating period 2007–2008: variability, sources, meteorological, and chemical impacts, Atmos. Chem. Phys., 11, 8157-8170, doi:10.5194/acp-11-8157-2011, 2011.

The authors have to modify the conclusion they give in lines 12-16 on page 6353. From my understanding, the lack of observations in the second half of August 2014 means that it is impossible to decide if the emission reductions shown by DECSO for September 2014 actually occurred in August or September 2014. While I agree that it is highly probable that the emission reductions did indeed occur in August as a consequence of the implemented pollution control strategies, from a scientific point of view, it is impossible to draw this conclusion without doubt. I believe it is absolutely necessary to explicitly state this uncertainty of the time of emission reduction. Also, looking at Figure 3, it seems that there are no measurements in the Month of September, except for the last days of the month. What is the implication of this for the conclusions? And again, the statement cannot be understood by a reader unfamiliar with the DECSO algorithm, so the authors should add one or two sentences about this.

We agree with the referee that although highly probable we are not sure the emission reductions did occur at the end of August. There are several reasons that makes it probable:

- Although in Figure 3 no measurements appear in September, this Figure shows only observations directly over the center of Nanjing, while the DECSO algorithm is using all measurements in the neighborhood that have been transported to or from the Nanjing region. This most important feature of DECSO has now been emphasized more in the text.
- Reductions in emissions at the end of August or the following months can appear with a time lag in the Kalman filter results (see e.g Brunner et al., 2012). This time lag is not fixed but depends on the amount, interval, accuracy and distance of the observations and it is therefore difficult to quantify. In future research we intend to reduce this time lag by using a Smoothing Kalman Filter technique.
- Due to the fact that we use monthly mean values and the Olympic Games took place at the end of the monthly period, the effect will be less obvious in August because of the first half of the month having normal NOx emission levels. Since many regulations for

NOx had a more permanent character the emission reduction is better visible in September.

We changed the text at page 6353, line 8-10 into:

“This reduction is <u>probably</u> caused by <u>the more permanent</u> air quality regulations taken by…”

And line13:

“This is partly a consequence of the use of monthly means, while the regulations became active at the end of August. It is also a consequence of the lack of……..”

Also the conclusion given in line 20 on the same page 6353 has to be modified, as DECSO did not detect any emission reduction for the YOG period, as there were almost no measurements during the YOG period. The conclusion has to be softened.

We soften our conclusion in the revised text.

“We conclude that the NO_x emission reduction detected by DECSO **for the YOG period and afterwards** was at least 25%, showing that the air quality regulations taken by the local government were effective.”

In line 20 of page 6354, the authors should explain why the legislative measures which were only in place until 31 Aug 2014 (see Tab. 1) still effect NO_2 concentrations in the following month, given the short lifetime of tropospheric NO_2.

There are several regulations still effective after the YOG. We underline those regulations in table. 1

In line 27 on the same page 6354, the authors should explain how they arrive at a resolution of 50 x 90km; the spatial resolution of the model was not explicitly given before.

We mentioned the model resolution in Section 2.1 P6342 line 9. The model has a spatial resolution of 0.25x0.25 degree. The solid rectangle showed in figure 8 is about 50x90 km^2. We add this in the caption of the land use figure to make it more clear to the readers.

In lines 12-15 on page 6355 (or earlier, when introducing the concept of the OmF filter), the authors should elaborate why they chose an OmF filter and do not explicitly filter out scenes contaminated by high aerosol loads, using OMI or MODIS AOD measurements.

As shown by Lin et al. (2014) the relation between aerosols and cloud retrievals are complex and non-linear. It is therefore not straightforward to filter deviating NO_2 retrievals based on aerosol information. It depends on the type of aerosols and the concentration as shown by both Lin et al. (2014) and Leitao et al. (2010). The study of the effect of aerosols on NO2 retrievals and how to filter or to improve these retrievals is topic of future research. On the other hand, filtering of outliers in data assimilation by using an OmF filter criterion is not uncommon. The reason why we have not applied such a filter in previous versions of DECSO is already explained in the text. However, in this case we have applied a very cautious version of the OmF filter, which avoids building a complicated filter based on aerosol information of type, concentration and its interaction with clouds.

In the text on page 6349 we have added: “The effect of high aerosol concentrations on the NO_2 retrieval is non-linear and depends strongly on both the type of aerosol and its concentration (Lin et al., 2014, Leitao et al., 2010). It is therefore difficult to filter out outliers in the observed NO_2 based on aerosol data.”

In line 24 on the same page 6355, the authors write about high electricity consumptions by power plants. They probably mean high electricity production of power plants.

Yes, the referee is correct. We modify it in the paper.

In Table 1 on page 6361, it is not clear if earlier regulations were complemented or replaced by later regulations. Also, the authors should give reference to the source of information.

The time in the table shows the start time for different regulations. Some of them will be effective after the YOG. We change the table in our paper to make it clear. We got the information in this table from a large range of media, newspaper and internet sources.

In the discussion of Table 2 on page 6362, the authors should clearly state on which grounds they decided on the redistribution factors, or they are arbitrary.

We estimate the factor table for the situation of china to the best of our knowledge.

The caption of Table 2 is changed to “Table 2. The estimated redistribution of MEIC sectors over SNAP 97 sectors”

The sentence in line 3 page 6343 is changed to “[…] in the CHIMERE model, we estimate the redistribution of the emissions over the sectors (see Table 2). ”

In Figure 1 on page 6363, the authors should clarify if the x axis is sun local time or time zone local time.

Sun local time and time zone local time is the same in Nanjing. But we add sun local time in the figure to make it more clear.

In Figure 2 on page 6364, the colorbar does not have any units. What do the colors mean?

The colorbar represents the frequency of satellite observations for that specific value of OmF. We add this in the caption of Figure 2.

In the same Figure 2, it is unclear what is actually shown. Are these time averages of the whole period Jun-Aug? All individual satellite measurements? All individual model grid cells? What is the spatial domain used for this Figure?

They are all individual satellite measurements over the whole Asian domain from Jun. to Aug.

We change the caption of Figure 2 into:

“Figure 3. The comparison of the absolute OmF (10^{15} molecules/cm^2) of CHIMERE v2006 and CHIMERE v2013 for the whole East Asian domain from June to August . The color represents the frequency of satellite observations for that specific value of OmF. ”

For Figure 3 on page 6365, the authors should explain the meaning of the dotted horizontal lines. Also, it is not clear which spatial domain this Figure refers to. Finally, it is not clear what σ_{obs} refers to - is this the same as E_{obs} from Eq. 1?

We remove the dotted horizontal lines in Figure 3. The time series of the OmF is for the single grid cell over the center of Nanjing. We added this to the caption. The error bar is the root mean square error of observations (E_{obs}).

Here is the new caption of figure 3:

“The time series of the OmF from January 2013 to September 2014 for the single grid cell over the center of Nanjing. The error bar is the root mean square error of observations (E_{obs}). ”

Figure 5 on page 6367 lacks any proper reference to the source of the images

We mentioned it in the acknowledgement. But we also add the link to the figure caption.

https://ladsweb.nascom.nasa.gov/

In Figure 7 on page 6369, the authors should clarify if they mean sun local time or timezone local time. Also, given that the vertical bars are not errors but show the natural variability within one month, maybe the authors should not call the bars error bars.

We clarify the local time is sun local time. Indeed the vertical bars show the natural variability and are not error bars. We change the caption of this figure.

In Figure 8 on page 6370, it might be helpful to see a second subplot showing the effectively chosen land cover type for each grid cell, i.e., the scaled-down version of the same data.

CHIMERE model use the information of the percentage of different land use categories. Figure 1 (formerly Figure 8) shows the land use information used by CHIMERE.

In Figure 9 on page 6371, again the authors write error bars even though the vertical bars do not contain error information but show the natural variability for a single month. Also, SD should be spelled out as standard deviation.

We agree with the referee and change the caption. We wrote standard deviation. But the SD is a rule of ACP and automatically changed.

Figure 10 on page 6372 shows that there is a problem with the error of the mean NO_x emission estimate from DECSO (shaded areas): For August 2014, the minimum value of the shaded area is higher than the maximum value for September 2014, i.e., there is no overlap between the credible regions of the emission estimate for August and September 2014. However, the authors do draw the conclusion that the emission reductions seen in September actually happened in August already. I see a contradiction here: Either, the authors' conclusion is correct; in this case, the errors shown as shaded areas in Figure 10 are clearly too small. Or, the errors shown in Figure 10 are correct, but then the authors' conclusion would be merely speculation, as it would not be backed by the error estimates.

Reductions in emissions at the end of August or the following months can appear with a time lag in the Kalman filter results (see e.g Brunner et al., 2012). This time lag is not fixed but depends on the amount, interval, accuracy and distance of the observations and it is therefore difficult to quantify. Although the strong point of a Kalman Filter is its detailed error analysis, this time lag is not incorporated in its formalism. Thus the error of DECSO does not account for the delay. There is no good procedure to calculate the error due to this time lag. In future research we intend to reduce this time lag by using a Smoothing Kalman Filter technique. We add more explanation in our paper.