Dear Ruixiong,

I have now received reviews of your revised manuscript from both reviewers. One of the reviewers has entered his comments in the wrong field so I attach it below for your reference.

As you will see, both reviewers are not at all satisfied with the manuscript and the changes you made. After reading the manuscript again carefully, I agree with their assessment: the manuscript as it is cannot be published in AMT.

There are three main reasons for this conclusion:

1) The manuscript lacks important information. Even after reading it several times, it is for example not clear to me

- If and how the surface measurements were sampled in time to match the overpass of the OMI data
- If and how the OMI data were sampled in space to match the surface data
- Whether a constant row filter removing rows 26-55 was used or if the (time dependent) flagging in the DOMINO product was used (both is stated in the text).
- How exactly the drift over the ocean was accounted for in the data analysis
- Which version of the AMF lookup table was used (you make reference to a recent paper by Lorente et al. for DAK but it is not clear if you use the DOMINO LUT or a more recent version from the QA4ECV project)

2) The explanations given in the manuscript for some of the observed effects are unclear or not convincing:

- The offset correction using data over the ocean is an interesting idea and is in fact used in many satellite data products. However, here the tropospheric slant column is used, which is the result of subtracting the assimilated stratospheric NO2 column from the retrieved slant column. Any global drift in the OMI NO2 slant column data would be absorbed by the data assimilation. The remaining (very small) drift is probably indicative of some problems in the stratospheric correction which would also explain the large variability (note that even negative columns occur in some months) and is certainly not representative for offsets in NO2 slant columns over all of the US. This is the reason why the authors do not apply monthly corrections which they should if they would trust the offset correction approach.
- The explanation given for the offset trend (increased striping) does not make sense for two reasons: First, striping is by definition deviation from the mean value and thus should not contribute to a drift in the mean, and second, I assume that the authors use destriped data (but also this information is not given in the manuscript).
- The albedo trend correction is an interesting result, but little is said about what the reasons for the differences between MODIS and TEMIS albedo values are. It also is not clear to me if MODIS albedo trends of the order of 0.5% per year are significant.
- The lightning filter makes a lot of sense if satellite and surface observations are to be compared. However, the results are puzzling and in contrast to the conclusions drawn in the manuscript: As is evident from Table 1, surface station trends are more affected by the lightning filter than OMI data with the exception of the West, where only little lighting is found over the regions having NO2 above the threshold of 1E15 molec/cm2. The clear

improvement in consistency of the two data sets in the South when applying the filter is mainly the result of a change in in-situ trend! In that sense, the lightning filter is more a filter on the surface data than on the OMI data. This result points at sampling issues in the surface data, not inadequacy of the standard OMI product.

3) The underlying assumption of the whole manuscript is, that trends of surface NO_2 in-situ observations should agree with OMI NO_2 column trends. As both reviewers point out, this appears to be an apple and oranges comparison, and while it is interesting to compare these two trends, it is by no means clear that they should be exactly the same. As in part pointed out in the manuscript, there are several reasons why they could be different, including representativity of the measurement location (see Fig. 9), vertical NO_2 distribution, non-linearity of NO_x chemistry and different sensitivity to changes in boundary layer height.

In my personal opinion, the better agreement between surface in-situ trends and the OMI trends after applying the corrections you suggest is a) a coincidence and b) not significant within the uncertainties of the method and the comparison.

In order to make this manuscript publishable in AMT, you need to

- Add the missing information and clarify the details of your approach
- Reconsider your discussions, explanations and conclusions in view of the comments made by the reviewers and listed above (or convince me that I have not understood your arguments)
- Rephrase the manuscript in order not to oversell the achieved improvement in consistency as proving that your corrected OMI time series is more correct than the original one, unless you can show that this is the case.

These are major revisions and as both reviewers have declined to see another version of this manuscript, I will have to be satisfied with the next version or I will have to reject the paper.

Best regards,

Andreas

==================== Report of anonymous referee # 1 ================================

Unfortunately, the authors didn't change the manuscript in several passages that could have been clearer. Also, I very strongly disagree with the authors' comment that "All observation data need to be corrected when they are used for trend analysis." In its generalness, this is not scientific, in my opinion. This is only the case if one wants to extract information from measurements that was not originally measured, such as comparing apples and oranges. Also, the manuscript still leaves the impression that it is "wrong" of the OMI VCD trends to not be identical to the in-situ measurement network trends, which is still a comparison of apples and oranges in my view.

Having said that, I don't see any willingness by the authors to accept my views or change the manuscript accordingly (which would be possible without harm to the original message / scientific content of the manuscript). Unfortunately, the authors chose not to make it more balanced paper. I still don't like the paper very much, and find it to be rather unbalanced.

However, given that I only asked for "minor revisions" in the first place, and that the authors did address the most severe points, I don't see any grounds to not publish the article. I wouldn't know what to ask for in a potential revision of the manuscript -- I could raise the same points again as I raised in the original review, and the authors still wouldn't agree.

Given that the actual technical modifications to the OMI retrieval are interesting and should be made public to the scientific community, I don't oppose publishing the paper. I just don't like it.

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