Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-536-RC2, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of daily meteorology on the interpretation of space-based remote sensing of NO_2" *by* Joshua L. Laughner et al.

Anonymous Referee #2

Received and published: 8 September 2016

Review of "Effects of daily meteorology on the interpretation of space-based remote sensing of NO2" by J. L. Laughner et al.

In this manuscript, the authors investigate the impact of using daily instead of monthly high resolution a priori NO2 profiles in satellite retrievals of tropospheric NO2 plumes used for emission estimates. They explore qualitatively what the expected changes are, investigate the effects more quantitatively in simulated retrievals and finally apply daily a priori profiles to a limited set of real OMI data.

The manuscript is well written and nicely explains the basic effects and how they impact on the results. The topic of study – improvements of NOx emission estimates from satellite NO2 measurements – is relevant for the community and the relatively large

sensitivity reported is important for the interpretation of existing and future studies. As the paper is more on the technique and less on the impact on atmospheric chemistry and physics I think it would have been more appropriate for AMT but it also fits into the scope of ACP. I therefore recommend it for publication after revisions taking into account my comments listed below.

1. While I see the nice qualitative discussion of the effects as a strong point of this manuscript, the quantitative results are much less convincing in my opinion. The reason for my scepticism is the large change in emissions and lifetimes the authors find when changing their a priori spatial or temporal resolution in combination with the large uncertainties given in the tables. Most of the results are in agreement within uncertainties when changing from monthly to daily a priori in spite of the large changes seen. To me this indicates that the time period used for averaging is too short to really separate the effect of a priori changes from noise in the data, and as the authors acknowledge, all previous studies used much longer averaging times. Please comment on the magnitude of uncertainties and the significance of differences seen.

2. In the discussion of the results both in the text and in the abstract, I'm confused by statements such as "Comparing an optimized retrieval to a more standard one, we find that NOx emissions estimated from space-based remote sensing can increase by 100% when daily variations in plume location and shape are accounted for in the retrieval." If I'm not misinterpreting Table 4, the change in emissions when moving from monthly to daily a priori is closer to 45% and actually is a decrease, not an increase of estimated emissions. This also makes more sense considering the qualitative discussion given in the first part of the paper. The factor of 2 increase is relative to a low spatial resolution a priori which is also interesting but not the focus of the study and also not what is suggested by the formulations in the text. I think these statements need to be rephrased.

3. The discussion of standard mathematical methods for fitting a function to the decay curve is a bit out of place in such an article, in particular as the method used for emission estimation is not the topic of the paper. I would suggest to shorten this part and to remove the discussion of Matlab internals which are of little interest to the reader.

4. The approach taken to averaging the model data in time (Equation 5) appears overly complex and not transparent. The obvious way to treat this problem is simple interpolation in time to the OMI overpass.

5. On a more general note I think that the paper would benefit from a short discussion of the impact model errors could have on the results. It is obvious that in theory, using daily a priori data is better than using monthly averages as the process of NO2 retrieval is not linear. However, in real data this might not necessarily be true. At the high spatial resolution of the model used here, even a small uncertainty in wind direction, emission height or emission source location can move the NO2 plume into different model grid cells, potentially leading to poor matching of plume position in measurement and model and thus wrong air mass factors and NO2 columns. I find this an interesting topic in particular in view of future instruments having improved spatial resolution.

6. Equation 2: $w_s(p)$ is later written as $w(p)$

7. Equation 4: Please add how cloud radiance fraction was computed

8. Page 6: Add reference for MOZART model

9. Page 7, line 5: Cloudy AMF is smaller, not lager for boundary layer NO2 profiles

10. Page 14, line 17: something missing here?

11. Conclusions, line 28: Please add again that you count days even if only a single pixel shows a change larger than the noise (which I personally find a strange way of counting)

12. Pageline 6: I do not understand the sentence “Further work is needed to understand the impact of this change on top down constraints of NOx emissions, given the recent work showing that bottom up estimates are high by 50%”. I think the relevance is obvious if you believe your own results – all previous estimates based on this technique and using monthly high resolution a priori profiles give too high emission estimates. Whether or not the emission inventory is off is another topic (which could of course also impact on your AMF values and thus emission estimates) and should not be mixed here.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-536, 2016.