

## ***Interactive comment on “Evaluation of a regional chemistry transport model using a newly developed regional OMI NO<sub>2</sub> retrieval” by G. Kuhlmann et al.***

### **Anonymous Referee #1**

Received and published: 8 January 2015

General Comments: The paper discusses a detailed comparison between OMI tropospheric NO<sub>2</sub> column retrievals, the RAQM surface monitor network, and the Hong Kong high resolution CMAQ modeling system. The authors developed their own OMI retrievals by using their own model to replace ancillary parameters in the OMI NO<sub>2</sub> Standard Product v2. Considerable effort has been expended in testing retrieval parameters to develop an optimized product for the Pearl River Delta. In my opinion the paper is not ready for publication in its present form, and a major revision is needed. The main reasons for this judgment are the following:

1. The paper has problems with satellite retrieval naming conventions, proper

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acronyms, and there are considerable grammatical errors. Most notable of which is the need of a consistent and accurate naming of OMI-related products. There are also several paragraphs with mixed past/present tense. See the Technical Corrections (below) for more detail.

2. The paper is too long and should be shortened. Some sections are especially long and need to be summarized more succinctly and there are many instances of redundancy. The Appendix could be made a table. One or more of the Tables and/or Figures could be moved to the supplemental section. The PRD-3 scenario could be removed from the comparison or moved to the supplement.

3. There needs to be more effort comparing the results of the model evaluation with other related studies in the introduction, discussion, and conclusion sections. This applies to both model results and custom retrieval results. In general, more perspective on how the comparison results are related to other studies in the literature would be of significant value.

#### Specific Comments:

1. This paper would highly benefit from an additional comparison, so that the community can put the results into perspective. Specifically, I suggest that the authors use the scattering weights in the NASA SP2 and apply them as averaging kernels, which is what users in the community are asked to do for a “best” comparison to correct for the differences between their model and the OMI a priori NO<sub>2</sub> profile. Additionally, if possible, it would be of value if the DOMINO product was tested as well (with the averaging kernel). Although the two products have somewhat converged in comparison over the years, they are significantly different in high aerosol loading regions.

2. Throughout the paper, the OMI a priori NO<sub>2</sub> profile is loosely referred to as the NO<sub>2</sub> profile. Also, the idea of replacing profiles is discussed, but there is no mention of “averaging kernels” or the differences in studies that compare models and satellite retrievals with/without said kernels. I feel this is pertinent to the discussion of the paper,

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especially since the authors have used an impressive complex procedure, but have not quantitatively compared it to what other members of the community have done to account for model/retrieval inconsistencies. I have suggested changes in the technical corrections to address some of these issues, but the authors should take care to further review their descriptions of other studies and their relation to the HK PRD retrieval both in method and quantitative result. Further literature review and communication with the OMI user community would also further that effort. Furthermore, in general there should be more use of the word “tropospheric”, to distinguish the actual NO<sub>2</sub> product you are using, as well as more discussion of what an expected “real” profile of tropospheric NO<sub>2</sub> might look like in your region. A better assessment of the biases you expect vertically through the atmosphere, due to poor a priori profiles, should be included as well.

3. The literature review mixes discussion of results from old and new versions of OMI NO<sub>2</sub> standard retrievals. It is my opinion that the authors need to take special care to quantitatively compare their results only to other studies in the literature that used the v2.1 data. Furthermore, I would suggest that they focus the literature review of model/satellite comparisons that used the v2.1 data since both DOMINO and OMNO<sub>2</sub> SP are significantly different retrievals than in v1. This will help to preserve consistency of the impact this analysis will have on the community.

4. The authors describe a regridding/projection process for mapping the OMI retrievals to their fine resolution model grid. There is a 0.01 x 0.01 degree grid that is first used, which is then gridded again to a 3 x 3 km grid. It would be helpful if the spatial difference in these two grids was compared with the same units in some fashion. In general, I feel a better explanation is needed to explain why you need an intermediate grid at all.

#### Technical Corrections:

Throughout the paper, there is an inconsistent use of product names and OMI-related terminology. I have made suggestions as to how those can be corrected, but I suggest

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that the authors use a unique name for their retrievals that distinguishes itself from other commonly used acronyms in the community. There is the Derivation of OMI tropospheric NO<sub>2</sub> (DOMINO) v2.1 product, the NASA OMI NO<sub>2</sub> (OMNO<sub>2</sub>) v2.1 Standard Product (SP) (“collection 3”), and your custom product which I feel should be called the Hong Kong OMI NO<sub>2</sub> (HKOMI) product.

The usage of those product names should remain consistent throughout the paper. Related to this, I feel you should use the terms “custom” and “standard” (or something similar), rather than “global” and “regional” to distinguish between retrieval types. Take care to identify the proper data level when necessary (e.g. a custom level-2 product that is further processed to match your model grid). Typically level-3 products are global products, while a level-2 product is swath-level not global. You also interchange the use of “product” and “retrieval” in this paper. Discussion of the “retrieval” should be limited to details of the actual retrieval process. Discussion of the “product” should be limited to details of the final results. Furthermore, you should not call a satellite retrieval of tropospheric NO<sub>2</sub> a “measurement”. These are all “products”, and amongst the community the NASA product is often referred to as just the “SP”, but for publication I think it is appropriate to use the actual dataset name.

In my opinion the title should be changed to reflect the naming conventions. For instance, I feel that a better title might be “Evaluation of a regional chemistry transport model using a custom OMI NO<sub>2</sub> retrieval for Hong Kong”.

The paper has many inconsistent uses of tense, plurals, and other simple grammatical errors. Although I have provided a supplement of suggested corrections, they are not a complete list of needed grammatical edits. The authors should take care and properly revise the manuscript for formal publication as needed.

See markup and specific technical suggestions in the pdf supplement.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/14/C10964/2015/acpd-14-C10964-2015-supplement.pdf>

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