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Interactive comment on “Improved satellite retrievals of NO₂ and SO₂ over the Canadian oil sands and comparisons with surface measurements” by C. A. McLinden et al.

Anonymous Referee #2

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1 General comments

The manuscript "Improved satellite retrievals of NO₂ and SO₂ over the Canadian oil sands" by McLinden et al. gives a thorough description of numerous improvements made to the operational OMI NO₂ and SO₂ retrievals in order to be more accurate over Canadian oil sand regions. The paper is well-written and gives a detailed account of the various improvements implemented by the authors, warranting publication after a number of issues have been addressed.

My main concern is the choice of journal for publication. In my opinion, the paper is of C9296

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rather technical nature. While many improvements to operational retrieval algorithms are introduced, the paper does not provide new insight about the Earth's atmosphere and its underlying processes. I personally would be happier to see this article published in AMT, but maybe that's a matter of preference.

Either way, the following questions should be addressed by the authors before publication:

2 Specific comments

1.) On p. 21620 the authors describe their approach to deal with the temperature dependence of the absorption cross-sections. They state that for both NO₂ and SO₂ they use the exact same correction formula. However, it seems implausible to me that both cross-sections should exhibit exactly the same linear dependence on temperature. Some information on how this correction formula was derived, using which laboratory cross-sections, is needed to justify the chosen α .

2.) In the discussion of the validation of the VECTOR RTM on p. 21627, the authors state that the resulting AMFs differ by less than 3% on average and claim that "this agreement is acceptable given that not all input parameters were identical". Without further information, the reader starts wondering why not all inputs were chosen identically, and if the agreement could actually be worse if all inputs were identical. So either the authors should repeat the validation with identical inputs to both RTMs or, if not possible, they should justify why they think the differing inputs lead only to the observed 3% mismatch and are not causing more trouble.

3.) In the EC-AMF sensitivity study (on p. 21631), the authors evaluate the influence of doubled PBL concentrations on the AMF and come up with a 6% decrease of the AMF. They claim that further increasing the NO₂ burden of the PBL would not significantly change this due to some saturation effect. However, the authors fail to substantiate

this claim by further AMF calculations for, say, a 3- and 4-fold increased NO₂ burden of the PBL. Also, it would be interesting to know by how much emissions have actually increased since 2006.

4.) On p. 21635, the authors try to convert surface vmr to VCD, using CTM profiles. They assume that "the model can adequately capture the spatial and temporal behaviour of this ratio." However, the largest model uncertainties will be due to uncertainties in the emission inventories, impacting mainly the PBL and lower atmosphere. Therefore, I believe that the authors' assumption is not valid. It might be a better approach (and in line with the sensitivity study of the EC-AMF to PBL concentrations) to take the free troposphere profile from a CTM, only varying the PBL content according to the surface vmr, but maybe the authors can better justify their assumption or find a more elegant solution.

5.) In Figure 3, the authors compare their albedo product at the SO₂ wavelength for 2005 and 2011. There is a large difference visible between the two plots. The authors should try to explain this trend in surface albedo in the discussion.

3 Technical comments

p. 21614/l. 11: The reference "Nowlan et al. 2011" is not included in the reference list at the end of the article.

p. 21617/Fig. 2: As absorption happens on individual molecules, I would find it more instructive to see the vertical profiles as number density instead of vmr. Also, the shape factor could be calculated using number densities instead of vmr.

p. 21617/l. 7: At the high latitudes of the study area, the LT of OMI overpass is shortly before 13:00. However, I don't expect this to significantly influence the results.

p. 21622/l. 12: For the GOME LER climatology, reference to Koelemeijer et al. 2003

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should be given.

p. 21625/l. 29ff: As the authors state, the discussion of snow albedo seems misplaced in this study. I would prefer if the discussion would not be part of this manuscript. If the authors need treatment of snow albedo in a later study, they should include the discussion then.

p. 21626/l. 11: Proper reference to the data product (publication, dataset name, data availability) of the used O3 columns should be given.

p. 21626/l. 19: A good reference for the aerosol influence on AMFs depending on their relative positioning is Leitão et al. 2010 (doi:10.5194/amt-3-475-2010).

p. 21626/l. 22: Please give a link/reference to the For McMurray sun photometer measurements.

p. 21630/Eq. 8: The numerator in the third summand under the root should be $\frac{\epsilon_{Sb}}{M}$ instead of $\frac{\epsilon_b}{M}$

p. 21635/l. 26: The authors should provide information about the temporal representativeness of the WBEA measurements. Are daily averages taken, or only measurements of the LT of OMI overpass (which can be between 12LT and 14LT, depending on the viewing azimuth angle), or OMI overpass +- X minutes, or ...

p. 21636/l. 17: The authors refer to Equation (4), but Eq. 4 is actually about albedo. Either I don't understand why albedo plays a role here, or the reference is wrong.

p. 21649/Table 1: The authors speak of 30 altitude steps from 0 to 16km in 0.5km layers. However, going from 0 to 16 in 0.5 steps yields 33 layers?

p. 21654/Figure 2: In the caption, the authors speak of "number density profiles", while the plot labels speak of "volume mixing ratio". Which is correct?

p. 21655/Figure 3: A fifth panel, showing the data of (b) but integrated to the coarse spatial resolution of (a), would make it easier to compare the authors' albedo calcu-

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lation to the original Kleipool et al. climatology, as the effect of spatial resolution and different albedo dataset would not be combined into one plot.

p. 21657/Figure 5: The OMI snow albedo in (a) should be shown at the product's original spatial resolution, without interpolation (as in Fig. 3a).

p. 21662/Figure 10: The WBEA stations are not really identifiable in the plots. The dots should be made bigger, and the caption should say that the plot actually shows the WBEA stations. (Or, alternatively, don't show the stations in this Figure, and fix the reference in the text to point to S1 instead).

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