

## ***Interactive comment on* “Validation of ACE and OSIRIS ozone and NO<sub>2</sub> measurements using ground-based instruments at 80° N” by C. Adams et al.**

**C. Adams et al.**

cadams@physics.utoronto.ca

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Thank you for your comments, which have helped us improve the manuscript. We respond to each of the points below, with the referee’s comments given in italics.

General comments:

*This paper deals with a comprehensive comparison of satellite ozone and NO<sub>2</sub> measurements by OSIRIS/Odin and ACE/FTS, ACE/MAESTRO with several different ground based instruments including DOAS, SAOZ, Brewer spectrometers as well as a Fourier Transform Spectrometer operated at the PEARL station at Eureka in Northern*

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Canada. I have no really major objections to the publication of this paper, but ask the authors to consider the following comments. I would also like to point out that I find the paper very well written and easy to follow.

The paper presents a very comprehensive, in-depth validation study of Arctic ozone and NO<sub>2</sub> satellite measurements using ground-based data sets measured at PEARL, Canada. The very thorough discussion of the inter-comparison unfortunately though leads to a rather complicated and somewhat tiresome and lengthy read of the paper. The actual message is a bit lost in all the detail but the conclusion section does pull the manuscript reasonably well together and focusses on the central findings. In my opinion, it would clearly strengthen the paper if the authors would choose a little more selectively what they discuss and/or highlight. One suggestion on how to shorten and possibly focus the manuscript further is discussed below under "Specific comments" (2nd last comment).

See response under 2nd last specific comment. We will also shorten the discussion in Section 7, by eliminating the 3rd paragraph (starting at page 548, line 26). The opening to the 4th paragraph will read:

"The selection criteria of Batchelor et al. (2010) could not be applied directly to the DOAS (GBS and SAOZ) and OSIRIS datasets because only pressure levels were available for these DMPs. Furthermore, the imposition of dynamical coincidence criteria at altitudes up to 46 km reduced the comparison statistics. Therefore, a new set of dynamical coincidence criteria were developed. The best results were obtained when dynamical coincidence criteria were imposed only in the lower stratosphere where the bulk of the ozone column resides, at 131 hPa ( 14 km), 72.5 hPa ( 18 km), and 53.9 hPa ( 20 km). The difference in temperature. . ."

Minor comments:

*Abstract, page 520, lines 5-8: Only discuss and show the ozone springtime results*

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*including the proper coincidence criteria*

The spring-time intercomparisons were only performed for a subset of instruments over a shorter measurement period. Therefore, we presented ACE/Bruker intercomparisons in the abstract as an example of the impact of these coincidence criteria on the comparisons.

*Page 521, line 7: would be helpful if you briefly mention why that "isolation" matters here (e.g. 1 additional sentence)*

We will add the following text:

“Therefore, coincident measurement pairs can include one measurement inside the vortex, with, e.g., low ozone and NO<sub>2</sub>, and one measurement outside the vortex. This reduces the apparent agreement between two datasets.”

*Page 521, lines 15-16: Since mentioned here anyway, why not move the description of the 2 of the 3 stations (lines 27-29) to here and also introduce the 3rd station (maybe I missed this but I can't remember having seen an introduction to the 3rd location in the manuscript)*

The description of the measurement locations will be moved, as suggested.

*Page 521, line 17: replace "Beginning in : : :" with "Since : : :"*

This will be modified, as suggested.

*Page 521, line 26: "multi-year"*

This will be corrected.

*Page 522, lines 5-8: The statement is fine but the authors need to be careful*

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*when they explain which data set they use to validate the other one(s). Later in the conclusion section they say that "Satellite measurements were validated against four g-b ozone and 4 g-b NO2 data sets from PEARL." Hence, it is important that the g.-b. data was not in any form validated using the same satellite data sets. In section 5.3 the comparison between satellite and DOAS sounds like it goes a bit in that direction. It is important to make very clear that the satellite data is not used to validate the DOAS data which in turn is then used to validate the satellite data. As it stands, this is probably not as clear as it should be.*

We will update the text to make sure that the satellite measurements are not used to validate the other datasets. To clear this up, we will also change the section headings as follows:

5.3: Evaluation of NDACC DOAS settings → Comparisons with NDACC DOAS measurements

5.4: Evaluation of Bruker FTIR measurements → Comparisons with the Bruker FTIR measurements

6.3: Evaluation of DOAS measurements → Comparisons with DOAS measurements

6.4: Evaluation of Bruker FTIR measurements → Comparisons with Bruker FTIR

*Page 523, line 28: "For twilight periods when : : :"*

This will be changed as suggested.

*Page 524, lines 1-13: This should rather be discussed where the actual DOAS analysis is discussed in more detail and I hence suggest moving this paragraph to Section 3.*

The error discussion for the GBS/SAOZ measurements will be moved, as suggested.

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*Page 524, line 18: replace "the spring" with "each spring"*

This will be replaced, as suggested.

*Page 524, lines 18-23: If I understand the text correctly then there was only one SAOZ instrument deployed at any time, right? If this is correct then it might be better to change the sentence to ": : : the SAOZ instrument took measurements : : :. while in other years the SAOZ instrument was installed inside : : :"*

This will be changed, as suggested.

*Page 526, line 11-12: should read ": : : except when the AMF was limited : : : ." (?)*

This will be clarified to:

“Data were analyzed using the standard Brewer algorithm (Lam et al., 2007), with small changes to analysis parameters due to the high latitude of the measurements. The AMF was limited to be smaller than 5 instead of 3.5, which is acceptable under low ozone conditions and allows for more days with good data in the winter months. Furthermore, the ozone layer for the AMF calculations was set at 18 km instead of 22 km to better reflect Arctic conditions.”

*Page 530, line 2: ": : : were all fitted during : : :"*

This will be changed, as suggested.

*Page 529, line 16: the UV range of 380-390nm is extremely narrow, just 10nm – why?? How many absorption bands are included in a fit using just such a short wavelength range? How does the fit look?*

This was a typo! Data were retrieved in the 350-380 nm window. We will correct this in

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the text. This region was selected because these wavelengths were available for the UV measurements from 2007-2011, the fits looked good, and column amounts of NO<sub>2</sub> were reasonable. The figure below shows a couple of sample fits from (panel a) 30 March 2008 for the UT-GBS and (panel b) 30 March 2007 for the PEARL GBS both at SZA 90°.

*Page 523, line 26-27: The sentence: "These SAOZ Arctic AMFs produce total columns of NO<sub>2</sub>." is somewhat misleading; better to say something along the lines of: "These SAOZ Arctic AMFs are then used to convert the measured slant column densities into total vertical column densities."*

The text will be modified as suggested.

*Page 536, line 29: Based on Figure 3a, I would have thought that the maximum value (largest difference between twilight and noon NO<sub>2</sub>) is a bit later than around day 90, rather around day 110 but maybe this is just difficult to see properly in the figure.*

The error in scale factors depends on the ratio of twilight to noon measurements, instead of the absolute difference between twilight and noon (as shown in Figure 3). To clarify this, we will remove the reference to Fig. 3, and change the text:

“... with maximum values around days 90 and 240, when the ratio of twilight-to-noon NO<sub>2</sub> is largest.”

*Page 537, line 4: The diurnal effect is also called "chemical enhancement" and should be referred to as well e.g. with the reference of Hendrick et al., Atmos. Chem. Phys., 6, 93-108, 2006 and others.*

We will modify the text to include the terminology and references that you mentioned:

“In addition to affecting measurements taken at different times, the diurnal variation of NO<sub>2</sub> can introduce errors in individual measurements through the “diurnal effect”,

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which is also referred to as “chemical enhancement” (e.g., Fish et al., 1995; Hendrick et al., 2006; McLinden et al., 2006; Newchurch et al., 1996).”

*Page 539, line 27: ": : : and the Brewer instruments because : : :"*

This will be changed, as suggested.

*Page 540, line 24: ": : : different field-of-view : : :"*

We think that this should be the plural, fields-of-view.

*Page 542, line 29: Should be: (see Fig.8) not Fig. 7 (Fig 7 is dealing with the DOAS/satellite comparison not FTIR/satellite).*

We will correct the figure reference.

*Page 543, line 8: Suggestion: you could add "reduced" to make this statement a bit clearer, ": : : to reflect the reduced altitude sensitivity : : :"*

This will be changed as suggested.

*Page 543, lines 19-21 Section7: Since there have been other validation studies who have looked at this in detail before (especially Batchelor et al., 2010) you should in this paper only present the improved comparison after the corrections have already been applied and only briefly mention how much the application of the coincidence criteria have improved the comparison. It seems somewhat pointless to me to discuss a comparison of two data sets and their differences in detail if you already know how you can actually do it more realistically – and especially if you have already done it anyway. This would also help to shorten the very detailed and long comparison section a little. It is certainly important to mention, let's say in one or two paragraphs that this has improved the comparison in most cases (but not all) but it doesn't really*

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*make sense to me to discuss in such detail the comparison without the corrections.*

We will reduce the level of detail and replace the paragraph with the following text:  
“Batchelor et al. (2010) found mean relative differences between ACE-FTS v2.2 and Bruker FTIR ozone partial columns of -7.45

*Caption of Figure 3: Would be helpful to add here also that "Instrument abbreviations are given in Table 1".*

This will be added to the caption of Figure 3.

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Interactive comment on Atmos. Meas. Tech. Discuss., 5, 517, 2012.

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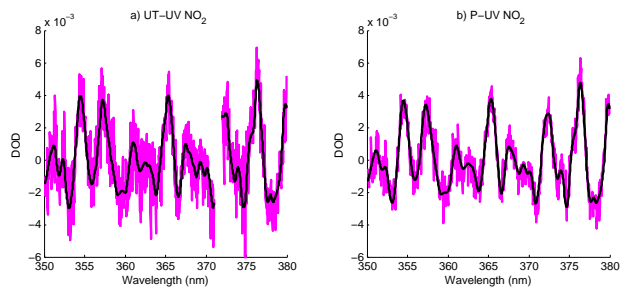
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Fig. 1.

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