

Interactive comment on “Validation of ACE and OSIRIS ozone and NO₂ measurements using ground-based instruments at 80° N” by C. Adams et al.

Anonymous Referee #2

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Review report on the manuscript "Validation of ACE and OSIRIS ozone and NO₂ measurements using ground-based instruments at 80° N" by C. Adams et al.

The paper presents a very comprehensive, in-depth validation study of Arctic ozone and NO₂ 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

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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).

This paper is recommended for publishing in AMT after the suggested revisions have been considered.

Specific comments:

Abstract, page 520, lines 5-8: Only discuss and show the ozone springtime results including the proper coincidence criteria

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

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)

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

Page 521, line 26: "multi-year"

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

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

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.

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

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 . . ."

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

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

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?

Page 523, line 26-27: The sentence: "These SAOZ Arctic AMFs produce total columns of NO₂." 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."

Page 536, line 29: Based on Figure 3a, I would have thought that the maximum value (largest difference between twilight and noon NO₂) is a bit later than around day 90, rather around day 110 but maybe this is just difficult to see properly in the figure.

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.

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

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Page 540, line 24: ". . . different field-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).

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

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

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

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

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