

Interactive comment on “Nitrogen dioxide observations from the Geostationary Trace gas and Aerosol Sensor Optimization (GeoTASO) airborne instrument: retrieval algorithm and measurements during DISCOVER-AQ Texas 2013” by C. R. Nowlan et al.

Anonymous Referee #2

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This article by Nowlan et al introduces a new imaging spectrometer that has been used for nadir observations as part of the DISCOVER-AQ multi-instrument campaign. The GeoTASO instrument is described as being developed for geostationary orbit for NO₂ vertical column retrievals using the DOAS technique, air mass factors and corrections applied through a CTM. GeoTASO was deployed on the NASA HU25C Falcon aircraft in a nadir view from a drop hatch, typically flying at 11 km. GeoTASO shows good agreement with ground-observations from Pandora on overpasses on polluted days (r

C5167

= 0.9), along with good agreements with GCAS ($r = 0.84$). This article fits very well with the expected content of Atmospheric Measurement Techniques and provides a solid description of the GeoTASO instrument and the analysis of this data complemented by several other remote sensing and in-situ instrumentation. It is our opinion that this paper should be published in AMT, following some revisions detailed below. There are unfortunately a number of gaps in the instrument description and algorithm which leave several uncertainties to its performance. These are considered as substantive revisions. Specifically these relate to the lack of information on instrument performance across the entire imaging area, co-adding steps, the lack of spectral fits shown, and the absence of a fuller description of the GCAS instrument. The data presented on FWHM and ILS of GeoTASO are for the optical axis. In optical systems this is typically where distortion or aberrations are at their lowest. Therefore a comment on the entire optical system (i.e. variation in performance over the entire imaging area) is deemed as an important requirement for the instrument description. Could the authors please make comment to how much the ILS differs across the entire image area either with a statistic or a graph? The co-adding appears somewhat unintuitive. It appears that improvement in SNR has been presumed to follow the relation of square root of number of measurements, though this has not been validated. By co-adding pixels at the stage of level 1 (i.e. before the fitting procedure) the RMS improvement can be quantified without any presumptions on instrument performance. The RMS of the native resolution appears to be somewhat higher than comparable studies, and this could likely be improved if co-adding occurred on level 1 data. It is tradition to show spectral fits in DOAS papers, particularly where new instruments are presented. It is felt to be highly desirable to have a representative fit at the native resolution, and any binned resolutions, in addition to showing a fit from the GCAS instrument. This will enable the high native RMS to be considered in the light of representative NO₂ SCDs. There appears to be no cited literature on GCAS, therefore it is a “black box” instrument. It would be suggested that the vital information on this instrument is added in section 3.2.6. Aside from these more substantive points the article has a few minor revisions.

These are split by section for ease of the reader. General comments: Could the active tone be reworded to a passive tone. This is not consistent over the paper but does occur in several places (i.e. “we plan to minimise the uncertainty by...” to “it is planned to minimise the uncertainty by...”) “min” as in minutes to be expanded to “minutes”
Section 1: Page 13103 line 17: The relevance of mentioning the UV channel could be argued as none of the data is usable. Page 13103 line 18: What is the cause of the out-of-band stray light? Is the out-of-band stray light in the UV caused by reflection of the 1st order diffraction on the dichroic beamsplitter?

Section 2: Page 13104 line 6: 415 – 695 nm is all visible, please remove the near-infrared claim. Page 13104 line 14: What is the bandpass of these filters? If the extremes of the transmission window are close to the fitting window for NO₂ there could be transmission issues if the cutoff is not sharp. Page 13104 line 20: It would be useful to state the spectral sampling here. Page 13104 line 25: Information on the tolerances on these slits would be useful. Page 13105 line 1: Do PolZero polarisation scramblers introduce any measurable image distortion? A brief mention would suffice. Not everybody has access to SPIE therefore clarity would be appreciated. Page 13105 line 4: What is the temperature does the unit operate at? Has it been designed to be thermally stabilised or are there online temperature readings to validate this? Page 13105 line 14: The along track IFOV most importantly a function of the FOV angle. It is appreciated this is narrow, but it is not zero. Therefore could this angle be added? Page 13105: It would be useful to see a CCD frame or a typical spectrum. Not absolutely necessary but if added it could add value to this section. Page 13136 Table 1: “Single frame cross-track field-of view” is misleading. I presume you mean CCD pixel as opposed to the entire frame which encompasses the entire focal plane? Page 13136 Table 1: Has the field-of view been properly characterised? Is the cross-track IFOV consistent over the entire FOV? Page 13136 Table 1: Could SNR be added in to the table. Page 13136 Table 1: Has stray light been measured for both channels? This would be a useful figure Page 13136 Table 1: The final row of the table is not needed. This is not inherently a feature of the instrument.

C5169

Section 3: for the most part comprehensive, though there are a few typos amongst other minor comments. Page 13106: Is there an overview paper on Discover-AQ that could be cited here? Page 13107 line 16: Is an NO₂ cross-section at 255K representative of boundary layer NO₂ in continental USA? Page 13108 line 10: “With an uncertainty of $\pm 10\%$ ” Page 13109 section 3.2.8: There does not appear to be anything that needs flagging up, but it should be noted to the editor that the reviewer has very little experience of CTMs. [NOTE TO EDITOR]

Section 4: There are a few areas that require minor attention in section 4, primarily in the subsections relating to spectral sampling and cross-track striping. The subsection on air mass factors is comprehensive Page 13110: Could DOAS be explicitly stated in the first paragraph of section 4 along with a reference? Page 13111 line 9: Replace roughly with approximately Page 13111 lines 12-15: Is this sentence on the UV channel required? A very similar statement was brought up on page 13103 Page 13111 line 16: There must be tolerances stated here. Page 13112 line 7: It could add value to have a skewness statistic here. Page 13112 line 15: add (FWHM) after “Retrieved full-width at half maxima” Page 13112 line 25: As the SNR has been quoted for the zenith sky spectra, could the SNR of the nadir view be added in a relevant section (13136 Table 1 as previously mentioned). Could this explicitly be for native resolution? Page 13113 lines 6 – 10: This sentence is superfluous as the previous sentence refers to table 5 which contains all of the information regarding cross sections used. If this is reworded and not removed, please correct “NO₂ cross sections” to “NO₂ cross section” in line 6. Page 13113 line 14: Could you confirm the spectral sampling and justify this undersampling correction? Page 13114 lines 1-3: This RMS is quite high in comparison to other flight campaigns. Could this be improved through co-adding pixels before the fitting of absorbers? Could a typical fit on NO₂ be added in a figure? Page 13114 line 4: Is the difference in RMS across different slits due to throughput, alignment of each slit with regards to the telescope and the rest of the spectrometer, or the manufacturing precision of the slit across its length? Page 13117 line 24-26: This appears to have nearly the same content as page 13113 lines 20-23. Could one of these sentences

C5170

either be removed or reworded to avoid repetition? Page 13117 line 24-25: the fitting precision is quoted, but not what this figure precisely means. Is this the best case, the mean over all flights? Please make this clear. Page 13118 line 8: could the “users may improve the effective precision . . .” comment be removed as this appears superfluous. Page 13118 lines 7-10: Has this precision been proved? It only applied if the noise is Gaussian. With the consideration of stray light etc. the square root of n rule does not apply. If the precision has in fact improved could a spectral fit be shown in a figure to highlight this? Page 13118 lines 11-18: What was the ground temperature at the time of the measurements close to 297 K? If not, this too will introduce errors. Page 13118 line 20: “reduced” to “improved” Page 13118 line 21: The quoted precision of 1×10^{14} molec cm^{-2} is very good but there’s no RMS or spectral fits to verify this. Page 13118 section 4.6.2: It should be noted that extensive analysis was carried out in Lawrence et al. (2015) on AMF uncertainties from airborne platforms. A comment on this would add value to this section. Page 13119 lines 6-9: This would fit better in the concluding remarks of the article.

Section 5: This is a very comprehensive section. As a general comment from the reviewer, it is somewhat a shame that a greater focus of this paper was not dedicated to the inter-comparison of GeoTASO and GCAS. Two airborne imaging spectrometers being concurrently deployed for atmospheric chemistry has not been seen in literature to the reviewer’s knowledge. Page 13150 Figure 9: Could the red arrows be made white instead. This will aid comprehension from colour blind readers. Page 13121 lines 20-27: Could this sentence be broken up as it is very long. Page 13124 line 9: The % estimated error should be qualified, otherwise this comment should be removed. The data does not clearly show the overestimation – is it significant on the correlation? Page 13124 line 15-21: Could this sentence be broken up as it is very long. Page 13152 Figure 12: Could the locations of the “petrochemical manufacturing and refineries in Texas City and . . . Houston Ship Channel” be highlighted on the map for clarity.

Section 6: Page 13127 line 9: An r of 0.16 cannot be considered “fair”. Could a

C5171

comment on the inability to decipher statistical significance on cleaner days be stated (to be complete) or to just mention the ability for good correlations to be found on polluted days?

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 13099, 2015.

C5172