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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.

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We thank Reviewer 2 for helpful comments. In the text below, the reviewer's comments are in italics, followed by our response and edits.

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 = 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.*

We have addressed these general comments in our revised manuscript as follows:

1. Instrument performance across the entire imaging area

The original version of the paper contained this sentence in Section 4.1: "The FWHM changes by less than 0.015 nm with wavelength and cross-track pixel across the detector array." We have added more detail by instead using: "In the array's spectral dimension, the FWHM changes by less than 0.015 nm across the entire detector array, with the FWHM slightly higher at the center of the array than at the edges. In the cross-track spatial dimension, the visible FWHM varies by less than 0.01 nm, with the FWHM varying from narrowest at the left-most cross-track positions to widest at the right-most positions. The UV FWHM is narrowest at the center cross-track positions, and increases by 0.01 nm by the detector edges These calibrations do not show any significant change in the slit shape as described in Equation 1 in either the spectral or spatial dimension."

2. Co-adding

We performed these first NO2 retrievals at the instrument's native spatial resolution in order to examine results on individual cross-track positions for effects such as striping and calibration, and also to keep a dataset that was flexible enough to provide to campaign team members who wanted either high spatial resolution observations (higher noise) or low noise (and low resolution), and specific spatial resolutions. This approach is being revisited for our next NO2 reprocessing, and would not be applied in the UV where stray light is an issue. At least at 250 m x 250 m resolution, the results are very similar from co-adding columns or spectra. We have also added this text to Section 4.3 on spectral fitting: "In the visible spectrometer, we find fitting uncertainties

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derived from slant columns co-added post-retrieval at this resolution are typically within a few percent of those derived using spectra co-added pre-retrieval with a common mode fit in the retrieval (e.g. Martin et al. (2002); Chan Miller et al. (2014); González Abad et al. (2015a)), indicating that influences from systematic issues are low in the 250 m x 250 m visible results." We have added a caveat to a later statement about 1 km x 1 km results: "if errors were Gaussian". RMS improvements with co-adding are discussed below.

3. Lack of spectral fitting figure

We have now included three sample spectral fits in a new figure.

4. Absence of fuller description of the GCAS instrument

There is a reference to the GCAS instrument paper (Kowaleski and Janz, 2014) in Section 3.2.6, which contains detailed information on the instrument. We have added the following text to Section 3.2.6 to include more detail within the GeoTASO paper: "The instrument uses an Offner imaging spectrometer with two 1072x1024 (spectral x spatial) pixel detector arrays in the UV/visible (300–490nm) and visible/near-infrared (480–900nm). The UV/visible channel used for NO2 retrievals has a spectral sampling of 0.2 nm and a spectral resolution of 0.57 nm. The full cross-track FOV covers 45o. At wavelength 440nm and at a spatial resolution of 250 m x 500 m, signal-to-noise is on the order of 540." As this paper is already long and has many figures, we would prefer to leave a figure of GCAS spectral fitting for a future planned paper on GCAS. This will also help keep the paper focused on GeoTASO.

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".

Although there has been much debate about the use of the active versus passive voice in scientific publications, these days most journals who require a specific voice request the use of the active voice (Nature Publishing Group for example). As far as

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we are aware, Copernicus journals do not express a preference. In keeping with recent convention, we have maintained the active voice in our article, but did find several inconsistent uses of the passive voice, and have modified those sentences. In other cases, the passive voice still seems to be more appropriate in order to bring more attention to the object of the sentence, particularly in the methods section. See for example: http://www.writing.utoronto.ca/advice/specific-types-of-writing/active-voice-in-science and http://www.nature.com/authors/author_resources/how_write.html. Our original submission used “minutes” but all cases were changed to “min” in the typesetting process, so presumably this is the AMT standard abbreviation.

Section 1: Page 13103 line 17: The relevance of mentioning the UV channel could be argued as none of the data is usable.
We have removed the mention of the UV channel.

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?
Grating orders 0, 1, 2 and 3 were modelled and stray light managed for spectra on each channel, so that no light intended for one channel would couple to the other. The stray light is most likely from far out-of-band light (>700 nm) that is not intended for either detector. An ad hoc baffle was added after the Houston 2013 campaign. A coating was added to a telescope mirror after the DISCOVER-AQ Colorado 2014 campaign to reduce the UV stray light. We have not discussed this further in the text after removing the mention of the UV channel as suggested in the previous comment.

Section 2: Page 13104 line 6: 415 – 695 nm is all visible, please remove the near-infrared claim.
The mention of near-infrared has been removed.

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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.
We have added the following sentence: "Additional UV and VIS blocking filters provide high in-band throughput with a ∼25 nm wavelength transition region to high level blocking (∼2% transmission) for out-of-band light within the sensor's wavelength range."

Page 13104 line 20: It would be useful to state the spectral sampling here.
We have added the sentence: "The spectral samping is 0.14 nm for the UV and 0.28 nm for the visible detector."

Page 13104 line 25: Information on the tolerances on these slits would be useful.
We have added the following to this section: "Under microscope testing, slit edges were free of pronounced jaggedness at high spectral frequencies, and were uniform in width to at least ±2 µm, the limit of the microscope setup."

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.
They are flat parallel windows tilted at a small angle. There is a small lateral image shift due to the tilt, and a focus shift due to the optical path length, but no distortion. Spots are centroid to the same locations whether the depolarizers are energized or not. We have added the following to the text: "These are flat parallel windows tilted at a small oblique angle. There is a small lateral image shift due to the tilt, and a focus shift due to the optical path length, but no distortion."

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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?

The UV and Visible focal planes are cooled to -25 C using a thermoelectric cooler (TEC). A water loop to an external chiller removes the rejected heat. Temperatures are continuously monitored on both the hot and cold sides of the TEC. The chilled side is very stable (T $<$0.2 K). The larger insulated enclosure around the optical bench is separately controlled to 24 C and temperatures are continuously monitored using 10 additional thermistors. Temperatures vary by $\sim$1-2 degrees. The bench is low-CTE graphite and the flexure-attachment scheme is designed to athermalize optical positions over a wide range of temperatures. We have modified this paragraph as follows: "The spectrometer and telescope are contained within a thermally-stabilized housing with continuous temperature monitoring. Each focal plane is chilled to -25C, with an observed stability of $\pm$0.2C. The insulated enclosure around the optical bench is separately controlled to 24C with a stability of 1–2C. The bench is graphite with a low coefficient of thermal expansion, and the flexure-attachment scheme is designed to athermalize optical positions over a wide range of temperatures. The instrument housing is mounted to the seat rails and the telescope looks at the nadir direction through a fused silica window on the bottom of the aircraft."

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?

We have added the following sentence: "The 1.2 mrad along-track IFOV (39 µm slit) projects to an IFOV of 13–15 m at the ground." The is no pronounced distortion at the the edges of the full FOV.

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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.
As the paper is already very long, we have not included this additional figure, but have now included typical fits of the spectra in a later figure.

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?
This has been changed to "single pixel".

Page 13136 Table 1: Has the field-of view been properly characterised? Is the cross-track IFOV consistent over the entire FOV?
Yes, the IFOV is quite consistent from edges to centre and is constant for radiometric purposes. (The optical design of the telescope targeted a linear mapping of object-space angles to focal-plane displacement.) There is a very small change in focus along the axis of the slit, which is shown in the GeoTASO instrument paper (Leitch et al., 2014). For purposes of geolocation, an absolute check of platescale was done at the end of integration with moderate fidelity. Final tweaks to geolocation were done after flight based on projection to visible surface imagery. Any remaining uncertainties likely come from the digital elevation model.

Page 13136 Table 1: Could SNR be added in to the table.
We have added the following line to the table:
Single pixel signal-to-noise ratio | 110 (340 nm) | 65 (440 nm)

Page 13136 Table 1: Has stray light been measured for both channels? This would be a useful figure

We have added the following sentence to the section on calibration: “The mean out-of-band signal in the visible channel was less than 1% of the mean in-band illumination, as measured using an incandescent source and a 445 nm longpass filter.”

Page 13136 Table 1: The final row of the table is not needed. This is not inherently a feature of the instrument.
This row has been removed.

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?
There is an overview paper planned, but not yet written. We have added a reference to the program website here.

Page 13107 line 16: Is an NO2 cross-section at 255K representative of boundary layer NO2 in continental USA?
The temperature of the cross sections used in the retrieval is an effective temperature determined from NO_2 and temperature profiles as described in Herman et al. (2009). This considers the stratospheric and tropospheric temperatures for accurate derivation of total column NO_2. While answering this question, we discovered the temperature listed in the paper was incorrect for the Texas retrievals (it should be 250 K, not 255 K which was used for Maryland campaign (Lamsal et al., 2014)). The text has been expanded to reference the effective temperature: “Total column NO_2 is derived using NO_2 cross sections from Vandaele et al. (1998) at an effective temperature of 250 K derived from NO_2 and temperature profiles as described in Herman et al. (2009).” The error in total column from temperature uncertainties is expected to be small (3.3% for every 10 K difference) (Lamsal et al., 2014).

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Page 13108 line 10: "With an uncertainty of ±10%"
This has been corrected in the text.

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?
This is not strictly a DOAS fit; scaling and background polynomials are retrieved, but the radiance is fit directly in the retrieval. We have modified the second sentence in the section to "First, we retrieve the slant column amount of the gas in the viewing path from each nadir spectrum by direct fitting (Chance, 1998) with a zenith sky spectrum as a reference." We have also added a reference to Chan Miller et al. (2014) in the next paragraph, which probably provides the most detailed published description of this approach.

Page 13111 line 9: Replace roughly with approximately
This has been changed in the text.

Page 13111 lines 12-15: Is this sentence on the UV channel required? A very similar statement was brought up on page 13103
We have removed this sentence.

Page 13111 line 16: There must be tolerances stated here.

Table 1. Retrieved slit parameters for slit function described by Equation 1 and resulting full-width at half-maxima (FWHM).

	Slit Size			
	26.0 μm	32.5 μm	39.0 μm	45.5 μm
UV				
h	0.217	0.239	0.263	0.292
a	-0.048	-0.013	-0.013	-0.0045
h'	0	0	0	0
a'	0	0	0	0
w	0	0	0	0
FWHM	0.34 nm	0.39 nm	0.43 nm	0.49 nm
VIS				
h	0.440	0.475	0.542	0.627
a	-0.048	-0.035	-0.034	-0.032
h'	0	0	0.470	0.627
a'	0	0	0.074	0.073
w	0	0	0.133	0.283
FWHM	0.70 nm	0.75 nm	0.88 nm	1.00 nm

We have added information on the slit size tolerance in Section 2.1, and discussed the variability in the slit width as described above.

Page 13112 line 7: It could add value to have a skewness statistic here.
We have added a table that includes all retrieved slit parameters including those describing slit function asymmetry.

Page 13112 line 15: add (FWHM) after "Retrieved full-width at half maxima"
FWHM has been added.

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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?
We have added the SNR to Table 1.

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 "NO_2 cross sections" to "NO2 cross section" in line 6.
We have removed the detailed sentence and now only briefly list the molecules fit in the next paragraph for completion.

Page 13113 line 14: Could you confirm the spectral sampling and justify this under-sampling correction?
We have added the following text: "which is necessary for the 26 and 32.5 µm slit sizes where sampling is 2.5 and 2.7 pixels/FWHM, respectively."

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?
These RMS results are at native resolution. The RMS improves with co-adding. We have added the following to the text: "For retrievals using co-added spectra, the mean RMS is $\sim 1.6 \times 10^{-3}$ at 250 m $\times$ 250 m resolution. GeoTASO design requirements were driven by NO_2 fitting at a resolution of 1 km $\times$ 1 km. At this resolution, the mean RMS is $\sim 7 \times 10^{-4}$, which is comparable to previous fitting results in this spectral region (Martin et al., 2002; Chan Miller et al., 2014), and includes some remaining

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systematic residuals as well as other residuals which are not necessarily systematic from co-adding inhomogeneous scenes and different wavelength positions and slit functions." We have now included a figure which shows a typical fit.

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?
We have added the following: "The exact source of these RMS differences still needs investigation, but is likely due to differences in throughput and in part to an imperfect undersampling correction."

Page 13117 line 24-26: This appears to have nearly the same content as page 13113 lines 20-23. Could one of these sentences either be removed or reworded to avoid repetition?
We have rephrased the second sentence to avoid repeating the same information from the previous section.

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.
We have now specified that this is the mean for all flights.

Page 13118 line 8: could the "users may improve the effective precision" comment be removed as this appears superfluous.
We have removed this phrase and moved the beginning of the sentence to the section on spectral fitting so it precedes the discussion of RMS.

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?
We have included text related to this in Section 4.3 and a figure of a spectral fit, as described above.

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.
We have added the following text: "During GeoTASO flight times, surface temperatures were on the order of 2-10 K warmer, which could introduce a small error at the very warmest temperatures."

Page 13118 line 20: "reduced" to "improved"
This has been changed in the text.

Page 13118 line 21: The quoted precision of 1x1014 molec cm-2 is very good but there's no RMS or spectral fits to verify this.
While checking the RMS on this fit, we discovered a transcription error; the fitting precision is actually 7e14 for the offset in these cases. We have changed the phrase to: "however, its effective precision is improved to 7×10^{14} molecules cm^{-2} (RMS=5×10^{-4}) due to averaging of many spectra in the along-track direction."

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.
We have added the following text: "Lawrence et al. (2015) estimated an AMF uncertainty of ~8% in remotely sensed NO_2 from an airborne mission over Leicester

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City in the U.K., primarily resulting from uncertainties in the surface albedo and NO_2 profile shape."

Page 13119 lines 6-9: This would fit better in the concluding remarks of the article.
We have not discussed specific future improvements to the algorithm in the conclusion so there is no obvious home for this statement if moved. As this is actually a fairly minor source of uncertainty (∼3%), we have removed the mention of hypothetical BRDF improvements using wavelength dependencies.

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.
We have now included a table (see response to Reviewer 1) which lists comparison statistics with GCAS for all days during the campaign. We are planning a more substantial paper on GCAS retrievals and GeoTASO comparisons. This future article will also include data from the Colorado campaign, which involved several coincident flights including raster scans of the entire Denver metropolitan area. Coincident data from both instruments is also being included in other DISCOVER-AQ papers in preparation.

Page 13150 Figure 9: Could the red arrows be made white instead. This will aid comprehension from colour blind readers.
The colour of these arrows has been changed to white.

Page 13121 lines 20-27: Could this sentence be broken up as it is very long.

We have split this sentence into three sentences.

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?
There are a few very high values of FRM measurements relative to GeoTASO, but it is true that it is difficult to ascribe the over-estimate to a specific source and not significant on correlation. We have removed this sentence.

Page 13124 line 15-21: Could this sentence be broken up as it is very long.
We have changed the semicolon to a period and split the sentence.

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.
These locations have been added to the maps. We have also added the location of the I-10/Sam Houston Parkway junction discussed in the text for completeness.

Section 6: Page 13127 line 9: An r of 0.16 cannot be considered "fair". Could a 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?
The added table now shows more complete results for comparisons on other days, including p-values for statistical significance, and overall statistics. We have rewritten the text in the conclusion that discusses comparisons with Pandora, and removed the mention of the r=0.16 comparison in the conclusion (the only day when comparisons were not statistically significant).

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Additional References:

Chan Miller, C., González Abad, G., Wang, H., Liu, X., Kurosu, T., Jacob, D. J., and Chance, K.: Glyoxal retrieval from the Ozone Monitoring Instrument, Atmos. Meas. Tech., 7, 3891–3907, doi:10.5194/amt-7-3891-2014, 2014.

Lamsal, L. N., Krotkov, N. A., Celarier, E. A., Swartz, W. H., Pickering, K. E., Bucsela, E. J., Gleason, J. F., Martin, R. V., Philip, S., Irie, H., Cede, A., Herman, J., Weinheimer, A., Szykman, J. J., and Knepp, T. N.: Evaluation of OMI operational standard NO2 column retrievals using in situ and surface-based NO2 observations, Atmos. Chem. Phys., 14, 11587-11609, doi:10.5194/acp-14-11587-2014, 2014.

Figure 1: Sample NO_2 spectral fits showing observed (blue) and simulated (red) optical depths from spectra collected over South Houston (29.70 N, 95.24 W) on 13 September 2013 at 10:46 LT, SZA = 44.1 for a) native resolution (no co-adding); b) 250 m $\times$ 250 m resolution (27$\times$4 co-adds); and c) 1 km $\times$ 1 km resolution (108$\times$16 co-adds).

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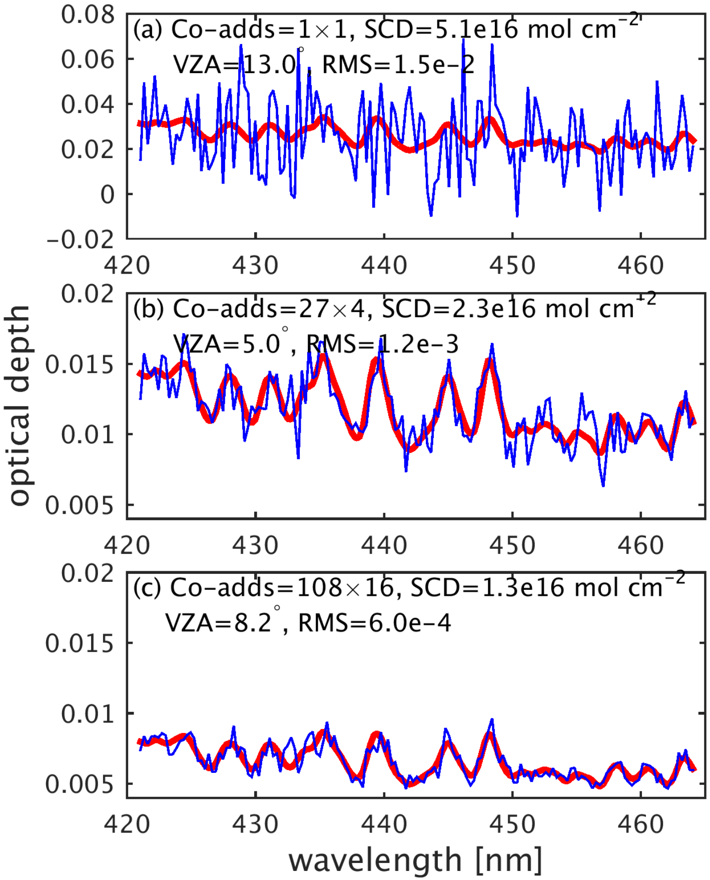

Fig. 1.

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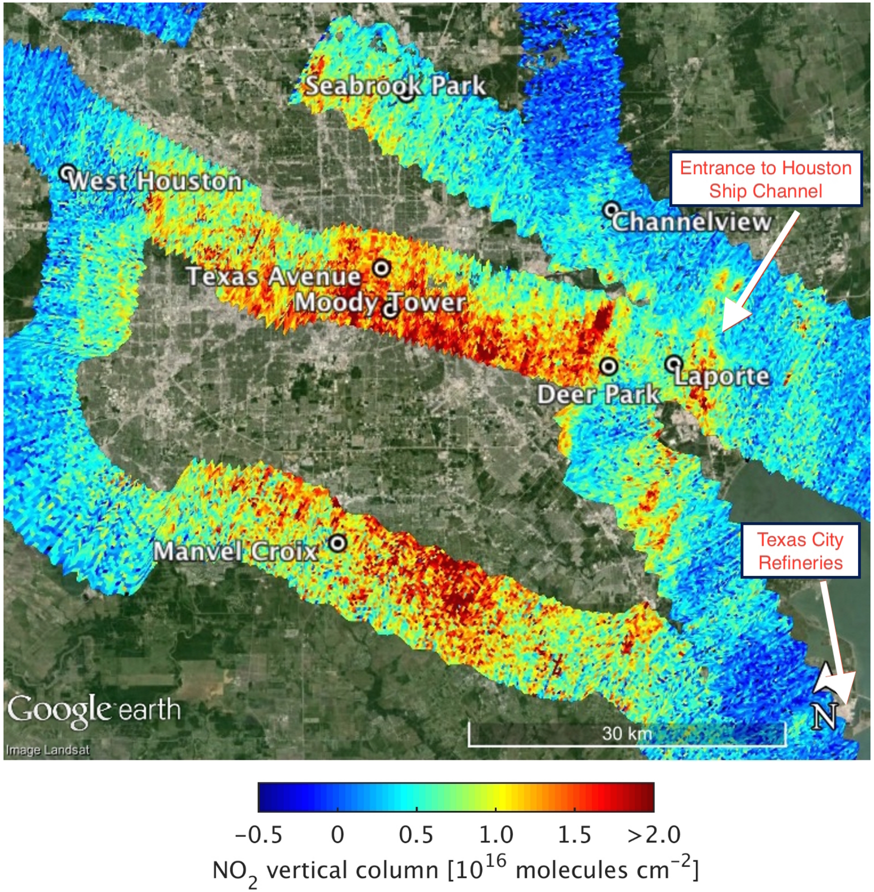

Fig. 2.

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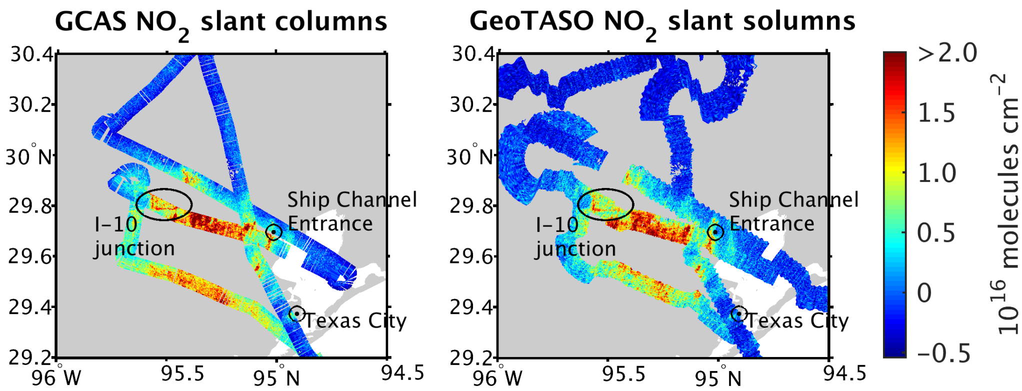

Fig. 3.

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