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Interactive comment

Interactive comment on "TanSat ACGS on-orbit spectral calibration by use of individual solar lines and entire atmospheric spectra" *by* Yanmeng Bi et al.

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Discussion paper

Reply to interactive comments

September 11, 2020

1 Reply to interactive comments

Thanks for the comments, however, most of these comments are incorrect, and show the reviewer's lack of knowledge of on-orbit wavelength calibration. These comments are very likely to mislead the readers into believing that wavelength calibration on orbit is an easy thing.

1. There are a number of problems with this paper. Space-borne spectrometers require calibration in terms of radiometric, geometric (geolocation), spectral, and polarization behavior. In terms of spectral calibration, both wavelength (dispersion) calibration as well as calibrating the instrument line shape (ILS) functions. Of all these categories of calibration, fitting the dispersion is probably the easiest and best understood. It is so straightforward and robust, in fact, that few groups even bother to do this in a dedicated fashion, because it is easily fit for simultaneously with other atmospheric parameters required to derive e.g. column mean carbon dioxide concentration (XCO2) or solar-induced chlorophyll fluorescence (SIF). This is well documented in many publications (e.g., Reuter et al. 2010, Taylor et al 2011, Frankenberg 2011, O'Dell et al 2012, Crisp et al. 2017, Wu et al. 2018).

Reply: What are a number of problems? We have not found them in this paragraph.

How do you know few groups even bother to do this? Groups are what kinds of groups? calibration group, retrieval group or application group? If you search spectral calibration or wavelength calibration, you will find many papers focusing on this topic.

Fitting the dispersion between two spectra is not so easy, so straightforward and robust. On the contrary, it is very complex because of the fitting algorithm, the possible multi-solutions depending on the degree of convergence. Especially, when other atmospheric parameters are derived with the dispersion based on optimal estimation, for instance, the XCO2 or SIF, fitting between the observation and the simulated atmospheric spectra will become more complex because the dispersion and other parameters are competitive to contribute to the goodness of fit. If the fitting failed, no reasonable results are outputted. The failure often occurs in the retrieval. If the offset is off by more than about one spectral sample, the fit will often fail to converge. So, the fit is not always robust, easy and straightforward.

All these papers given by the reviewer are not for spectral calibration, but for data retrieval. They just mention the correction of the residual spectra shifts in the process of XCO2 retrieval. For example, Reuter et al. (2010) mentions the wavelength shift in one sentence in section 3.2.1. Taylor et al (2011) writes "...a wavelength multiplier (f). The wavelength multiplier is necessary as the wavelength calibration given in the TANSO-FTS L1B files does not account for either short-term drifts in the scan laser frequency ...", They use a multiplier to correct the wavelength bias rather than dispersions because GOSAT uses an interferometer, not a grating spectrometer. The reviewer confuses the basic calibration terminology between different instruments, which may mislead readers and editors. Because these papers focus on retrieval, rather than spectral calibration, we do not think the spectral calibration is well documented in the given papers. Therefore, these papers are not listed in our manuscript.

The author and co-authors are the members of the TanSat calibration team and our responsibility is providing well calibrated spectral radiance data for the users who use the spectra data for XCO2 retrieval. So, we have to calibrate the satellite observations. We believe that the user communities are interested in this spectral calibration which is also considered in their retrieval.

2. Therefore, the methods espoused are nothing new. It is confusing to me why they focus on individual solar lines, when they could just as easily fit for example a 2 or 3-parameter update to the wavelength dispersion by using all the solar lines (or all solar lines greater than a certain depth); this would be significantly more robust than using a single solar line. And also why they choose the Kurucz spectrum as their reference, where nearly all groups have found that the Toon solar reference spectrum (http://mark4sun.jpl.nasa.gov/toon/solar/solar_spectrum.html) is considered superior in terms of accuracy.

Reply: No paper shows the same method as that shown in section 2 in our manuscript. So, the methods using individual Fraunhofer lines is new. The method described in section 3 is developed by Geffen and van Oss (2003). In principle, direct computation of spectral shift based purely on individual Fraunhofer lines position is more robust than matching two entire spectra, as explained above. Second, according to our experience, the computation of using individual solar lines is faster than that of fitting two entire spectra because the iterations are avoided.

Different solar spectra have different accuracies. The accuracy of intensity has little effect on the spectral calibration because what is needed is the position of Frauhofer lines. This is different to XCO2 retrieval where the accuracy of intensity has large effects on the XCO2. The absolute intensity can be removed by normalization or scaling the spectra in the wavelength calibration, as noted in section 2.D by van Geffen JH (2003). It is not important which solar spectra is used in this paper. More important thing is how to perform the spectral calibration on orbit to assess the wavelength variation. Therefore, Kurucz spectrum can be used as reference. This spectrum can well satisfy TanSat calibration requirements. Also, this spectrum is widely used in science community. Only this spectrum is available when we began to study this method in the year 2014.

3. But these are minor problems. The biggest problem is that much of this work is already documented in a prior publication by many of these same authors, in the recent paper “Inflight Performance of the TanSat Atmospheric Carbon Dioxide Grating Spectrometer” (Yang et al., 2020). That publication also fits for spectral shifts using the solar spectrum, and produces time series of the results, just as in this paper. Remarkably, the Bi et al. paper under review here does not even reference, discuss, or compare to the results from Yang et al. in terms of the wavelength shifts found in both works. The single reference to the Yang et al. paper in this work is to state the size of the spatial field-of-view of TanSat; nothing about its results on spectral shifts relative to preflight.

Reply: This comment confuses the two types of problems. It is well known to all that the biggest problem should be big errors in principle, big defects in method, or big errors in conclusion. Reference documents are not part of the big problems.

"The single reference..." is not right. We do reference professor Yang’s two papers. Please see the last two papers in the references in the manuscript. When this manuscript was prepared, the latest Yang’s paper was just accepted, and had not been published. So we did not know how to cite an unpublished paper.

If you compare professor Yang’s paper and this manuscript, you will find that much of this work is not documented in the professor Yang’s paper which describes the infight performance of the TanSat ACGS. He summaries the inflight spectroscopic performance in section III.A, as well as ILS, radiometry, dark current, SNR, gain coefficient and so on. This manuscript describes the spectral calibration method using individual solar line in many details and compares this with the fitting method using entire atmospheric spectra. Therefore, much of this work is not documented in Yang’s paper.

The reviewer do not really read Yang’s paper. This comment, "That publication also fits for spectral shifts using the solar spectrum ...,", is not right. Please note that professor

Yang also uses the available Fraunhofer lines method to get the wavelength shift, but not fitting the solar spectra to derive the spectral shift. He describes this method in the second paragraph in section III.A. Now, Dr. Yang's paper has been published, we cite this paper in section 1.

4. Another serious problem with this paper is that although they say that the solar and atmospheric methods give similar results, they never actually demonstrate this. There is no figure that compares them, no method to actually discuss and compare quantitatively their respective results. I tried to do this manually, and they actually did not agree (comparing the offsets given in their table 2 results, to those in figures 4-6 for the solar method).

Reply: A comparison over Beijing in 2017 is performed for the two methods (see the new figure 1 in this document). We select these orbits according to certain conditions. Only the cloud-free and clean scenes are selected because of the little aerosol effects. Dark ocean scenes are also excluded. A total of 7 orbits are selected. We calculate the shifts for these orbits using the second method. Then the shifts in one orbit are averaged to obtain a shift. These averaged shifts are compared with those derived from the first method. For all field of view, the average differences in O2A, WCO2 and SCO2 bands are -0.0017 nm, 0.0016 nm and 0.0020 nm, respectively. We add the figure of the comparison. The intra-orbit shifts from the second method on 5 April 2017, is also provided as another case that is similar to that case on 23 April 2017 in Figure 10.

We think the above comparison shows that results from the two methods agree very well. And, table 2 only shows an example that the 2nd method can get the reasonable shift.

5. A minor point is that while they discuss the solar doppler shift (induced by the relative motion of the spacecraft and the sun, when they use the solar method), they never discuss the doppler shift of the telluric (earth's atmosphere) lines, induced by the

relative motion of the satellite and the target point on the earth's surface. It's not clear if they take this into account.

Reply: Yes, we correct them. The 4th co-author Dr. Liu is responsible for the geometric calibration of TanSat. The new figure 2 in this document shows the doppler shift between the instrument and the sun in solar mode in O2A band in 2017. The new figure 3 in this document shows the doppler shift variations between the instrument and the surface targets in nadir mode in O2A band in 2017. We do not show these figures in the manuscript because the doppler shift calculation is of high school students. The satellite velocity calculated by Dr. Liu is stored in the variable FrameGeometry/satellite_velocity in the L1B file that are available in the web site (http://satellite.nsmc.org.cn/portalsite/default.aspx?currentculture=en-US).

Finally, there are many English grammar errors that require correcting. I do not bother to list them, as this is a relatively minor point considering the structural deficiencies in the paper.

Reply: Several reviewers have listed them, and I have revised these English grammar errors. I also do not bother to list your syntax errors in your comments.

Therefore, I recommend to reject this work for publication. It introduces nothing new to the field, its results are not coherent, it does not try to explain the physics of what is going on (e.g. the noticeable jump in spectral shift around DOY 150 in Figure 4). It does not explain the disparity of results across the 9 FOVs, in particular in the strong CO2 band. So it is not new, it does not explain anything to us regarding what is happening with TanSat in particular, the results are confusing and not well-presented, and the effect is easily and automatically taken into account by the TanSat XCO2 and SIF retrievals anyway (e.g., Liu et al., 2013, Du et al., 2018).

Reply: The reviewer obviously do not have the relevant expert knowledge about the instrument spectral calibration and its status assessments. We have developed the individual solar method to obtain the only confirmed solvable directly. There is no doubt that this method is new. Also, this method successfully detects the jump in spectral shift around DOY 150. We explain this jump caused by the switching of strategy of solar calibration in the 2nd paragraph in section 2.2. If you do not understand that this switching can break the raw heat balance to cause spectral shift, please you study some basic knowledge about instrument calibration.

The quite little differences among the 9 FOVs are normal. The key point is that they have coherent variation. Also, the calibration accuracies for each of the FOVs meet our requirements.

HOW VERY INTERESTING it is to say that the effect is easily and automatically taken into account!

It looks that the two papers given by the reviewer support these comments, but in fact it's not. The two papers are about products retrieval and do not demonstrate the effect is easily and automatically taken into account. In the paper by Liu et al. (2013), he only says that shifts are estimated from the spectra, but we do not know how to estimate, and this retrieval is tested using data from GOSAT which uses an interferometer, rather than grating spectrometers used by TanSat. The spectral calibration for the two types of instruments are very different. The reviewer uses the word of 'automatically'. We do not understand what this word means in retrieval? Does it mean that people should not carefully consider this shift?

In the paper by Du et al. (2018), Professor Yanmeng Bi is the fourth author. He contributes the method for radiance calibration and wavelength calibration to this paper. So, he is the co-author in Du's paper. In this work, the spectra with unreasonable wavelength shift due to the instrument performance or calibration error, were simply removed during the retrieval process. NO SHIFT WAS ESTIMATED!

2 Summary

Most of these comments are not conducive to improve the calibration methods, and are not conducive to enhancing the conclusions of our manuscript. We can feel that the reviewer may be familiar with the retrieval of CO2, but has very little knowledge about instrument and spectral calibration. He confuses the basic terminology between interferometer and grating calibration, and does not really understand the goals and methods for assessment of instrument status on orbit. He thinks the fit of two entire spectra is the easiest method. Actually, he does not understand the nature of fitting two spectra to get multi-variables that are competitive in the process, and can lead to improper results. Particularly, these results are not unique based on the goodness of fit. Finally, fitting often fails if one of variables becomes abnormal in the iterations. He does not understand why new method is needed for spectral calibration on orbit.

Therefore, all these comments have no real values. Most of them are very wrong, very malicious and are very likely to mislead the readers into believing that wavelength calibration on orbit is an easy thing.

But we still work hard to reply these comments point by point, and hope this reviewer can understand the nature of spectral calibration. This hope becomes one of the aims of this manuscript. Other aim is to provide useful information to the users of TanSat products.

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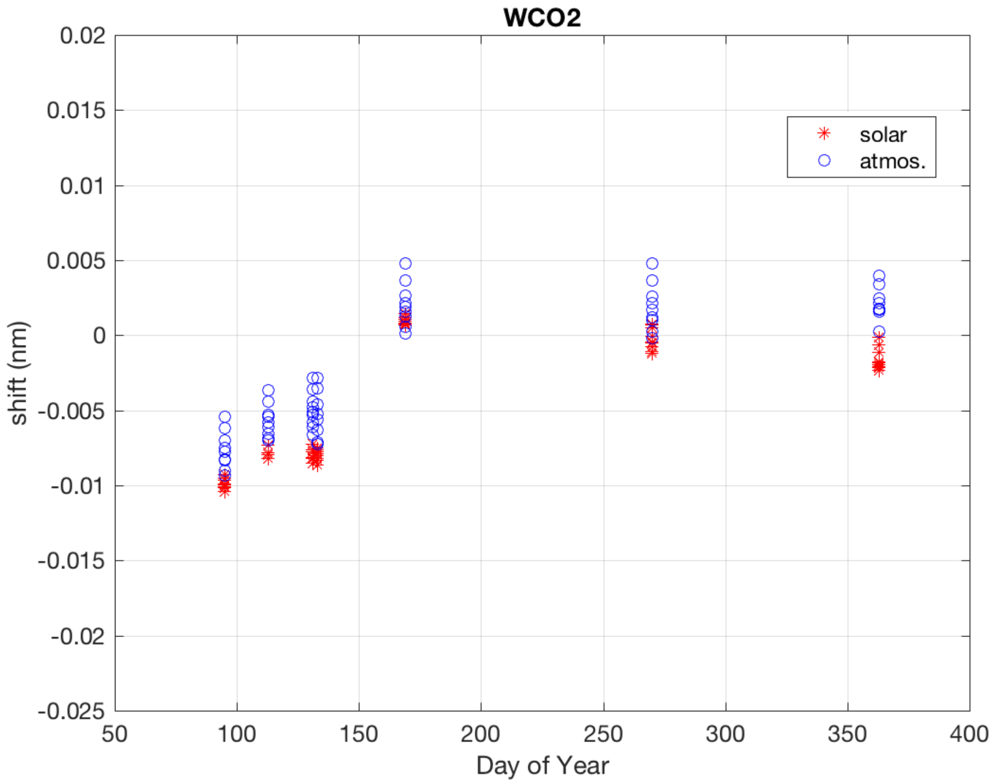

Fig. 1. The comparison of wavelength shifts form the two methods for nine FOVs in WCO2 band for the orbits over Beijing, China, in 2017.

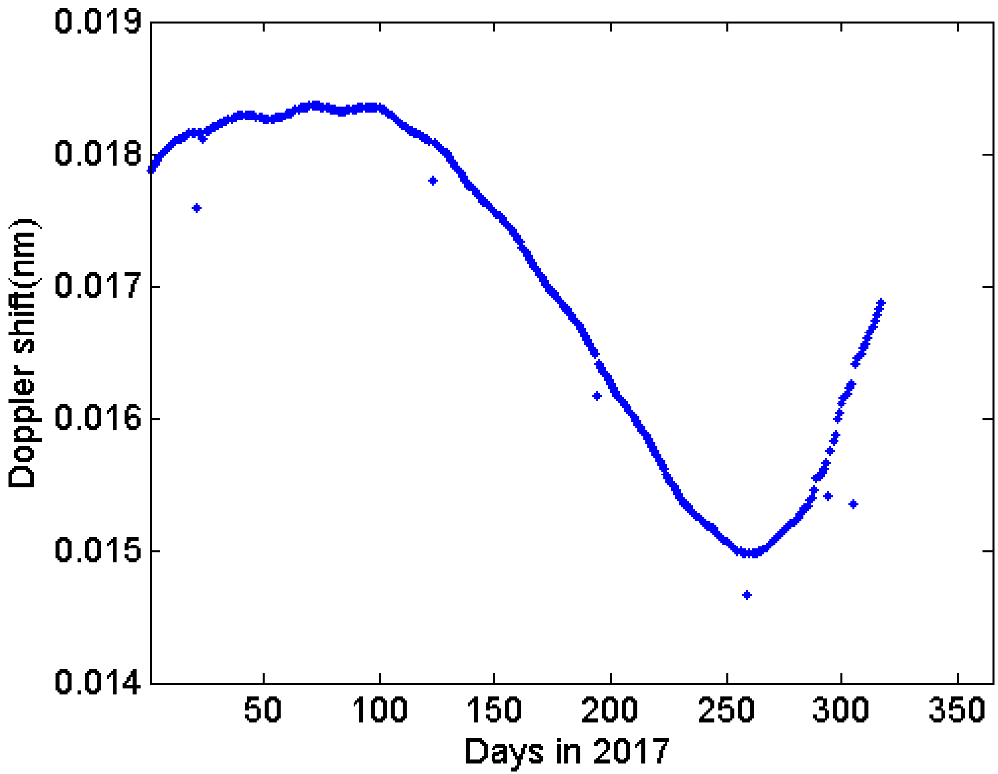

Fig. 2. The doppler shift between the instrument and the sun in solar mode in O2A band in 2017.

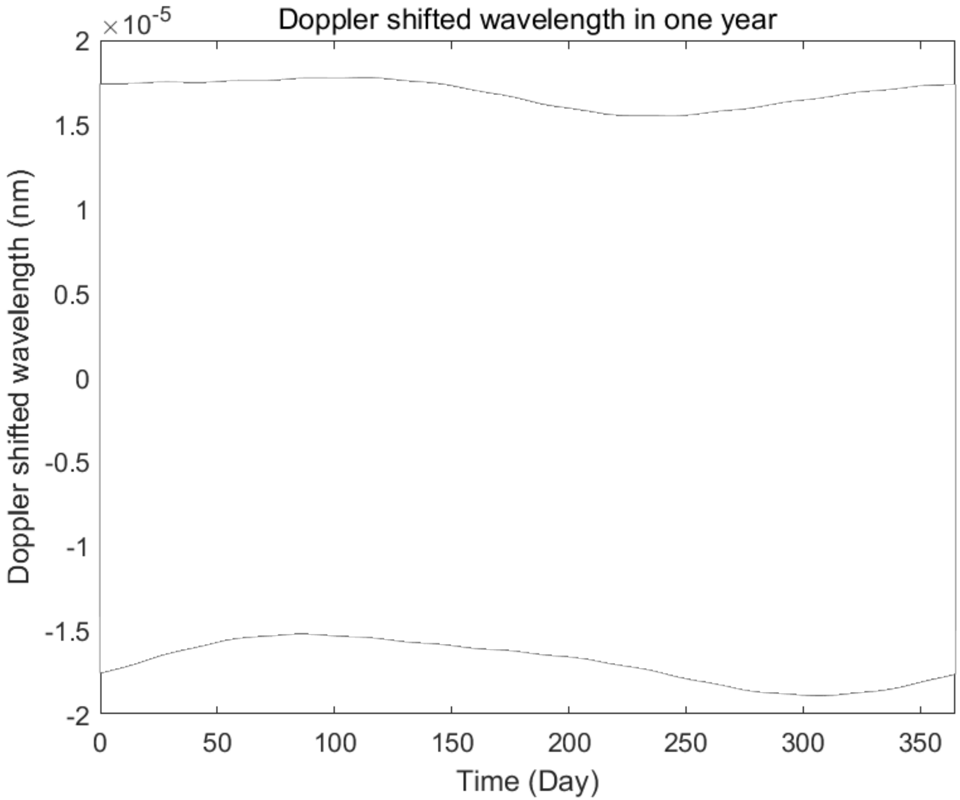

Fig. 3. The doppler shift variations between the instrument and the surface targets in nadir mode in O2A band in 2017.