Point-by-point reply to review by Dr. Chris Sioris

By Xi Xi on July 6, 2015

We greatly appreciate Dr. Sioris' timely review of the manuscript. The detailed comments in this review will definitely help to improve this manuscript. Here we would like to provide a point-by-point reply to Dr. Sioris' comments and clarify some misunderstanding and/or ambiguities. Each original comment is presented in italics, followed by the reply to each comment. **The changes to the manuscript have been implemented in the revised manuscript. The page number and line number refer to those in the revised manuscript, which has been uploaded to the AMT Discussion Section.**

Comment: *This manuscript contains some novelty but is generally of poor quality. However, the idea of remote sensing of CO_2 and CO in particular from a geostationary orbit is a great idea and missing in terms of planned atmospheric chemistry missions. With the scientific importance of such a mission being so high and highly capable Dr. Sander and Prof. Yung as co-authors, I believe that it is not the right decision to reject this manuscript. However, in my opinion, the manuscript covers up some important details and attempts to present an overly optimistic picture. This is acknowledged by the authors in the last paragraph, but to acknowledge this is not enough. The single-measurement precision will be larger than what the authors report if the aerosol properties are not perfectly known, yet the authors conclude that they have provided evidence that all-sky retrievals meet the measurement requirements for accurate flux inversions. There is a good deal of cleaning up of obvious and/or fundamental errors that needs to be done before this manuscript is acceptable.*

Reply: We acknowledge that some details might be omitted in this idealized simulation study. However, there is definitely no intentional attempt to "cover up some important details" or attempt to misguide the reader through an optimistic picture. As discussed in the Conclusion section, a simulation study such as this one could hardly reach the realism in real retrievals. This is an intrinsic and unavoidable limitation in many simulation studies. It is agreed that the single-measurement precision will be larger than what is reported in this manuscript if the aerosol properties are not known well. We apologize if the original statement is not clear. What is meant is that the single-measurement precision in the all-sky simulations are far better than the requirements for GeoFTS, thus leaving a large margin for imprecision caused unknown aerosol properties. In this way, the simulated retrievals provide "evidence" that there is a high probability that, in real retrievals, the single-measurement precision will meet the requirement for accurate flux inversions. **The statement in P28L20 has been revised for clarification.** We will not be able to know what the real single measurement precision would be until we do real retrievals with spectra measured by the instrument. Therefore, at this stage, what we could do was to acknowledge the limitation of this study.

There are indeed some improvements that need to be done for this manuscript and this review has greatly facilitated this process. We will clarify some issues which were perceived as "obvious and/or fundamental errors".

Comment: *There is no precision requirement on XH_2O but precise XH_2O is required for objective 3. There is no accuracy requirement for any gases, but maybe there should be. The retrieval bias for xCH_4 for all-sky conditions is larger than the precision requirement. Is that a concern?*

Reply: Up to now, there are no systematic studies that quantify the precision requirement on XH_2O, so there is not benchmark to adhere to. Because of this, it is hard to judge if the retrieval precision for XH_2O is precise enough or not. The statement about "precise XH_2O" is indeed ambiguous and **we have removed the word "precise" on P5L3 in the revised manuscript**. As there are no studies that quantify accuracy requirements, we did not focus on retrieval accuracy.

It is true that the retrieval bias for XCH_4 for all-sky conditions is larger than the precision requirement. However, in real retrievals, the retrieval biases will be corrected through validation with ground-based measurements and the implementation of correction factors. Therefore, the retrieval biases are not comparable to the precision requirement and this does not need to be a concern.

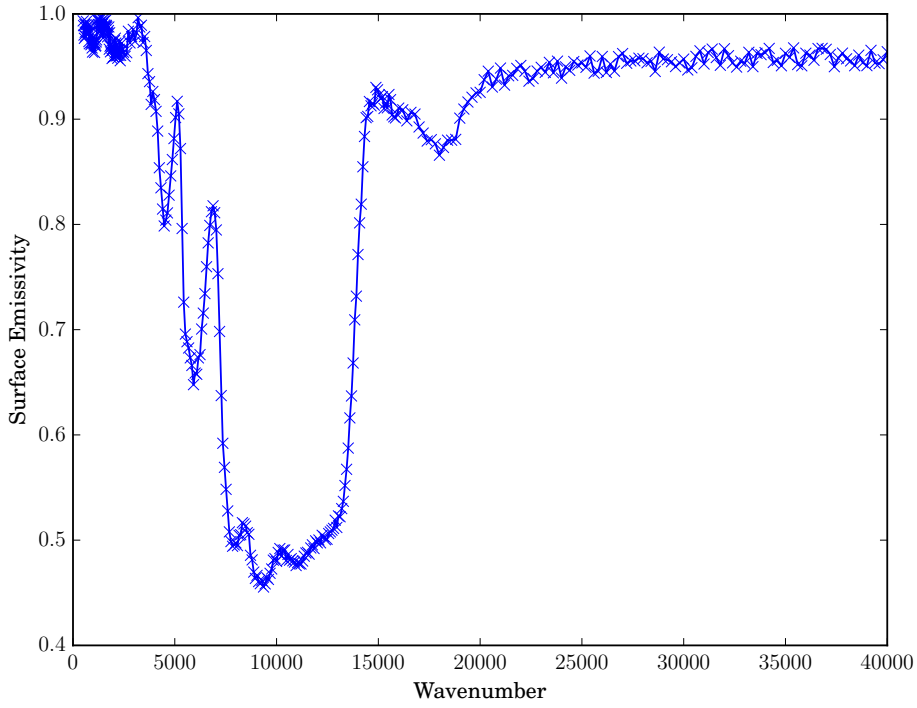
Comment: *In the shortwave, the albedo of the surface is important (as is the bidirectional reflectance distribution) and the surface emissivity is not. Clearly at 1.6 μm and in the O_2 A band, surface emissivity is not a factor. Yet surprisingly, the authors are able to retrieve it well given their DoF of 8.7 out of a possible 9. I am very surprised to see surface albedo missing from the state vector when it might be the most important state parameter for accurately modelling the radiance. Also, does the surface albedo vary from site to site? The range of surface albedos should be stated since this is important for understanding the influence of aerosols and clouds.*

Reply: There might be some misunderstanding here. For an opaque surface, surface albedo and emissivity must add to one, by Kirchhoff's law: surface albedo = 1 - emissivity at a particular wavenumber. By retrieving a scaling factor for the surface emissivity, we are retrieving surface albedo indirectly. **We have added one sentence on P10L24 to clarify this point in the revised manuscript.** Yes, the surface emissivity and thus the surface albedo vary from site to site. And the surface albedos for other sites depend on the surface properties of each site. This was deemed to be known information and thus was omitted in the manuscript. **It has been added to P10L24 in the revised manuscript to clarify this point.**

The figure on the next page shows the emissivity at test location 1. Based on the emissivity, the surface albedos for test location 1 are as follows:

4210-4260 cm^{-1}: 0.134 to 0.147
4260-4320 cm^{-1}: 0.147 to 0.160
5950-6100 cm^{-1}: 0.348 to 0.341

6190-6260 cm^{-1}: 0.330 to 0.318
13000-13170 cm^{-1}: 0.483 to 0.474

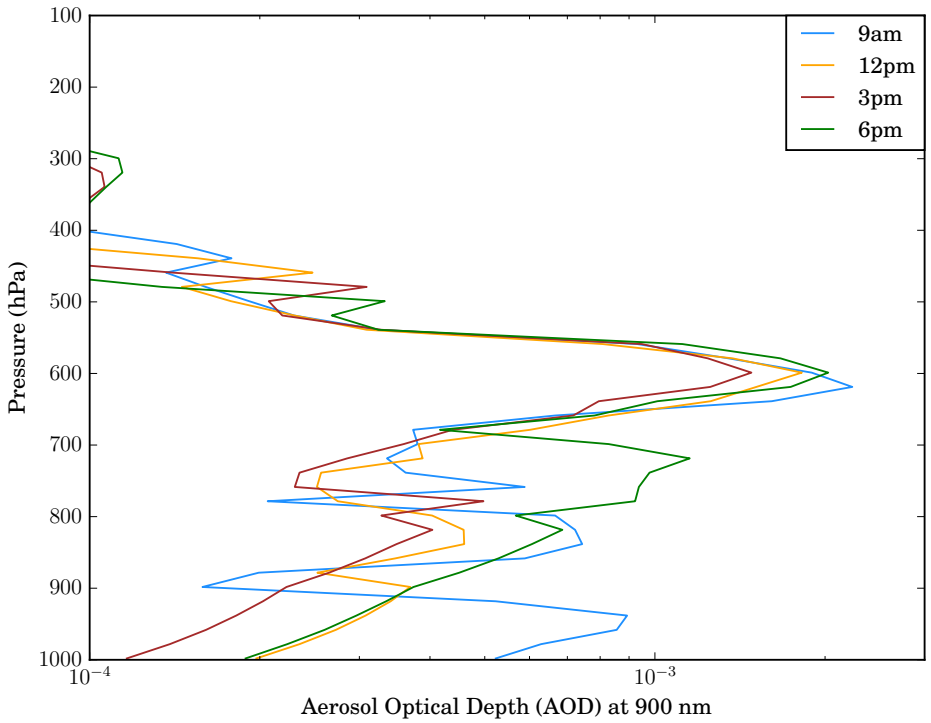


Comment: *The aerosol loading for the all-sky cases are not clear. An input OD of 0.2 is mentioned in section 3.3 but the authors don't disclose the input OD for retrieval results reported in section 3.2. Rather than be quantitative, the authors state "small amount" on P5817L9 and "less than ~0.30" on p5824L23 and "small amount" again on P5826L20. This is not acceptable. Furthermore, "amount" is a vague term.*

Reply: We would like to clarify that we did not intend to "cover up" the input OD for retrieval results. The input OD was not disclosed because this section was meant for a brief exploration on the influence of aerosols and clouds and the input OD is not new science to be presented. The figure on the next page shows the vertical profile of the aerosol optical depth at location 1. As this figure offers little new scientific insight to the reader, it was omitted from the manuscript. Again, this omission was not meant to cover up any undesirable details.

On P5817L9, the phrase "small amount" was used as a brief mentioning of the addition of aerosols and clouds to the model atmosphere. As section 2.2.1 is focusing on the forward model, we did not see the need to give the details of aerosols and clouds because they might deviate from the focus of this section. On P5824L23, "less than ~0.3" optical depth was the definition of the "small amount of aerosols and clouds" which pass through the pre-retrieval filtering. Because this is the amount of aerosols and clouds that a retrieval algorithm needs to deal with, we decided to study the total scattering OD that ranges from 0.0 to 0.20. On P5826L20, the "small amount of

aerosol and cloud contamination" is referring to total scattering optical depth less than ~0.30. It is agreed that "amount" is a vague term. A more quantitative term is total scattering OD, as used in this study. The phrase "small amount" was meant as a reference to the total scattering OD below ~0.30. **We have added one sentence to P15L8 in the revised manuscript to clarify this point.**



Comment: *Clear-sky retrievals should include aerosol. To me, clear-sky means cloud-free, not aerosol-free.*

Reply: This might be one way to classify "clear-sky" and "all-sky" conditions. We have explained how "clear-sky" and "all-sky" are classified on P5825L5 and this should not lead to confusion about the interpretation of the results by the readers.

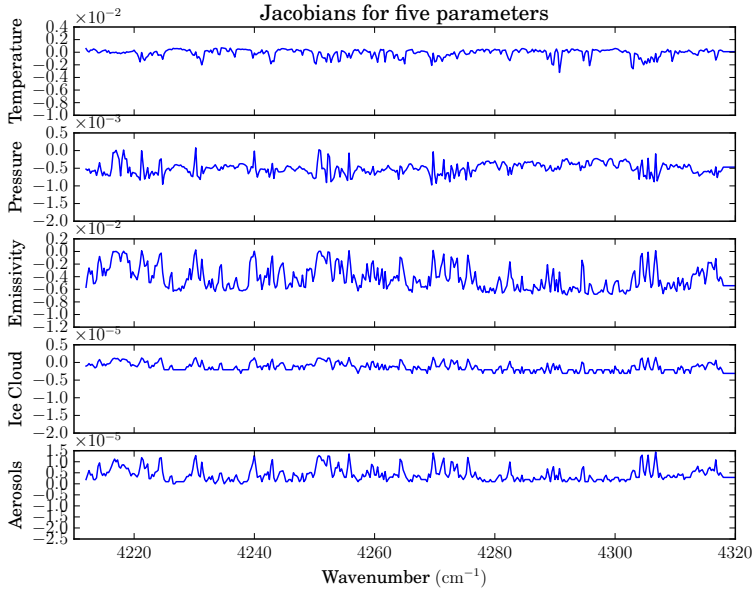
Comment: *Reading the last sentence starting on P5819, I am to understand that all nine parameters are biased by 3%, but did the authors mean only the trace gas parameters? If not, then isn't a frequency shift of 3% too large to retrieve? This would mean a shift of >300 cm^{-1} for the A band. Is that correct? Also, how is a 3% bias in the zero-level offset calculated?*

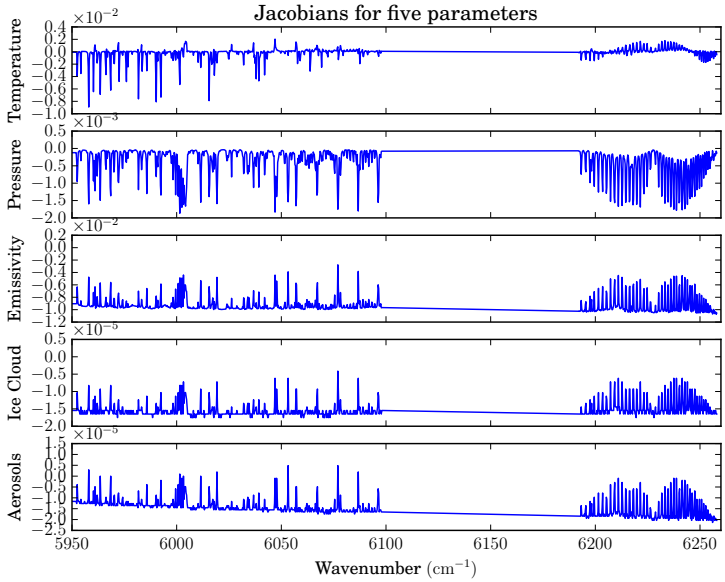
Reply: There might be some misunderstanding here. The sentence means that all the nine parameters are biased by 3%. The 3% uncertainty is based on the a priori frequency shift. For example, if the a priori frequency shift is 1.0 cm^{-1}, then 3% of it would be 3%*1.0 = 0.03 cm^{-1}. It is not 3% of the absolute wavenumber, which could be >300 cm^{-1} in the A band. The 3% bias in

the zero-level offset is calculated as 3% times the a priori zero-level offset, which has the same unit as the radiance simulated.

Comment: *I would like to see the Jacobians for five of these other parameters: temperature, pressure, surface emissivity, clouds, and aerosols. I would like to see the correlation coefficient for aerosols and cloud Jacobians. Also, what is the correlation coefficient between the Jacobian spectrum for pressure and each of the following Jacobian spectra: CH_4, CO_2 and H_2O.*

Reply: The figures below show the Jacobians for the five parameters. The correlation coefficients, r, are as follows: r (aerosol jacobian and cloud jacobian) = 0.91; r (pressure, CH_4) = 0.45; r (pressure, CO_2) = 0.53; r (pressure, H_2O) = 0.43.





Comment: *There is only one aerosol OD*

parameter in the state vector but four aerosol types are used. This seems unnecessary. It would be fine with me if only sulphate aerosols are included.

Reply: Over the targeted region, there are other types of aerosols present, so this is why four aerosol types are added to the model atmosphere. In real retrieval algorithm, one scaling factor could be used for each type of aerosols at each atmosphere level, as shown in Table 2 of O'Dell et al. (2012). In this simplified simulation study, only one scaling factor is used for all aerosols.

Comment: *The SNR should depend on the hour of the day but it does not. Either this should be changed or the last two sentences of section 3.1 should be deleted.*

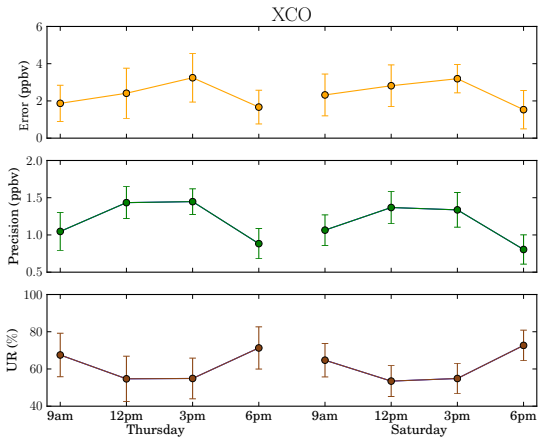
Reply: It is agreed that the SNR might change over the day if the integration time of the instrument is kept constant over the day. **A note on the constant SNR has been added to P14L27 in the revised manuscript.**

Comment: *The Miller et al. reference is for LEO. I believe Rayner et al. is the only appropriate reference. Rayner et al. have a 3 ppm requirement for CO_2, which is more forgiving.*

Reply: It is agreed that Rayner et al. is a more appropriate reference for GEO. **The reference to Miller et al. has been removed in the revised manuscript. The change could be found on P4L30.**

Comment: *The authors could have explored the ability to detect the weekend effect in CO_2 or CO. Otherwise, what was the point of using two different days of the week?*

Reply: We have analyzed the retrieval results over two different days of the week. The figure on the right shows the results for XCO over a Thursday and a Saturday. It shows that, regardless of the amount of CO during a weekday or a weekend day, the patterns for retrieval error and precision are similar. Therefore, we aggregated the results from different days to one single figure in Figure 7, instead of having one figure for each gas species. Figure 7 in the current manuscript is in a more condensed form and it still conveys the key scientific message.



Comment: *The authors do not consider the 2.0 μm CO_2 band which is considered valuable for aerosol and cirrus detection.*

Reply: The choices of spectra windows are constrained by many considerations including science priorities and project cost. The 2.0 μm CO_2 band was considered but was not chosen for this project. Other bands such as the A band are also valuable for aerosol and cirrus detection.

Comment: *It would be interesting to comment on the virtue(s) of an FTS rather than a grating spectrometer.*

Reply: As this study focuses on simulated retrievals, the virtue(s) of an FTS, compared to a grating spectrometer, is not in the scope of this manuscript. Because of this, there is no discussion on FTS v.s. grating spectrometer. Interested readers could find some discussions on this in the literature.

Comment: *P5811L17 Why mention AIRS? It is quite different spectrally from GeoFTS.*

Reply: It is true that AIRS is different spectrally from GeoFTS. AIRS was mentioned in order to have an exhaustive review of the satellite missions which have been measuring CO_2 and CH_4 from space. In other words, the mentioning of previous missions is not limited by the spectral bands used.

Comment: *P5812L2 SCIAMACHY had a revisit time of 6 days.*

Reply: The correction has been made to P4L5 in the revised manuscript.

Comment: *P5812L6 Note that OCO-2 has a smaller footprint than GeoFTS.*

Reply: This point is noted. As OCO-2 has a revisit time of about 16 days, the spatiotemporal scale (especially the temporal scale) of XCO_2 retrieved by OCO-2 would be insufficient for flux inversions over days or hours.

Comment: *P5812L13 "to realize" -> "thereby promising to realize" (since GeoFTS is only a proposed mission). Perhaps the authors should state the status of the GeoFTS mission. Has it been proposed? An earlier version of this manuscript referred to it as a "proposed mission".)*

Reply: The change has been made to P4L16 in the revised manuscript. The status of the GeoFTS mission has been added to P4L18. Yes, it is a proposed mission at this point in time.

Comment: *P5812L25 Given the wording of the first objective, equally important specifications are the spatial resolution and temporal frequency (along with the precision). I realize they are provided later but this seems like the more appropriate place.*

Reply: It is agreed that this is also a good place to mention specifications about the spatial resolution and temporal frequency. As this is in the Introduction section, we did not want to give too much technical details. This is why these information are provided later in the text.

Comment: *P5814L12 "1-10 km" -> "10 km". 1 km is not achievable given 2.7 km pixel length and width, but also cities are larger than 1 km^2 anyway.*

Reply: The change has been made to P6L11 in the revised manuscript.

Comment: *P5815L1 "four" -> "at most four usable" (This is evidence of excessive optimism).*

Reply: The change has been made to P6L26 in the revised manuscript.

Comment: *P5815L5 SNR is a measure of data quality. I suggest removing one of the two.*

Reply: The term "data quality" will be removed in the revised manuscript, as shown in P6L31.

Comment: *P5815L16 Are the aerosol profiles from GEOS-CHEM? If not, what is "realistic" (P5817L7)?*

Reply: Yes, the aerosol profiles are from GEOS-CHEM. **This point has been added to P8L24 the revised manuscript.**

Comment: *P5816L8 Convergence is stated in terms of the radiance but on P5819L12, convergence is determined in terms of the state vector.*

Reply: When the values in the state vector converge to stable values, as described in P5819L12, the match between the modeled radiances and the synthetic data, as described in P5816L8, will be deemed satisfactory. In other words, these two statements are equivalent.

Comment: *P5816L14 "Due to computational resource constraints". This sound phony. Some other excuse could be found or no excuse is needed at all.*

Reply: It is unfortunate that this phrase was deemed a phony excuse although it is simply stating a matter of fact. **It has been removed in the revised manuscript, as shown on P8L5.**

Comment: *P5816L22 This sentence is too simple. For example, the solar geometry and surface albedo are critical inputs to the forward model.*

Reply: It is agreed that the solar geometry and surface albedo are critical inputs to the forward model. This sentence is the first sentence in section 2.2.1 and serves as a general introduction/ description of the concept of a forward model. As an introductory sentence for this section and this paragraph, it is not meant to list down all the detailed inputs to the forward model, which are provided later in this section.

Comment: *P5817L10 "lower stratosphere" -> "near the tropopause".*

Reply: The change has been made to P8L26 in the revised manuscript.

Comment: *P5818L2 "Initially, the SNR is set at 300" A period should follow this statement and it should be moved immediately before the sentence that currently precedes it ("The outputs ..."). Otherwise, it sounds as if noise has been added to the forward model (2S-ESS) as well.*

Reply: It is agreed that the current statement might be misinterpreted. **The sentence "The outputs …" has been revised in P9L12 to clarify that noise is only added to the output from VLIDORT, not to the output from 2S-ESS.**

Comment: *P5818L8 Already mentioned Lambertian surface on P5816.*

Reply: This sentence is meant to reiterate the assumptions made in the forward model. Therefore, the Lambertian surface is mentioned again here.

Comment: *P5818L16 The reference to TES can be removed.*

Reply: The change has been made to P9L28 in the revised manuscript.

Comment: *P5820L14 "true" -> "observed"*

Reply: According to Rodgers's 2002 book on retrievals, "the true state" is a more appropriate term. We can derive "the retrieved state" from observations, but the phrase "the observed state" is rarely used.

Comment: *P5821L1 Jacobian is already defined better on p5819.*

Reply: The term is defined again as it serves as one of the diagnostics of the observing system. The definition is repeated for the sake of completion and consistency in section 2.3.

Comment: *P5822L7 Why is it brighter at 3 pm than at 9 pm according to the model? Are they almost equally close to solar noon? Is daylight saving time used?*

Reply: As the targeted region is North America, 3pm and 9am are not equally close to solar noon. The geographical location of the target region, i.e. North America, dictates the brightness of the reflected sunlight at 3pm and 9am. Daylight saving time is used because we are focusing on summer time over North America.

Comment: *P5822L23 The averaging kernel indicates that most of the elements of the retrieved state are coming from the observations. The averaging kernel does not tell you about truth. The "retrieval error" or the term I prefer, namely "retrieval bias, informs you about the closeness to the truth.*

Reply: It is agreed that the phrase "are close to the true state" on P5822L24 is better phrased as "are sensitive to the true state". **The change has been made to P13L10 in the revised manuscript.** It is also agreed that the averaging kernel does not tell the truth and the retrieval error informs about the closeness to the truth.

Comment: *P5824L2 There is no spectroscopic issue, unless different spectroscopy is used by LIDORT and LBLRTM. This would not be a real spectroscopic shortcoming. Real shortcomings in spectroscopy can be evaluated with real measurements.*

Reply: Yes, there is no uncertainty/shortcoming in spectroscopic parameters in this study. Our point is there is a large margin for other sources of error such as spectroscopic issue. We mention it so as to emphasize that, in real measurements and retrievals, the spectroscopic issue needs to be taken into consideration.

Comment: *P5824L12 "stronger signals" -> "stronger absorption signals"*

Reply: The change has been made to P14L25 in the revised manuscript.

Comment: *P5824L23 At which wavelength is OD<0.3?*

Reply: This OD<0.3 is at 755 nm.

Comment: *P5825L4 "higher instrument noise". For real measurements, there will be more noise at noon, but the SNR will be more favorable at noon than other local times (i.e. signal increases as well). For the simulations here, the SNR is diurnally constant (=300), so the authors need to clear this up.*

Reply: The value of SNR depends on the integration time, as mentioned in P5815L2. By changing the integration time, it is possible to maintain an approximately constant over the day. Furthermore, there is no validated way to model the diurnal variation of a GeoFTS' SNR. Because of these reasons, the SNR is held constant at 300 over different times of day. **This point has been added to P14L27 in the revised manuscript.**

Comment: *P5825L7 Black carbon does not scatter conservatively so "total scattering OD" definitely needs to be reworded ("total particle OD" or "total condensed phase OD"?). Also, this implies that Rayleigh scattering has been ignored but molecular scattering could be relevant in the cores of A band lines.*

Reply: It is agreed that the term "total scattering OD" might cause confusion and need to be reworded. **"Total particle OD" has been used in revised manuscript. The changes are found in P15L21, P15L31, P16L10, P16L18, P16L26, P16L28, P16L31, P17L17, P39L3, and x-axis label in Figure 8.**

Comment: *P5825L26 "all the" -> "all of the"*

Reply: The change has been made to P16L9 in the revised manuscript.

Comment: *P5825L27 Please elaborate on more effective pre-screening. Does this mean improved detection of optically thin clouds? I hope it does not mean that the OD threshold is lowered to <0.3.*

Reply: The pre-screening involves empirically-determined thresholds such as delta_p = absolute value of (a priori surface pressure - retrieved surface pressure) and the value of the cost function χ^2. For example, in O'Dell et al. (2012), the scenes with delta_p > 40 hPa or $\chi^2 > 2.3$ are flagged as cloudy and are filtered out. The "more effective pre-screening" refers to improved techniques/ algorithms which could correctly filter out spectra contaminated with aerosol and cloud OD even less than 0.30 (e.g., OD < ~0.20). In this way, it will probably lead to improved detection and filtering out of spectra contaminated with optically thin clouds and lower the overall retrieval errors. On the other hand, the more stringent filtering criteria might lower the throughput of the mission because more spectra will be filtered out.

Comment: *P5826L15 (see also P5825L23) "Kuang et al. (2000)'s findings" -> "the findings of Kuang et al. (2000)"*

Reply: The changes have been made in P16L7 and P16L30 in the revised manuscript.

Comment: *P5826L16 "from a simulated study." -> "using simulations."*

Reply: The changes has made in P16L30 in the revised manuscript.

Comment: *P5826L19 With two significant digits given in 0.30%, this implies ~330 ppm of CO_2 was used. Is that correct?*

Reply: Sorry for the confusion. 0.30% was round up from 0.26% = 1ppm/380 ppm *100%. **0.30% is changed to 0.26% and 0.15% is changed to 0.13% in the revised manuscript. The changes could be found in P17L2.**

Comment: *P5826L20 "slight" –>"slight in an absolute sense" (Relatively, it is a doubling).*

Reply: The change has been made to P17L3 in the revised manuscript.

P5828L10 This sentence belongs at the end of major scientific contribution #1.

Reply: Major scientific contribution #1 is about clear-sky retrieval results. By discussing the influence of aerosols and ice clouds, we could then conclude that there is a high probability that the requirements for GeoFTS will be met in both clear-sky and all-sky conditions. Therefore, this sentence is positioned in major scientific contribution #2.