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Interactive comment on "Comparison of the GOSAT TANSO-FTS TIR CH_4 volume mixing ratio vertical profiles with those measured by ACE-FTS, ESA MIPAS, IMK-IAA MIPAS, and 16 NDACC stations" *by* Kevin S. Olsen et al.

Anonymous Referee #2

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Olsen et al. (2017) compares GOSAT TANSO-FTS profile retrievals and partial columns of methane (CH_4) to coincident data from ACE-FTS, ESA MIPAS, IMK-IAA MIPAS, and NDACC, focusing on the upper troposphere/lower stratosphere (UTLS) over 2009-2013. This work expands an earlier TANSO inter-comparison in the Arctic to include global measurements from additional satellite and ground-based remote sensing instruments, aiming to identify possible zonal variability in the profile retrievals. Given the importance of understanding global CH_4 and the uncertainty in its recent trends, precise and accurate measurements with global coverage are very much needed. As GOSAT is one of the primary satellites in monitoring atmospheric CH_4 today, the as-

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Discussion paper

sessment of the spatial biases of TANSO measurements would give confidence to the scientific community's use of an important data set.

The approach described in the paper is comprehensive, and the methodology is thought-through. However, the exposition of the significance, conclusions, and limitations of this work require more development. The paper would benefit from a rebalancing of its structure, with the most critical changes being: (1) augmenting the discussion of previous and/or similar validation efforts of TANSO CH4, (2) paring down of the instrument background sections to focus on details directly relevant to their results, and (3) discussing the reasons for and implications of the differences highlighted in the comparison. Therefore, I would recommend publication of this manuscript after these points are addressed.

1 Scientific Evaluation

General Comments

1. This paper would benefit from a more rigorous analysis (or if already done, a more comprehensive description) of the causes of the differences noted in the results. The authors are thorough in considering different parameters, but the text lacks a synthesis of how these difference relate to the results of the comparisons. For example, what component of the differences between TANSO and the other instruments can be explained by spectroscopy verses a priori profiles used? This can be addressed for the specific parameters already mentioned in the text; e.g. many of the retrievals incorporate the same linelists–do those profiles show better agreement to each other? The implications of these differences for the scientific community's use of the TANSO product based on these differences would also strengthen the paper.

2. At several points throughout the paper, principally in Sections 2-5, the authors include technical details about the satellites/instruments that do not seem pertinent to the study presented (e.g. the inclination angles). The extraneous information was distracting and diluted the narrative of the instrument comparison. I recommend including only those details that the reader should know to understand and evaluate the results and conclusions of this comparison, especially if those details are published elsewhere. For necessary details where the connection to the present study is not clear, explicitly listing the relevance and/or implications would be useful. (e.g. The authors list measurement windows and spectral resolution but need to comment on how these inter-instrument differences might relate to the results.) The authors might also consider moving some of these details that are useful but not central to the paper into an appendix or supplement.

3. Given that methane is generally provided in units of ppb, do the authors have a specific reason for using ppm? If not, I suggest changing the references and figures to ppb. Figure 5 in particular would be more clear without extraneous zeros and decimal places.

Page 2

l. 11-13: The redundancy in the list of GOSAT objectives can be pared down. In addition, these objectives should be related, at least in part, to the objectives of this research. i.e. How does this paper contribute to the objectives of the satellites and the scientific community? This question is briefly touched upon on l. 31, but needs to be developed.

l. 24-26: Given the focus on zonal dependence, providing the latitude of Eureka in the text would be useful. Also, please include citations for PEARL and NDACC.

l. 29-31: As the main objective (and contribution) of this paper is to expand TANSO validation globally, more consideration of the issues of spatial coverage is needed, including a literature review of zonal biases (possibly does not exist for this particular TANSO product, but if that is the case this should be stated) and a description of mechanisms

that make the Arctic non-representative (e.g. polar vortex changing vertical profiles of trace gases and reducing the accuracy of a priori information).

Page 3

l. 5/Table 1: Please add year ranges to Table 1. It is not clear when mentioning the 2009-2013 time frame in the abstract whether all of the instrument measure consistently over that time period.

l. 7-14: If this is an outline, put section numbers after each sentence. If this is an overview, this paragraph might fit better in the methods section rather than the introduction.

l. 27: What type of coverage? Spatial? Spectral?

l. 28: Is this paper the first time methodology for CO_2 is applied to CH_4? If so, the retrieval (or the at least the aspects that differentiate it from other TANSO retrievals) should be described more.

l. 33: Do the 2011 updates have a reference (e.g. on the HITRAN website or used in a validation paper)? If so, please include a citation.

Page 4

l. 4-6: This sentence, with the important conclusions of the referenced paper added, should be moved to the Introduction, at the end of the second paragraph.

l. 9-11: If information about MAESTRO is relevant, include a reference for the instrument; otherwise this sentence can be removed.

l. 16: Is 5km the lowest altitude for ACE-FTS measurements filtered using the recommended flags (e.g. not a priori values)? Listing the lowest altitude of the data used in this paper would be more relevant, particularly for the discussion on vertical range in subsequent sections.

l. 33 and 36: Do the percentages listed apply to all trace gases or just methane? Please make this more clear.

Page 5

l. 4-5: Because this is the data version used, the results from Waymark et al. (2013)

should be summarized, as is done with the above papers.
l. 25: By "Initial guesses" do you mean a priori profiles? Please clarify.

Page 6
l. 6-7: What are the reasons for the outliers/discontinuities? Would these impacts the results of this study?
l. 30: Using "some information" is vague and does not tell the reader the relevance of the included information.
l. 32-35: Is there a reference for an inter-comparison of these instruments? How are inter-site differences due to different instrumentation accounted for in this study?
l. 36: Are those references for the most recent versions of the retrieval software? The spectroscopy has presumably changed since 1995/2004.
l. 37: Does "harmonized" mean consistent between sites? Please use a more clear term.

Page 7
l. 10: Given the differences delineated in this section, have you done a covariance analysis or sensitivity test to assess whether the results of the comparison depend on the retrieval software used, instrument, or any other difference across NDACC sites?
l. 20: Are these measurements representative of year, season, location, tropopause height, etc.?
l. 25: Please define "sunset/sunrise measurement" in this context.

Page 8
l. 5-6: Please summarize the results that the bias is consistent with.
l. 24: Why was reducing coincident measurements an objective, if you could average them and thereby reduce potential bias (c.f. Kulawik et al. 2016)? Was this a data processing issue from the large number of coincidences? If the coincidence criteria varies by instrument, some sort of bootstrap or sensitivity test with a subset of data should be run to see if the VMRs are different with the more lax coincident criteria.

Page 9
l. 8-9: Including references for the approach mentioned would be useful.
l. 19: Please define a z score and/or include a reference.
l. 26-27/Figure 2: Does using only the first 200 observations of the year capture any time-varying spatial coverage of the satellite data? Would it be more appropriate to use the first 20 observations of each month of 2012, for instance?

Page 10
l. 5-6/Figure 3: Please provide a more clear description of what the pressure levels in the legends correspond to (i.e. the pressure widths of the averaging kernel rows vs. the pressure on the y-axis), as relates to the findings of the paper. (You could perhaps include the simple averaging kernel equation if useful, but if the text becomes too detailed I would suggest moving this description to an appendix/supplement.) Also, if these pressure levels are meant to be compared across instruments, using a single colour scale for panels a-d would be helpful, e.g. binned into ranges or following a colour gradient.
l. 8-12: Please add the implications for these comparisons. Related to the previous comment, the remarks on "full-width at the half-maximum values" would be more understandable by rewording the phrase "values when considering the location of the appropriate pressure level" and adding a more clear description of the averaging kernel widths (p. 9 l. 33-34).
l. 25-26: How do you determine the influence of this dependence on the results? (This paragraph might need to be moved closer to the discussion of TANSO priors later in the manuscript.)
l. 32: What is the implication of the flat trends over mid-latitudes and tropics, as relates to the objective of this paper to determine zonal dependence of the retrievals?

Page 11 l. 28: Please include a reference for the claim that the NDACC a priori/measured pressure profiles are accurate. (This might fit better in Section 2.) l. 27-29: I'm not sure I follow the logic here; does this just affirm that interpolating to

a common pressure grid does not introduce additional bias or uncertainty? l. 30-32: Do these extrapolated values actually become part of the profile comparison? If not (as indicated in step 5 on p. 12 l. 10-11), why extrapolate these values at all? If so, profiles with extrapolated values included in the comparison would be problematic: the minimum altitudes for these instruments are so high that they tend to be in the region of the atmosphere where CH_4 varies significantly with altitude, and the extrapolation would have large uncertainties. l. 32-33: Is this sentence a way of saying that the averaging kernel equals zero where no measurements exist? Wouldn't this zero out the extrapolation referred to in the preceding sentence?

Page 12

l. 16-17: Do you look at the longitudinal variability for each zonal band? If so, does it vary between instruments?

l. 17-20: If my understanding is correct that you apply these filters only for individual points, as opposed to the entire profile, how do you account for heterogeneity in the underlying profile? For example, are all seasons represented at most altitudes ranges? If representation bias is not accounted for, the differences between the underlying measurements might account for some of the features illustrated in Figure 5, e.g. the strong agreement at high vs. low altitudes.

l. 21-22: I find it surprising that measurements within the polar vortex did not impact the results, unless the problematic profiles were discarded through other filters or a priori values were used. Do Holl et al. (2016) apply the same data flags listed in this paper?

l. 28-29: Following on comments made on the manuscript's introduction, an explanation of why zonal biases may exist should be included.

l. 29-31/Figure 5: Reiterating the last general comment, units of ppbv in the left-most panel would remove some of the extra text on the axes associated with the decimals and might be more intuitive for CH_4.

l. 31-32: Do the sizes of the bins alter the comparisons? Are these zonal ranges narrow enough in the Northern Hemisphere? i.e. Are the profile differences at 50-60N

comparable to 30-40N?
l. 37: Please give a brief explanation of what this statistic tells us (or justification for using it) as opposed to the general correlation coefficient and/or include a citation.

Page 13
l. 18-22: The pressure level at which CH_4 decreases is the tropopause height. Unless I am misunderstanding this paragraph, the implication is that the tropopause heights of the instruments are different, which would very likely account for at least some of the profile differences observed. How do the calculated the tropopause heights compare among the various instruments? If they differ, it would indicate that some of the assumptions underlying the pressure interpolation (outlined in the paragraph on p. 11 l. 25-29) might need to be reconsidered. Measurements for which the a priori values have a significant influence could be especially susceptible to tropopause height biases.
l. 24/28: By "below 90 hPa" do you mean less that 90 hPa or at lower altitudes? Similarly, does "Above 100 hPa" mean greater than 100 hPa, or at higher altitudes? Looking at the figure, the reader can deduce the appropriate answer but would benefit from less confusing wording.
l. 24-27: This result seems to me as one of the most important in the manuscript and deserves elaboration. Does the variability have any notable features? Does it depend on sensitivity (s) or a priori influence? Did you find covariance with latitude (i.e. within the 30 degree bins), tropopause height, or season?
l. 33-34/Figure 6: Do zonal differences exist in the unsmoothed data? How do the unsmoothed data fit into the goal mentioned in the introduction for assessing the applicability of Holl et al. (2016) to lower latitudes?
l. 35-36: This sentence is confusing due to the the vague phrasing and verbosity (e.g. "actual differences one would expect"). Please reword.
l. 37: More consistent across instruments? Or across altitudes for each instrument?

Page 14

l. 9-11: Why would the differences between ACE-FTS and ESA MIPAS be smaller? Does this shed light on differences between each of these instruments and TANSO?

Section 7.1: Does any of the methodology apply to Section 6 (e.g. criteria for minimizing the dominance of the prior) and vice versa?

l. 21-22/Figure 8: The way this is described, it seems contradictory with the caption on Fig. 8, "The vertical range of partial column integration varies for each pair of coincident profiles." If you mean that for each coincident measurement pair you match the vertical range of TANSO and each of the other vertical profiles, but that the vertical ranges across all coincident measurements vary, please describe this more explicitly somewhere in this section. Also, if that interpretation is correct, does the vertical range impact the distributions or correlations of the data? (This same question applies to Figure 9.)

l. 34: Why a sensitivity threshold of 0.2? This seems a little low. The minimum of three pressure levels also seems low unless they are contiguous (i.e. don't skip filtered out data in the profile). If the data points do not adjoin each other, did you apply criteria on how far apart the levels can be?

l. 5-7: How different are the results when these 23% are excluded? Do they account for the outliers in Figures 8 and 9?

Page 15

l. 10-20: Given that the values are all on a pressure grid, what is the advantage of integrating in altitude/z versus pressure/P? Do you account for water vapour (i.e. use "dry" P/T)?

l. 27: If partial columns with large gaps in the vertical are included (ref. my comment on p. 14 l.34), an uncertainty related with the interpolation should also be propagated through the calculation.

l. 29/Figures 8 and 9: Given the emphasis of the paper on zonal dependencies, please add a colour scale to each plot associated with latitude (bin)?

l. 35-37: What would cause a bias in the intercept? Altitude range? Spectroscopy?

Page 16

l. 8/Table 3: Why is the minimum altitude for the NDACC measurements so high? Figure 3 indicates that TANSO is at least somewhat sensitive closer to the surface that 3km.

l. 26: When combining the results, are all data weighed equally, or do you take into account the uncertainties of measurements? Is this the average across all latitudes, or is it a bias that is consistent for all latitudes? Have you also assessed whether altitude-related biases exist in the combined data?

l. 28-29: Did you find a bias in the sub-tropics or mid-latitudes?

l. 31-32: It is not clear what type of comparison was done? Regression? ANCOVA?

Page 17

l. 13: The mismatch in vertical extent you point out seems to indicate that these other satellites are not appropriate/useful for validation. If this is not your argument, please rephrase this sentence to make the argument more clear.

l. 15: Have you tried smoothing the TANSO profiles to NDACC to see if the agreement is robust?

l. 18-19: Given these biases, would you recommend "calibrating" the TANSO retrievals?

l. 19-21: Please include how this altitude feature varies (or doesn't) with altitude.

l. 25: It is not clear what "taken over altitude and latitude" means; please reword.

l. 26-27: What improvements are expected in future versions of the retrieval (e.g. priors, spectroscopy)? Based on your results, what would you recommend needs the most/least attention to produce a more accurate data product? Given the limitations of this TANSO product, what applications would it be suited to?

2 Technical Suggestions

Comments referring to the addition or removal of punctuation were included where I thought they might improve readability and are thus suggestions rather than corrections, except in cases where the serial comma in a list needs to be added.

Page 1
l. 3: Change "CO_2 and CH_4" to "carbon dioxide (CO_2) and methane (CH_4)"
l. 15: Change "examine" to "examining"

Page 2
l. 3-6: Add VMR (parts per notation) in parentheses after listed percentages.
l. 5: Change "investigated" to "investigate"
l. 7: Unless "over the equator" is specifically what is meant, change to "in the tropics"
l. 29: Remove the comma after "local"
l. 33: Change "made in coincidence" to "coincident"

Page 3
l. 30: Add a comma after "surface temperature"
l. 32: Add a comma after "(Maksyutov et al., 2008; Saeki et al., 2013)"

Page 4
l. 9: Remove dash after "ACE"
l. 19: Add comma after "Boone et al. (2005)"
l. 24: Remove comma after (Picone et al., 2002)"
l. 25: The use of "and" does not seem correct. Could be replaced with "assimilated into" or "from" depending on the relationship between the met data and the model.
l. 30: Add comma after "profiles"
l. 35: Add comma after "Odin"

Page 5
l. 7: Awkward placement of "(inclination of 98)"

l. 9: Add comma after "cloud parameters"
l. 11: Add comma after "2004"
l. 12: Reword end of this sentence, e.g. change ", but" to "with"
l. 24: Add comma after "limb scan"
l. 26: Add comma after "temperature"

Page 6
l. 20: Change the commas around "below 25km" to parentheses.

Page 7
l. 7: Rephrase "dynamical nature" to a more precise term.
l. 15-16: The use of and phrasing after the semi-colon is awkward and makes the sentence unclear.

Page 8
l. 11-12: The parenthetical is awkwardly worded; please revise for clarity.
l.12: Change "differences is also" to "differences are also"
l. 13: Please reword "When examining dates with several measurements" to make more clear.
l. 16-17: The grammatical structure of this sentence is difficult to follow. Please reword.

Page 9
l. 32: Add comma after "differ"

Page 10
l. 10: Change comma after "kernel" to semi-colon.
l. 13: Add comma after "role"
l. 20: Add comma after "altitudes"
l. 22: Change "70km. This is shown in Fig. 3e." to "70km (Fig. 3e)."
l. 22: Add comma after "development"
l. 27-28: Please reword. The structure of this sentence is difficult to follow, e.g. the verb ("are shown") appears twice and each has its own modifier.

Page 11
l. 22-23: Remove parentheses, and add a period or semi-colon after "retrieval"
l. 24: Please state which retrieval you're referring to (seems like the higher-resolution profile, but not self-evident).
l. 27: Remove comma after "equilibrium"

Page 12
l. 21-22: Change "looking for" to "filtering" and remove ", and then filtering these events," to make the sentence more clear.

Page 13
l. 20: I think a verb is missing after "VMR decrease" (e.g. "occurs").
l. 33: Add a comma after "zonally"

Page 16
l. 29: Change "or" to "and" ("0.014 ppmv or 0.020 ppmv"), and "Pole" should be plural.

Page 17
l. 10: Remove commas after "sensitivity" and "product"
l. 12: Remove comma after "altitudes"
l. 12-13: Please reword "and that there is a limitation on the useful upper altitude of its data product of below 15 or 20km" to follow the clarity and structure of the beginning of the sentence.
l. 14: Add "upper" before "troposphere" (Without the addition, this sentence is misleading.)
l. 20: Phrase starting with "and in a consistent manner" needs rewording for clarity.

Figures
Figures 2 and 10: The legend has two icons for every instrument, which adds extra visual clutter.
Figure 3: Several line colours do not appear in the legends of a-d. Instead of using a legend, you might consider labelling each line with the pressure using the same colour

for the text.
Figure 7: A heat map or similarly sequential colour scheme could be more helpful for this type of plot.
Figure 8: The “R” is missing on the R^2 line of each sub-figure, and it looks like some other letters and numbers might also be missing.
Figure 10: The degree symbol is missing between parentheses on the x-axis label. Also, please add additional tick marks on the x-axis. You might consider including light gray grid lines behind the data.

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