

Interactive comment on “Seasonal Variability of Stratospheric Methane: Implications for Constraining Tropospheric Methane Budgets Using Total Column Observations” by K. M. Saad et al.

K. M. Saad et al.

katsaad@caltech.edu

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We thank Referee #2 for their comments.

The primary change in the updated manuscript is a reprocessing of the TCCON tropospheric methane (CH₄) column-averaged dry-air mole fractions (DMFs), which is described in detail in added supplement, “Updates to Tropospheric Methane Data” (Appendix A). Although some of the regression statistics and comparisons have changed as a result of measurement updates, the main conclusions, the mismatch in tropospheric seasonality and the dependence of the stratospheric contribution error on

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tropopause height, remain the same.

In our responses below, page and line numbers included refer to the previous discussion draft. Appendices are referred to based on their order in the revised manuscript, and their headings are noted to avoid ambiguity.

It lacks precision in the text in many places (see specific comments), so are legends of some figures. Several important sentences, often when synthesizing results are confusing and not clear and make the reading not fluid at all with this version (see specific comments). I find the result section, a bit too descriptive, not providing systematically explanations or hypotheses for the inferred results. This has to be improved as it is not done either in the discussion part.

When discussing values presented in figures, the text now repeats these values more consistently. We have characterized the results more systematically, with greater detail and hypothesized explanations given for each feature. In addition to changing the wording where requested in the specific comments, we have altered ambiguous phrases, removed redundancies, and partitioned long sentences to make explanations simpler and more straightforward. We have also described and removed inconsistencies in terminology for greater clarity. The discussion of the figures in Section 3 has been updated to delineate the results quantitatively and with more detail. We also have made existing explanations more evident and provide additional hypotheses for results.

About the hypotheses, for instance among several other things reported below, I wonder why the aseasonal run disable the seasonal emissions and scale up the rest instead of prescribing the annual mean of seasonally changing sources ? This is strange as it changes the spatial distribution of emissions on the top of the suppression of seasonality.

We agree that producing aseasonal emissions by changing the seasonally varying

C2

fluxes to be constant throughout each year for each grid box would be ideal. Unfortunately, the model infrastructure made such a simulation difficult to execute as it required the emissions code to be re-written, risking differences due to compiling changes. Thus the scaling technique was developed as an alternative to assess first-order impacts of emissions seasonality. We have added this explicitly as a limitation that should be improved on in the future. However, most of the notable results, especially the phase lag in the tropospheric seasonality, are consistent between the model runs despite any differences in the spatial distribution of emissions. This demonstrates the robustness of our conclusions regardless of the emissions fields used. Additionally, the analyses comparing the base and aseasonal simulations are aggregated on zonal or hemispheric scales and therefore should not vary because of the spatial differences of their emissions at smaller scales.

Abstract : “large number of highly variable sources” not all methane source are highly variable. On what scale ? And sinks ? I suggest because of a large number of uncertain sources and sinks.

The phrase, “highly variable sources” has been removed for conciseness.

Page 2 : lines 1-5 : the words “atmospheric inversion” should appear somewhere in this paragraph.

The term “atmospheric inversion” has been added for clarity.

Lines 16-17:Do they have the same bias as aircraft observations of clear-sky only measurements (aircraft do not fly in bad weather conditions)? It is worth noticing this issue somewhere.

TCCON FTS instruments do not make measurements in rainy or completely overcast

C3

weather, which is now noted.

Lines18-20 : Fraser et al : how did they do that ? did they account for observation systematic errors as well ? Please be more precise when quoting papers. Idem for Wecht et al.

Additional descriptions of the approaches of Fraser et al. (2013) and Wecht et al. (2014) are now included in the introduction, and greater detail was added for several other references elsewhere in the manuscript. While Fraser et al. (2013) performed a variety of observing system simulation experiments (OSSEs) to test measurement and sampling biases, their focus was the information content of different types of observations in relation to atmospheric inversions. We have included their sector and regional error reduction results for the reader's reference.

Lines 33-35 : ambiguous sentence. Please rephrase. Indeed tropospheric CTM do not reproduce well stratospheric transport...

“Insofar as,” has been changed to, “Provided that,” to make the conditional aspect of the sentence more clear and reduce ambiguity.

Page 3 : Line 6 : “systematic model biases” : strange expression. Maybe systematic errors would be enough. What about the random part or errors? Do you address this as well ? Please reformulate.

By biases, we refer to the measurement-model mismatch due to inaccuracies inherent in the model; we agree that “systematic errors” also relays this meaning and have changed the wording. Because the focus of this work is on systematic differences between observations and the model, we do not quantify random model error except to note how the scatter and goodness of fit of the linear regression analyses compare

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between subsets of data (e.g. Northern vs. Southern Hemisphere and $X_{\text{CH}_4}^t$ vs. X_{CH_4}).

Line 6 : “seasonal cycle and spatial distribution of CH₄” concentrations ? emissions ? please be more precise.

This phrase now reads, “the seasonal cycle and spatial distribution of CH₄ DMFs” for clarity.

Line 15 : it would be good to briefly recall how the TCCON total columns are inferred. In particular, what is the influence of the modelled CH₄ profile used in the retrieval (as a priori) on the final product. As this profile comes from a model, it would be worth commenting on this considering the topic of the paper.

A brief description of the TCCON total column retrievals is now included at the end of the first paragraph of Section 2.1. In addition, a detailed description and references for the CH₄ a priori profiles have been added to the text. In testing the influence of the TCCON prior profiles in their comparisons to aircraft in situ profiles, Wunch et al. (2010) found that the total column retrievals using TCCON a priori profiles produced the same calibration values as those using the aircraft profiles as priors.

The newly added Appendix A, “Updates to Tropospheric Methane Data,” includes a more detailed description of how the $X_{\text{CH}_4}^t$ measurements are determined, processed to address spectroscopy-related errors, and calibrated to in situ aircraft profiles. The consideration of the chosen TCCON priors on the model comparison is addressed by smoothing the GEOS-Chem profiles using the TCCON scaled priors, as described in Appendix B3, “Model Smoothing for Measurement Comparisons.” The strong agreement between the integrated and smoothed GEOS-Chem column-averaged CH₄ DMFs also supports a negligible influence of the TCCON priors the results (Fig. 12).

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L16 : “precise” : please be more quantitative here or remove the word. How precise compared to surface networks for instance? how is your data uncertainty estimated ?

In addition to the details provided in response to the previous comment, Wunch et al. (2015), which describes in detail the determination of the TCCON total column uncertainty budgets and quantitative measures thereof, has been added to the references cited on p.3 l.16. A sensitivity study to assess uncertainties related to a priori profiles, spectroscopy, and instrumentation found aggregated X_{CH_4} errors to be below 0.5%, or about 5 ppb (Wunch et al., 2015). Appendix A, “Updates to Tropospheric Methane Data,” provides more details on the tropospheric measurement uncertainties, including $X_{\text{CH}_4}^t$ precision values and the aircraft in situ calibration curve, for reference.

Page 4 : Lines 14-15 : please provide a reference for emissions and OH. Do they vary inter-annually ? For OH concentrations, what is your ratio NH/SH ? More precisions are needed here. Indeed you release emissions every hour but their time evolution is monthly or annually probably. Please precise this not to let the reader think that we know methane emissions with an hourly time step !

References were cited for the “default” offline CH₄ simulation, which included a description of these fluxes. We have since added details and references for each of the emissions categories have been added for the reader’s convenience. The list of emissions, which were grouped by time evolution (annual, monthly, and daily), now includes references and additional details that should make the time scales of their variability more apparent to the reader.

The Northern to Southern Hemisphere ratio of 1.0 (monthly range of 0.975 – 1.02, applying a six month lag in the Southern Hemisphere) is consistent with the ratio of 0.97 ± 0.12 found by Patra et al. (2014). The tropospheric OH are monthly-averaged output from a GEOS-Chem tropospheric chemistry simulation (Park et al., 2004). The

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description of tropospheric OH and stratospheric loss parameterization fields now include references.

Section 2.2 : It would be useful to position GEOS-CHEM with other transport models based on previous Transcom-like experiment (e.g. : Patra et al., 2011): is it a “fast” model (inter-hemispheric exchange time ?), what about stratosphere/troposphere exchange time ? ... It would be very useful for other modellers to use the results of the paper.

Unfortunately, Patra et al. (2011) does not disaggregate the quantitative metrics asked for by the reviewer by model in the TransCom-CH₄ model comparison. Based on Fig. 8 therein, the interhemispheric exchange time in GEOS-Chem appears near the model median and slightly below observations over the 1996-2007 time series, which we have added to the conclusions for the reader's reference.

Page 5 : Line 1-2 : this first sentence needs precision : what is GGG2014 ? What is GEOS5 ? Acronyms have to be defined and explained

GGG is the name of the software and not an acronym. GGG2014, the current version of the TCCON retrieval software package, is described more fully in Section 2.1, where it is first introduced, to avoid confusion. The full name for the GEOS-Chem GEOS5 meteorology is now included on p.5 l.2.

Lines 10-15: the choice to disable the seasonal emissions and scale up the rest is strange as it changes the spatial distribution of emissions on the top of the suppression of seasonality. Why not prescribing the annual mean of seasonally changing sources ?

Please see the above response to the related general comment.

C7

Line 15 . What is “TCCON daily median scaled priors” ? you need to provide more details here.

GEOS-Chem smoothed column-averaged DMFs were only calculated for days in which TCCON measurements were made and were smoothed using solar zenith angles, vertical scaling factors, and surface pressures for TCCON measurements used in the comparisons. The added discussion of the TCCON retrieval in Section 2.1 provides a description of the vertical scaling factor that clarifies subsequent references. To further lessen confusion, this sentence has been changed to, “For comparisons with column measurements, model vertical profiles were smoothed with corresponding TCCON CH₄ averaging kernels, interpolated for the daily mean solar zenith angles, and prior profiles, scaled with daily median vertical scaling factors and interpolated to the daily mean surface pressures measured at each site, following the methodology in Rodgers and Connor (2003) and Wunch et al. (2010).”

What is the influence of these "priors" on the TCCON products and on the comparison proposed here.

As mentioned in the note above referring to the comment about p.3 l.15, Wunch et al. (2015) describes sensitivity experiments to assess the systematic errors in the TCCON retrievals that could potentially result from the a priori profiles. As Fig. 10 of that document illustrates, shifting the trace gas profiles down 1 km in altitude and increasing the temperature by 1 K and pressure by 1 hPa throughout the vertical profile each alter X_{CH_4} by about 0.05 – 0.1%. For the purposes of this work, the strong agreement between the GEOS-Chem column-averaged CH₄ DMFs and those smoothed using the TCCON scaled a priori profiles, as described in Appendix B3, “Model Smoothing for Measurement Comparisons,” demonstrates the unlikelihood of the TCCON priors being the reason for the measurement-model disagreement (Fig. 12).

C8

Line 20 : "While X_{CH_4} 20 changed slightly" : how much is the change ? please provide % for instance. Why only testing above levels ? please provide explanations.

The median change in $X_{\text{CH}_4}^t$ of about 1 and 5 ppb for a respective one and two-level increase in tropopause is now stated.

Accurately representing GEOS-Chem's tropospheric column for the purpose of comparison to measurements depends on setting the tropopause so that the calculation from model output is consistent with the way the model defines the troposphere. Shifting the tropopause level allowed us to test the degree to which calculating $X_{\text{CH}_4}^t$ using the daily average tropopause could bias the comparison. Furthermore, because the vertical gradient of CH_4 is steepest across the UTLS, choosing a lower tropopause level would change the vertical integration much less than choosing a higher level. Thus, integrating to higher pressure levels would provide a better measure of sensitivity to the integration tropopause height chosen.

Line 24: "small". Please be more precise. Remain within ± 5 ppb for instance ? Idem for larger NH changes : 'varies from -10 to +13 ppb ?

Quantification of the seasonal cycle has been added: "within ± 4 ppb" for the Southern Hemisphere and "varies between -10 and $+13$ ppb" for the Northern Hemisphere troposphere.

Page 6 : Lines 1-2 : what do you mean by " common " ? Why the age of air increases when seasonality is suppressed ? Please provide more clear explanations.

Because the transport of tropospheric air to the stratosphere air is governed by vertical

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ascent in the tropics (Brewer, 1949; Dobson, 1956), stratospheric air has a shared source of CH_4 that lessens the interhemispheric gradient seen in the troposphere (Boering et al., 1995, 1996). The age of air does not increase with dampened seasonality; rather the signal of tropospheric seasonality in a given parcel of air dissipates as its residence time increases (Mote et al., 1996). We now discuss this in more detail in Appendix A, "Updates to Tropospheric Methane Data."

Line 3 : "relatively short" : please provide an estimate

The model's equilibrium lifetime of CH_4 in the stratosphere is about 22 months, which we now state in the text.

Page 7 : Line 9-10 : tropospheric slope does not seem lower than one for southern stations. Indeed it seems there is a little north-south gradient in the tropospheric slopes. Did you investigate it ?

The tropospheric slope did not have an interhemispheric difference prior to the $X_{\text{CH}_4}^t$ update. However, with the updated $X_{\text{CH}_4}^t$ observations, the plots show interhemispheric differences in both $X_{\text{CH}_4}^t$ and X_{CH_4} . These Northern and Southern Hemisphere comparisons between TCCON and GEOS-Chem are described fully in Section 3.

Figs 4 : this figure is not enough analysed. You do not comment : - the negative bias of GEOS-CHEM at most sites for the tropospheric & total columns (4ab) - the fact that stratospheric columns of GEOS-CHEM seems underestimated for more southern sites and overestimated for more northern sites (4c) - possible reasons for the poorer agreement in the stratosphere.

The discussion of Fig. 4 now includes a systematic description of the plots, with associated hypotheses. The underestimation of CH_4 concentrations in GEOS-Chem has

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been documented elsewhere. In the TransCom-CH₄ model comparison, GEOS-Chem CH₄ concentrations were lower than the model median, and when using the same OH fields much lower than the range of other models (Patra et al., 2011). The negative bias was previously described as an offset when discussing the impact of the aseasonal simulations, and we have added that the direction of the offset (i.e. GEOS-Chem is systematically low) and provide a hypothesis for why the offset changes between simulations.

The stratospheric contribution of CH₄ increases from the equator to the poles due to the zonal gradient in tropopause height. We have added a discussion of the zonal gradients in the measurement-model differences in $X_{\text{CH}_4}^t$, X_{CH_4} , and the stratospheric contribution. We also directly compare the agreement (both slopes and R^2 values) across plots and hypothesize why correlations vary for different vertical levels.

You may also consider two slopes, one for the southern stations (larger than 1) and one for the northern stations (smaller than 1) on fig 4a, or a non linear continuous decrease of the slope from south to north. Why only keeping a global slope ?

We had plotted regression lines across all sites in Fig. 4 and listed in the text the individual hemispheric regression results for the stratospheric contribution. However, we agree that providing regression equations for each hemisphere is more illustrative. Regression lines and equations for Northern and Southern Hemispheres now appear on the plots in Fig. 4.

Page 8 Line 3-6 : any possible explanation for the differences with ACE ?

The structure of the differences with ACE-FTS measurements illustrated in Fig. 5 demonstrate that the cause is systematic to the model. GEOS-Chem is too low above the tropical tropopause in both boreal spring and fall and too high in boreal spring di-

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rectly above the Northern Hemisphere mid-latitude tropopause and in the Southern Hemisphere high altitudes. The ACE-FTS data gaps in the tropical troposphere prevent assessing whether vertical ascent into the stratosphere is too weak. Because the stratospheric loss parameterization is produced from NASA Global Modeling Initiative (GMI) model output, biases in the rate of loss could result from intra-model differences in transport schemes. A more thorough description of Fig. 5 and possible explanations for differences have been added to the paragraph on p.8 l.3.

Additionally, the ACE-FTS climatology plotted in Fig. 5 is an older version of the measurements (v. 2.2, Jones et al., 2012), which also could impact some of individual grid box differences; however, a comparison to the monthly means of the v.3.5 CH₄ DMFs (which are used in the $X_{\text{CH}_4}^t$ calculation) indicate that the data version likely would not change main features of Fig. 5.

Line 12-14 : “As the effective..pressure heights” : unclear sentence. Please rephrase.

The sentence has been rephrased: “The disagreement exhibits a large spread for relatively few tropopause pressure heights because the model’s effective tropopause, that is, the pressure level at which the model divides the troposphere from the stratosphere in GEOS-Chem, is defined at discrete grid level pressure boundaries.”

Page 10 line 10 : “production” or emissions ?

We infer that the referee meant p.10 l.5 and have changed “production” to “emissions.” Otherwise, we do not understand the comment in the context of p.10 l.10.

Page8-9 Line 15-4 : the part about troposphere is confusing as figure 6b shows similar trend for stratosphere and troposphere but you mention in the text much

C12

lower sensitivity. Please clarify this section.

While the slope is similar between the stratospheric contribution and tropospheric column, the correlation coefficient is higher for the stratosphere than the troposphere, meaning that the tropopause height can explain a higher percentage of the variance in the measurement-model mismatch for the stratospheric contribution versus $X_{\text{CH}_4}^t$. Moreover, despite the similar slopes, the direction of the relationship with respect to $\Delta\text{CH}_4=0$ is opposite: Fig. 6 shows that the mismatch increases as the tropopause height decreases for the stratospheric contribution (with the model's contribution of the stratosphere becoming larger than that of the measurements) and vice versa for the tropospheric mismatch (with the measurements and model showing better agreement when the tropopause height is lower). These points of clarification have been added to Section 3.1.

Page 10 Line 5 : "production" do you mean emissions as there is no methane 3D production in the atmosphere ?

As stated above, "production" has been changed to "emissions."

More, your statement brings more the summer large wetland emissions as an explanation for the phase of the modelled signal than the loss which should produce more a fall maximum as in surface observations (although Park Falls is not the best example to discuss seasonal variations as the signal is complex). Please clarify.

We agree that the emissions are likely the main driver of the model's surface seasonality, and we have removed, "and loss," for clarity.

Park Falls was chosen because of the TCCON sites that also have surface observations, the $X_{\text{CH}_4}^t$ seasonality most closely matches the Northern Hemisphere mean

C13

shown in Fig. 7; thus Fig. 8 provides a good basis to compare surface and tropospheric column measurements. While the site does have a complicated seasonality near the surface, we find it notable that GEOS-Chem is able to capture several of those features, especially the local minimum in October, but still deviates from the observations, as we note on p.10 l.8.

Page 11 : Lines 1-3 : please develop a bit why you discard OH as an hypothesis to explain the inferred changes?

The sensitivity experiments we ran tested a number of different OH (as well as emissions and meteorology) fields, which included scaling the default OH fields and using different scalings of the "Standard Chemistry + Biogenic VOCs" OH output (which is now described in more detail in Appendix B1, "Equilibrium Sensitivity Experiments"). The seasonal phase shift appeared in all simulations, regardless of OH used, although the seasonal cycle amplitude and the shape of the springtime maximum varies between simulations. A table delineating these simulations has now been added to Appendix B1. Additionally, p.11 l.2 now refers to a figure, also in Appendix B1, which illustrates the tropospheric seasonality of each of these simulations, as well as deviations from the base simulation.

Lines 7-8 : "The model sensitivity kernel implicitly includes.. " well do you mean variance matrices associated with observations ? with prior emissions ? Indeed, transport errors are generally implicitly include in atmospheric inversions by inflating observations errors but are not part formally of the variance matrix of emissions. Lines 7-8 : "which are compounded if vertical levels are subject to different errors" Confusing sentence. What do you mean ? please clarify.

The sensitivity kernel refers to the linear operator that maps CH_4 emissions to CH_4 concentrations; together with the error covariance matrices, the sensitivity kernel is used

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to calculate the gain matrix used in inversions to determine posterior emissions. The literature is inconsistent in how to refer to this operator; thus we use the term "sensitivity kernel" because we thought it describes the function of the operator: to provide the change in the CH₄ concentration resulting from a perturbation to emissions for a given grid box. The response of modeled CH₄ concentrations to changing emissions depends on the model's transport and chemical loss, as well as assumptions about when and where fluxes occur. Therefore, uncertainties in these terms will be implicitly included in the sensitivity kernel. We have clarified what we refer to as the sensitivity kernel after the introduction of the term and have altered the wording to make the logic more linear. The subsequent sentence now states, "The model's stratospheric response to emissions perturbations differ from that of the troposphere and are subject to different transport and loss errors."

Page 12 : Line 6 'Although the stratosphere accounts for about 30% ' if you refer to top panel of figure 9, I suggest up to 35 % (JJA)

The top panel of Fig. 9 is the fraction of total emissions that are seasonally varying (that is, from wetlands, rice paddies, biomass burning events) in GEOS-Chem. The 30% value cited is the mean fraction of the total column of CH₄ (in units of molec-cm⁻²) that exists in the stratosphere.

Fig9 : The legend of figure 9 is unclear. Top panel : fraction of what ? Bottom panel : the orange curve is a difference or the error of the aseasonal ? Unclear.

The upper panel of Fig. 9 is now labeled. As the caption reads, the orange curve is the difference between base and aseasonal simulation tropospheric columns. The label provides a qualitative description to improve on the originally submitted figure after we received feedback that the label, which explicitly stated that the curve is the tropospheric difference, was unclear.

C15

line 10 : "The seasonality of the stratospheric error will therefore distort the inversion mechanism and thus posterior emissions estimates." : well only if these error are not included in the inversion variance matrices. I would be more comfortable writing may distort or precise the conditions of influence of the seasonality in the stratospheric signals on surface emissions through inversions.

The uncertainties associated with transport are generally accounted for in inversions as a subjective percent error applied to all grid boxes, which would not capture the stratospheric errors presented here. Incorporating stratospheric uncertainties into the error covariance matrix would require a thorough characterization of those errors as a function of longitude, latitude, altitude, and month. Such efforts would be indispensable in improving the forward model, but our concern is that the error covariance matrix is not equipped to correct for these systematic biases. The conditions of influence of the stratospheric seasonality are delineated in the subsequent text.

Line 11 : "product of transport errors" : how did you evaluate the possibility of issues related to OH radicals ?

We infer that this refers to p.14 l.8. As mentioned above, we ran sensitivity experiments testing various OH fields, and these are now described in more detail in Appendix B1, "Equilibrium Sensitivity Experiments." Because the tropospheric phase shift appeared in all simulations, regardless of OH used, we believe that the tropospheric OH cannot account for the error in seasonality. We have added a table describing these simulations and a figure that plots the tropospheric seasonality of each of these simulations to Appendix B1.

Lines 10-12 : it is never mention except in caption of figure 4 that 't' in CH4t refers to troposphere

C16

The description of the tropospheric CH₄ columns introduces the superscript *t* notation to indicate a tropospheric column-averaged DMF (p.4, l.1).

“their emissions are very uncertain” : you may quote a recent estimate such as in Kirschke et al. 2013 or IPCC.

The 2000-2009 range for natural wetlands given by Kirschke et al. (2013) (142-284 TgC·year⁻¹) is now included.

Page 13 : Lines 7-8 : “both the magnitude and seasonality of the difference is significant” : the unit (tons) makes it difficult to say so. There is obviously a sensitivity if transport error shift the seasonality but what does it give in terms of ppb ? or in terms of % of initial emissions ? This would be more clear for the reader.

The value plotted in Fig. 10b is a sensitivity, in units of kgCH₄ per 1 ppb, and can be thought of as the change in emissions needed to increase the DMF at the surface by 1 ppb. Because the seasonality of wetland emissions is such that many grid boxes have no wetland emissions in the winter (Fig. 10a), the emissions related to the phase lag as a percentage change from the prior would produce infinite or very large percentages. Thus, presenting the values as percentages would provide a large range of values but very little information about the absolute emissions. We have set the units of Fig. 10 a and b equal, to make the comparison more clear to the reader. Additionally, we have updated the calculation as the sensitivity to 1 ppb increase in CH₄ over the tropospheric column, not merely at the surface, as the focus on this analysis is the assimilation of column data.

Lines 8-10 : “The largest disagreements between measured and modeled X_t occur ... than annually.” This sentence is unclear to me. Please rephrase.

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This sentence has been expanded and clarified, “Large differences between measured and modeled $X_{CH_4}^t$ are concurrent with low emissions from seasonal sources. The adjustments to prior emissions produced by larger measurement-model disagreement that occur when seasonal sources are a small fraction of total emissions will overestimate posterior emissions from aseasonal sources. Thus these seasonal errors will bias source apportionment toward emissions that do not vary on timescales shorter than annually.”

We have also added a more explicit description of the relationship between the seasonality of measurement-model disagreement and that of emissions that vary monthly before the discussion of Fig. 10.

Page 14 : line16 “the meridional gradient” of what ? emissions ? concentrations ?

This sentence has been changed to clarify that we refer to the meridional gradient of X_{CH_4} .

Unit of figure 12 ? Kg/yr ? Maybe change to Tg/gridbox or Tg/yr/latitude ?

Figure 12 plots CH₄ column-averaged DMFs in units of ppb, as described on the labels. If referring to Fig. 12, however, the units on the figure are listed as “ΔCH₄(Tg mo⁻¹).” The caption has been changed from “Tg” to “summed over each zonal band, in Tg·mo⁻¹” for consistency.

Conclusions Line 3 : re-precise in the start of conclusion the you used GEOS-CHEM and what are XCH₄ and X_tCH₄ as it has to be readable by itself.

The phrase “retrieved and modeled X_{CH_4} and $X_{CH_4}^t$ ” has been changed to “TCCON and GEOS-Chem pressure-weighted total and tropospheric column-averaged DMFs

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of CH_4 , X_{CH_4} and $X_{\text{CH}_4}^t$ to be more readable.

Page 15, lines 1-5 : If stratospheric CH_4 is largely independent from tropospheric CH_4 , is it worth developing full tropospheric and stratospheric chemistry models or prescribing stratospheric CH_4 based on satellite observations is enough ?

The insensitivity of the stratosphere to perturbations in tropospheric CH_4 suggest that prescribed stratospheric CH_4 could be prescribed in such a way that ensures mass conservation. For example, the stratospheric fields could be scaled according to the mass flux from the troposphere. As models develop their representation of stratosphere-troposphere exchange, however, the conservation of mass will need to be more carefully considered. Thus, more developed linear schemes for stratospheric CH_4 , such as the UCX mechanism we cite or Slimcat (Monge-Sanz et al., 2013), could provide computationally inexpensive ways to set stratospheric CH_4 .

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