Atmos. Chem. Phys. Discuss., 13, C4829–C4852, 2013
www.atmos-chem-phys-discuss.net/13/C4829/2013/

Atmospheric Chemistry and Physics Discussions

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Interactive comment on "On the consistency between global and regional methane emissions inferred from SCIAMACHY, TANSO-FTS, IASI and surface measurements" *by* C. Cressot et al.

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Received and published: 16 July 2013

We thank both referees for their constructive comments. We have addressed all the issues they have raised. In addition, we have redone all IASI and SCIAMACHY computations with a different data selection as follows.

- In response to a remark from Reviewer#2 about the consistency between TANSO-FTS results and IASI results, we have tried a more restrictive quality control on the IASI observations, in which the data are removed when their departure from the prior simulation is larger than 3-sigmas (i.e. 3 times the standard deviation of the observation errors). It removes about 10% of the IASI data, improves the convergence of the minimization and makes the initial inversion configuration much more consistent with the other observing systems. This new result reinforces our conclusion about the usefulness of the IASI retrievals.

- We have found that some daily files of our SCIAMACHY observations in input to the inversion system had been corrupted, resulting in less data in boreal winter and autumn. The linear regression with the air mass factor is not affected, but the new bias-corrected results do not further diverge from the surface results any more. The other results with SCIAMACHY remain qualitatively similar to the results previously shown.

1 Referee#1

Very useful comparison of three different satellite instruments using different detection and retrieval techniques for column heights of methane to infer global and regional emissions. Also compared with ground based measurements. Handling these very different data sets by assimilation into an atmospheric chemistry model seems the only way to produce consistent error statistics. Improvement in CH_4 emission budgets is a commendable result. It is appreciated that data retrievals are taken at face value from satellite data provider. However, outlyer SCIA data beg the question on possible causes. Some discussion on whether spectral resolution and spectral interval selected for measurement of CH_4 ro-vibration spectra is appropriate, detector degradation and lost pixels all could add to understanding the cause of discrepancy. TANSO being an FTS uses quite different detection

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technique from SCIA, all having their own peculiarities. Furthermore, retrieval techniques are very different, SCIA/TANSO relying on ratioing with CO2, where interference from other lines (CO) could play a role. IASI, not sensitive to lower tro- posphere uses neural network trained on plausible answer, hence dependent on prior information.
Comment
I would appreciate some discussion on the underlying cause for data discrepancy

Among the possible causes for state-dependent biases in the SCIAMACHY data that would make our results inconsistent, detector degradation is an obvious one. To test this hypothesis with the paper tools, we could extend our study to previous years, despite the impossibility to compare with GOSAT results and, for the years before 2007, with IASI results. Given the large computational involvement of such results (several 10,000 CPU hours on the supercomputers of CCRT required for the Monte-Carlo study), we prefer to leave this study for the future.

and suggestion for ways to improve data set consistency.

Whatever its cause, the inconsistency could be damped, or even removed, with better characterization of the error statistics of the retrievals. By construction, the TCCON validation data cannot be extended for past dates, which limits the possibility to refine these statistics from ground truth. Simulations from chemistry-transport models, forced by surface-based inverted fluxes, are much less reliable than TCCON retrievals, but represent a source of information that may be further studied as a surrogate, but leaving no major independent data anymore.

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Referee#2

General comments

The authors apply an inversion scheme to a number of observational datasets of atmospheric methane. Comparison of derived methane sources and sinks allows them to indirectly evaluate the consistency between these datasets. An interesting additional feature of this paper is the use of diagnostics for the variances of observation, background, and analysis errors, which allow tuning the background and observation error covariance matrices. However, there are very serious issues with the paper, and major corrections are required to make the paper publishable. My main concerns are the following.

1. The conclusion on consistency between the satellite datasets is not justified. Even in the initial configuration with very conservative observation error settings, the global posterior fluxes do not agree within their respective uncertainties. With the tuned error covariances the posterior fluxes are simply inconsistent.

Following the usual convention in our field, our posterior emissions have been presented with 1-sigma uncertainties which represent 68% of the probability density function (pdf) only. Therefore we did not expect that the 1-sigma error bars systematically overlap: the overlap at 2-sigmas (96%) for some cases is enough. Our results fulfilled this criterion, which justified our conclusion about the statistical consistency between the different datasets. However, your remark led us to reconsider our algorithms and to use a more restrictive data filtering for IASI (see our general introduction above). By doing so, all posterior fluxes inferred from all configurations now agree within their 1-sigma uncertainties (except SCIAMACHY for which some of the regional fluxes agree within 2-sigma uncertainties), which reinforces our conclusion.

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2. The transport model error estimates are completely irrealistic, leading to strongly overestimated observation errors, which is indeed confirmed by the tuning diagnostics. The basic inversions should be redone with realistic transport model errors of around 2% (Figure 7 can aid in making this estimate) instead of 8%.

It is usual practice to inflate error variances to compensate for missing correlations (Chevallier 2007), in particular for transport errors because they are known to be heavily correlated. This complicates the interpretation of the resulting variances. In this context, our prior guess at 8% for these hybrid variances does not seem to be unrealistic to us.

3. The results of the inversions with tuned error covariances are ambiguous. This may be partly due to inaccurate reporting (see next point), but also because as the authors state one iteration may not be sufficient. However, the latter argument is used selectively for cases that do not satisfy the expectations.

We will present the tuning as a sensitivity test in the revised version of the paper rather than as a final answer to error assignment, and will discuss its limit.

In any case, the conclusion that the quality of the fluxes is improved after tuning the error covariances is not justified.

Based on independent data (the surface measurements), we find that the quality of the fluxes is improved after tuning for some of the cases but we agree that the benefit is not systematic. We will clearly state this.

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> 4. The manuscript is very sloppy. There are countless inconsistencies between numbers in different parts of the text, and between text, tables, and figures.

Our tables and figures had been updated several times in the course of the study, but our last update was incomplete. We sincerely apologize for the resulting inconsistencies and for the inconvenience they have caused. Further to the reviewer's comment, we have revised each individual number in the tables and in the figures to make sure that all results are now correct as they will appear.

> Specific comments
>
> P8025, L2 and furtheron: The term methane weighted atmospheric columns sounds strange (what is weighted?). I suggest changing to methane column mixing ratios.

Depending on the wavelengths used in the satellite retrievals and on the prior information that they include, the retrieved columns are weighted more or less towards the surface or towards the top of the atmosphere. This is rigorously described by a retrieval weighting function or averaging kernel. This will be made clearer in the revised version.

> P8026, L3-5: Some references demonstrating the use of inversions to improve both global and regional methane flux estimates would be appropriate here.

We will insert references to Houweling et al. (1999), Bousquet et al. (2006), Bergamaschi et al. (2009, 2010), Pison et al. (2009)

> P8026: I am missing a clear statement on the goal of the study.

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We will make it clear that we aim at comparing the four observing systems in order to assess the consistency of their information about methane emissions over the globe.

> P8030: Some more explanation of the method of Desroziers et al. is needed. In particular, it should be made clear that Eqs. (5)-(8) are not equalities. The left- and right-hand sides are only equal if the error covariance matrices have been perfectly defined (and the tuning aims at moving towards this condition).

This will be done in the revised version.

> Please explain also with equation(s) how Eqs. (5)-(8) are applied to the ensemble defined by all observations. I guess this is done by summing the diagonals

We indeed applied Eqs. (5)-(8) to the ensemble defined by all observations by summing the diagonals of the full matrices. This will be made explicit in the revised version.

> Next, clarify that the prescribed error variances are calculated by evaluating the RHS of Eqs. (5)-(8) , and the diagnosed values are calculated from the LHS.

We will clarify how the assigned error variances and their corresponding diagnosed values are calculated.

> [explain also how HBHT is evaluated]

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We will add that HBHT (or HAHT) variances are evaluated in the observation space from the mismatches between the prior (or analysis) state of the perturbed members of the Monte-Carlo study and the prior state of the corresponding inversion.

> Then the ratio is introduced as diag/var, but in the remainder of the text, and in Table 3, the ratio is var/diag. This should be made consistent.

The ratio is defined as var/diag but was erroneously written. We thank the reviewer for having spotted this mistake that will be corrected.

> Finally, isn't the full variance the sum of the observation and prior variances?

The full variance is the sum of observation and prior variances indeed. We used the expression 'full variance' as an abbreviation. We will make this clearer.

> P8031, L23: Is the production of OH obtained, or the concentrations?

These two quantities can be deduced from each other by our CTM and it is equivalent to talk about one or the other for our system.

> P8032, L14: I guess fluxes should be columns.

This will be corrected.

> P8033, L16: For MCF, we use the monthly variances . . . : to do what?

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We use them as a proxy for observation errors (that are driven by transport errors), as indicated in the following sentence. We will reshape the two sentences to make the text more fluid.

> P8034, L4: The reflection of solar radiation is not (necessarily) weak at high latitudes.

This statement will be corrected.

> P8033, L1-3: Please give typical values of observation and estimated transport error. This places the values for satellite columns into context.

We will do that

> P8034, L11: The mentioned reference Spahni et al. (2011) does not contain a justification or such large (8%) forward modelling errors. If modelling errors were really that large, I tend to conclude that there is no use for more accurate measurements. Indeed, the tuning procedure seems to indicate that these errors are far too large.

As explained in Section 2.1, this large value accounts for the large correlations between observation errors within a diagonal error matrix set-up. It should not be interpreted as a real variance. We will state this in Section 3.3 as well in the revised version.

> P8034, L23-25: Is it possible that CO_2 columns derived from GOSAT are better suited for scaling GOSAT than SCIAMACHY CH_4/CO_2 ratios? Wouldn't it be more appropriate to use an independent CO_2 estimate as light path proxy?

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Chevallier et al. (2011) did not use GOSAT to constrain their CO2 fluxes. They used either TCCON or surface air-sample measurements and we use the air-sample version. We will make this explicit.

P8035, L14: Please motivate the 3% CTM error.

IASI allows retrieving the partial column of methane in the mid-to-upper troposphere where the variability of methane is lower than for the total column. Therefore we expect the CTM error to be smaller than for the GOSAT and SCIAMACHY retrievals. Within this relative constraint, our prior guess of 3% is subjective and is therefore tuned afterwards.

P8036, L7: Add that this increase is compared to prior fluxes (and omit that in L9).

This will be done.

P8036, L9-10: It is indeed expected that chemical losses are constrained by MCF observations. But this turns out not to be true. There are large variations in chemical loss between the different inversions (which all have the same MCF observations included). Thus, the authors should remove the statement that CH4 losses are mostly constrained by MCF.

We will do that.

In addition, a satisfactory explanation of why CH4 loss varies so much between the inversions is then also needed.

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Within our multi-species inversion system, the loss of methane is constrained both by MCF observations and by OSSE-dependent CH4 observations. We will make this clear in the revised version.

> P8036, L19: Be consistent: the number 577 differs from 576 in Table 2.

This will be corrected.

> P8036, L21-23: No, the emissions are not consistent. $578 + -26$ Tg/yr (SC_1^1) is not statistically consistent with $531 + -20$ Tg/yr (IA_1^1). Correct this statement.

The posterior emissions fit within 2 sigmas (96% of the pdf). Moreover, as written above, a more restrictive data filtering for IASI now makes the posterior methane emissions for the initial configurations fit within 1 sigma for all the observing systems.

> P8037: This page is hardly readable with all these numbers. I suggest to make a table with the posterior fluxes and uncertainty reductions per region, and to demonstrate relationships between inter-inversion consistency and uncertainty reduction with reference to that table rather than introducing so many numbers in the text.

We will do as the reviewer suggests.

> P8037, L23-25: Why is there a lack of IASI data during the monsoon period? If it is due to clouds, then why doesn't the same hold for SCIAMACHY and TANSO-FTS?

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Indeed our argument was not appropriate since SCIAMACHY and TANSO-FTS retrievals also require clear sky. The misattribution is more likely caused by the combination of the larger footprint of the free tropospheric column with the lack of retrievals in the monsoon region.

P8038, L13: 'Mean bias' sounds like a duplication, because 'bias' is already a mean. So please clarify that this is the mean of the biases for individual stations.

We will do that.

P8038, L17: Table 2 gives 27.0, the text 26.9 ppb mean bias for SC_1^1.

We will correct this.

P8038, L18-19: Please add these RMS numbers to the table. It would be even better to mention the standard deviation, since this is separated from the bias (unlike the RMS). And actually, a much better performance metric would be the RMS (or standard deviation) of the bias. While the mean bias over all stations can be small by luck and with large compensating errors, this is not the case for the standard deviation of the bias.

Further to this comment, we have studied the RMS and find that the impact is smaller than for the biases, probably because of bias/random error compensation as the reviewer suggests. But the conclusions remain unchanged. We will add the RMS of the bias in the table, as suggested.

P8039, L13-15: See earlier remark on whether CH4 losses are constrained by MCF.

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See above.

> P8040, L15-18: The TANSO-FTS inversion growth rate is only marginally closer to that of the surface observation based inversion after bias correction. This rather seems a coincidence than a firm result of the bias correction.

We will suppress the statement.

> The reasons given for the fact that the SCIAMACHY inversion growth rate gets further away from that of the surface observation based inversion after bias correction seem to be speculation. Unless proof is given, these reasons should be removed.

As we wrote above in the preamble, new results obtained using SCIAMACHY data and after bias correction do not further diverge from the surface results any more.

> P8040, L23-24: Is 30.4 ppb comparable to 23.5 ppb? Still 30% difference.

The values will be updated.

> P8041, L5: The ratio is here again defined diag/var, whereas Table 3 gives var/diag.

This will be corrected. We apologize for the mistake.

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P8041, L9-10: The numbers 0.97 and 1.05 differ from the numbers 0.74 and 1, respectively, given in Table 3.

The values will be corrected.

P8041, L10 and further: This is problematic, since the background error ratios actually get worse for the inversions with alpha=0.6 for both SCIAMACHY and TANSO. This is attributed to the fact that only one iteration is made in the tuning process. This seems unlikely: the ratios simply go in the direction one would expect (i.e. they become smaller), and since they were already quite good for the initial SCIA and TANSO inversion, they deteriorate with alpha=0.6. Furthermore, it seems that the argument of having only one iteration is made selectively for those cases that do not satisfy the expectations of the authors.

This results will be presented as sensitivity tests that yield mixed results.

P8041, L13-16: For IASI the analysis error ratio actually gets worse (1/0.28 > 2.47).

This does not happen anymore with the tighter quality control and we will update the numbers.

P8041, L25: The number 9.23 should be 8.9 according to Table 3.

This will be corrected.

P8041, L26: The number 4.62 should be 5.53 according to Table 3.

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The values will be corrected.

P8042, L2-4: What motivates the choice gamma=0.075 for TA, while 1/8.9=0.11.

In the revised version we will use the exact value.

Similarly, what motivates the choice gamma=0.175 for SC, while 1/4.53=0.22.

Same.

Also, this experiment is not included in Table 3.

We will include it.

And what motivates the choice gamma=0.33 for IASI, while 1/4.01=0.25?

Same as above.

P8042, L4: The number 0.27 should be 1.93 according to Table 3.

The values will be corrected.

P8042, L5: The number 8.9 should be 9.23 according to Table 3.

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The values will be corrected.

P8042, L6: The number 1.07 should be 0.71 according to Table 3.

The values will be corrected.

P8042, L6: The number 1.55 is missing in Table 3.

The values will be corrected.

P8042, L9-13: How does one tune the observation error variance using the analysis error variance ratio? How does this lead to the choice of gamma=0.125 for TA? Is this a trial and error process? Please explain. And why does this alternative tuning process not work for SCIAMACHY and IASI?

We apply the ratio obtained for the analysis variances to the observation variances, by assuming that the former drive the latter in the case of satellite data. We will explain this and will also add the result of this tuning process for SCIAMACHY and IASI.

P8042, L10: Should Eq. (4) be Eq. (8)?

This will be corrected.

P8042, L13: Where does the scenario $SC_{0.2}^{0.6}$ come frome?

This configuration has been erroneously written and refers to $SC_{0.25}^{0.6}$

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P8042, L20-23: The posterior global annual emission for $TA^{0.6}_{0.125}$ is 568 (not 567) Tg according to Table 2. The global annual loss is 535 (not 545) Tg. Consequently, the mentioned growth rate is also wrong. The scenario $SC^{0.6}_{0.2}$ is missing in Table 2.

The values will be updated and the scenario $SC^{0.6}_{0.2}$ will be added.

P8042, L23-23: Is a growth rate of 27 Tg consistent with 19 Tg? That depends on the posterior error, which is not given.

We will add the error bar, which is much larger than this difference.

P8043, L3: It's rather a 3-fold overestimation.

This will be corrected.

P8043, L17-20: I cannot verify this conclusion, since the final SCIAMACHY configuration is missing in the figure.

As written above, the choice of final configurations will be removed.

P8044, Section 4.4.3: As before, virtually all numbers mentioned in this section are either inconsistent with Tables 2 and 3 or cannot be verified since the respective scenario or statistic (rms) is not included in the table. Moreover, the conclusion that with statistical consistency of the inversion the fit to surface measurements is improved, does not hold for TANSO-FTS.

We agree. This will be clarified.

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P8044, Section 4.5.1: Again wrong numbers. Why is TASU not included in Tables 2 and 3?

The results of the combination of TANSO-FTS and the surface sites do not add much to the paper and will be removed.

P8046, L8: Where does the number 526 Tg come from?

The number came from an older inversion. This will be corrected.

P8046, L10: On page 8039 the SCIAMACHY fit was $13.7 * A_f - 26.6$ ppb.

This will be corrected

P8047, L6-8: Where does 4.8 ppb come from? What is the meaning of ?4.8 ppb of standard deviation? anyway? The conclusion on the improved fit to surface observations is wrong (see earlier remark).

4.8 ppb is the mean bias (see p. 8044, l.8) and was erroneously called standard deviation. We will correct this.

The conclusion on the improved fit to surface observations is wrong (see earlier remark).

We will modulate our conclusion, based on this remark (see above).

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P8047, L20: I don't see any data of Bouwman et al. (1993) mentioned in the paper. What is compared here?

The sentence will be removed

P8048, L6-9: No statistics for the NH stations have been given. In fact, for the stations at latitudes below 50 degrees the RMS of the combined inversion was actually worse than the RMS of the TANSO-only inversion. The conclusion is thus not justified.

The combination of TANSO-FTS and the surface sites will be removed (see above).

P8048, L21-22: Actually, without tuning the inversions with different observing systems are closer together. The tuning deteriorates the consistency between the inversions. This suggests that the posterior errors given by the tuned inversions are too optimistic.

We agree that the benefit of the tuning is not systematic. We will clarify this.

P8049, 3-5: Why is SCIAMACHY not mentioned here?

We could not find a satisfying configuration for SCIAMACHY.

P8049, 5-9: The final TANSO- and IASI-based inversions are statistically consistent with the surface observation based inversion, but not with each other. This may lead to problems if they are combined.

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This remark led us to use a more restrictive data filtering for IASI observations (see above). It improves the agreement between GOSAT and IASI. Now, GOSAT, IASI and the surface data are more consistent with each other.

Table 1: Kaplan (2002) reports present-day global annual wetland emissions of 140 Tg, whereas Table 1 reports 177 Tg. Please explain.

We have taken the scaled value from the NitroEurope project (Peter Bergamaschi, personal communication, 2009), that arguably better represents wetland emissions. We will insert this information.

Table 2: Explain in the caption what C_{AF} means. Add a column with the standard deviation of the bias with surface stations. Some scenarios mentioned in the text are not included in the table. Please include. Add the bias and RMS for the prior simulation and SU inversions.

We will do this.

Table 3: It's more logical to put the analysis error variance ratio in the last column. Include missing scenarios and missing numbers. Use same number of digits for all numbers in a column. E.g., write 1.00 instead of 1.

We will do this.

Fig. 2: Explain what is plotted here. Column-averaged mixing ratios? Averaged over a month? The caption suggests that monthly averages have been used in the inversions, but this is certainly not the case. Refine the color scale to show more detail.

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We plot the satellite "super-observations" for the month of July 2010. We will change the caption and color scale as suggested.

> Fig. 3: The final configuration of SCIAMACHY is missing in panel c.

We will remove the choice of final configurations as written above. Figure 3c will be removed

> The error bars are missing for two inversions in panel d. Use one caption instead of separate captions per panel. The y-axis can be shrinked (say from 450 to 650) to show some more detail.

We will do this.

> Fig. 4: The final SCIAMACHY inversion is missing in panel c. Use one caption instead of separate captions per panel.

As written above, panel c will also be removed.

> Fig. 5: It's very hard to read these extremely small panels.

We will split them into 2 or 3 sub-panels.

> Fig. 6: What is 'too large'? And again, the panels are rather small.

The term 'too large' actually refers to a degradation of the bias by the involved inversion. Otherwise, we will do as suggested and the caption will be changed.

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> Fig. 7: The fit line for SCIAMACHY (panel a) does not correspond to the fit results mentioned in the text. Also, please add the fit results in the panels. The density of points is not visible in the current plots. Please make scatter density plots.

We will do as suggested.

> Fig. 7 shows that SCIAMACHY has a negative bias compared to the reference surface observation based inversion. However, the SCIAMACHY-based inversions yield a strong increase in emissions. How can this be reconciled?

By correcting for a negative bias, the SCIAMACHY concentrations are increased to be more consistent with the surface measurements. This consistently triggers an increase in methane emissions. However, our new regression (see our general introduction above) does not yield the same issue, but is not more consistent with the surface-based inversion.

> Fig. 7 gives an indication of observation errors, including transport errors. The standard deviation of model-obs differences cannot be accurately inferred from the figure, but it appears to be about 45 and 25 ppb (2.5 and 1.5%) for SCIAMACHY and TANSO-FTS, respectively. This standard deviation gives an upper limit to the observation error (incl. transport), because it also contains a component related to emission errors. Based on this figure it is clear that the assumed 8% observation error is far too large.

See above our comment about the correlations that this number actually accounts for.

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Technical comments

P8025, L24: spans -> is

P8026, L11: Atmosphere -> Atmospheric, ENVIronment -> ENVIronmental

P8027, L18: consists in -> involves

P8028, L29: consists in -> involves

P8029, L3: remove hyphen between inversion and members

P8029, L18: angle of view -> viewing angle

P8030, L15: applied on -> applied to

P8034, L23-26: Swap these two sentences.

P8035, L7: insert global between full and coverage.

P8035, L18: replace but with and.

P8037, L15: The surface . . . the satellite . . . Do you mean The surface observation based inversion . . . the satellite data based in version?

P8039, L24: infers -> triggers?

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P8040, L6L illustrates -> illustrate

P8041, L4: fairly -> fairly well

P8042, L23 and further: Statements like ?TANSO-FTS retrieves? are not correct. A satellite instrument does not retrieve something. Please reformulate. Also statements like 'IASI and the surface are in good agreement' and 'SCIAMACHY overestimates the growth rate' are incorrect.

P8043, L2: I in IA should not be italic.

P8048, L14: time exchange should be exchange time

Fig. 3: Do not use italics to denote the scenarios in the captions.

All technical comments will be followed.

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Interactive Comment