Dear reviewer 2,

We are very grateful to you for reviewing the manuscript and for submitting helpful comments and suggestions to improve the text. Here we respond point by point to your comments and questions.
You can find in red the relevant changes in the revised version of the manuscript.

The co-authors

General comments

- **1. In general the discussion would benefit from a clearer analysis and separation of the two major sources of error which were identified in the introduction: observations vs. transport errors. Of course representation error kind of mixes up these two categories, but for the purposes of this study the two have been effectively separated. When I look at Figure 5 it seems that for these large regions it is often the case that the three different observing systems cause a spread as large or larger than what is seen for the same observing system with three different version of physical parameterizations (i.e. the difference between the three reds is as big or bigger as the difference between the red, blue and green for each region). The material is there to clearly describe and define this, but the discussion of this point is lacking. An improvement of this point would benefit the manuscript overall.**

  We agree with this remark of the reviewer. Accordingly, we added Figure 6, which compares the spread in inversions due to the choice of the measurement dataset (blue error bars) and the spread due to the choice of the version of the model for each region (green error bars). In red, we represent the spreads of the 9 inversions. These spreads are plotted as a minimum-maximum range.
  In few regions, the spread between inversions using different version of the model and inversions constrained by different datasets are similar (examples: South America temperate, Africa, Australia and Boreal Eurasia). However, we notice that the spread found in inversions using different datasets are much larger than the spread found in inversions based on different versions of LMDz for several regions, such as South America Tropical, Europe and China.
  Consequently, we discuss these different results in the paper in the Section 5.2.

- **2. What was missing in this study was a discussion of the sinks of methane. I read it quite carefully, and Im not entirely sure if the OH sink was being optimized (let alone the soil sink, or if the Cl sink was even considered).**
  In this study, OH and $O(^1D)$ fields are prescribed (with a very small error bar of 1%). They are coming from a full-chemistry LMDz-INCA simulation of Szopa et al. (2013). The different characteristics of the OH field used here are in the range of the current knowledge on the hydroxyl radical exposed in Naik et al. (2013). Besides, reactions of CH_4 with chlorine are not considered in our system. We precise more clearly these points in the updated version of the text also acknowledging that the focus is more on methane emissions than on methane sinks.

- **3. (...) If it was being optimized, it would be interesting to see how the vertical mixing affected the magnitude and location of the tropospheric methane loss. If it is not being optimized, the differences in vertical mixing likely impact the lifetime simulated under each version of the model, and thus the global fluxes shown in Figure 4. In any case, it needs to be explicitly discussed.**
  As also stated in the answers to reviewer #1 comments, the different versions of LMDz derive different methane lifetime. We studied the impact of the vertical mixing on the methane lifetime in Locatelli et al. (2015). We found that the difference in methane lifetime due to changes in physical parameterizations could reach 0.2 years. It has been shown that the version of LMDz using the thermal plume model (LMDz-NP) simulated a methane lifetime 0.2 years higher that the LMDz-TD and LMDz-SP versions. You can have a look at the Figure 10 of Locatelli et al. (2015), which shows the different CH_4 mixing ratio equilibrium states reached after several years of simulation. These equilibrium states differ up to 25 ppb. Consequently, the different representation of the vertical mixing in LMDz modifies the methane lifetime in the different versions of LMDz and it directly impacts the estimation of methane emissions by inverse modelling. It is one contribution of transport errors leading to the uncertainties in inverse modelling.
  We specify now this point in the Section 2.2, referring more clearly to our previous paper on this important matter.

- **4. Although many numbers are used to describe the differences, the reader is left unsure about how significant an effect this is. Having the mean spread over several years for the surface-based inversions (in Table 2) is a start, but it doesnt show whether the patterns are consistent over these years, or whether the differences are more random in nature. Having only one year analyzed for GOSAT inversions exacerbates this. Although it might be significant extra work, considering the uncertainty on the posterior flux**

estimates would be an appropriate way to address this.
As mentioned by the reviewer (and also by reviewer #1), the computation of posterior uncertainties is very time consuming in such a large variational system. However, here, we can benefit from the study of Cressot et al. (2014), who have run Monte-Carlo simulations to compute posterior uncertainties for inversion configurations that are very close to ours (similar observation data sets, similar prior covariance matrix, similar optimization algorithm, etc.), and with the same transport model LMDz. In the revised version of the manuscript, we have applied the uncertainty reductions found by Cressot et al. (2014) to the results of our study. On Figure 5, we plot now the posterior error bars for BG-TD, EXT-TD and LEI-TD inversions, which correspond to the uncertainty reductions of Table 2 of Cressot et al. (2014).
We propose now in the text a discussion on the significance of uncertainties due to parameterization errors given the posterior uncertainties in each region (see Section 5.2).

<u>Minor concerns</u>

- **1. What is used for the driving meteorology? ERA-interim? I could not find this information easily in the paper. If ECMWF driving meteorology is used, did you consider using the convective mass fluxes that are stored? This is more consistent with the underlying transport of the model, which might solve some of the interhemispheric gradient problems associated with inconsistent schemes used to address sub-gridscale convection.**
  LMDz is a GCM and therefore it computes its own meteorology. In order to be more realistic, as classically done in many models, we nudge the LMDz horizontal components of the wind towards analysed winds from ERA-Interim. We then archive all the air mass fluxes and computes the inversion with an offline version of LMDz. Therefore the consistency is garanteed between meteorology and tracer transport.
  We specify more clearly these points now in the text (section 2.2).

- **2. The reference to GrooB and Russel in the text states that its from 2014, but its actually from 2005. But more importantly, details are missing with respect to how the comparison was carried out. Were the data corrected to account for trends in methane between 1991 (the beginning of the period used to compute the HALOE climatology) and 2010?**

We have corrected the reference to GrooB and Russel.
The HALOE data have not been corrected for trends in methane between 1991 and 2010. Indeed, here we are mostly interested in the CH_4 gradient in the UTLS (Upper Troposphere/Lower Stratosphere) region. Therefore, we consider that correcting CH_4 concentrations is not important as we focus on the large differences in CH_4 mixing ratio in the UTLS between model versions, which are not very sensitive to the mean atmospheric value. For instance, difference between CH_4 mixing ratio simulated by LMDz-39 and LMDz-19 reach 500 ppb at 10 hPa! We clarified the text on this point.

- **3. Furthermore, is the model subsampled in a way consistent with the measurements (in terms of space and season)? HALOE didnt measure much at high latitudes ($\geq$ 50 degrees or so), where stratospheric methane is particularly variable. Was this taken into account? Why not use a more modern sensor such as MIPAS or ACE-FTS in addition (or instead) ?**
  Yes, the model has been sampled at the same location and time that the HALOE data. For the comparison, we only use data located between 60 $^\circ$ N and 60 $^\circ$ S for the whole year 2010. Moreover, we have not used others sensors because we consider that the results shown with the comparison using HALOE data are clear enough to support our point about the improvement from LMDz-19 to LMDz-39 regarding UTLS exchanges.

- **4. I am also slightly confused by what is shown in the "percentage" profiles in Figure 3. Is this the contribution of each of the GOSAT retrieval layers? And if so, for an average of all columns for 2010? Or something else? This needs to be better explained. Although chronologically in the manuscript it might be hard to work in, I was wondering what the different versions of the LMDz-39 looked like on this plot. Perhaps it would be instructive to include a similar comparison, perhaps for zonally-averaged columns in the tropics, NH extra-tropics, and SH extra-tropics. This might work well in a discussion of the photochemical sink, and how that effects the estimated lifetime across model versions (see comment above).**
  Yes, this is the contribution to each retrieval layer to the total column (in percentage), which is shown on the Figure 3. It has been plotted to show that stratospheric concentrations contribute more to the total column in LMDz-19 than in LMDz-39. On the contrary, tropospheric concentrations contribute more to the total column in the LMDz-39 version. This is directly related to the vertical profile shown on the right side of the Figure. We have clarified the text in the updated version.
  The other versions of LMDz with 39 layers (LMDz-SP and LMDz-NP) have a very similar vertical profile to the one shown (LMDz-TD) on this plot. LMDz-TD, LMDz-SP and LMDz-NP may have large differences in the simulation of the vertical profiles at some specific location and time, but in the case of an annual global mean (as is shown

on the Figure 3) the three versions of the model are very similar. It is confirmed by the Figure 2 where we show that the bias between surface measurements and surface optimized concentrations are very similar in the three 39-layer versions of LMDz. So, we have decided to show only one version of 39-layer LMDz here (the LMDz-TD version), which is consistent with the LMDz-19 version because they both use the same physical parameterizations. We precise this agreement for Figure 3 in the updated version of the text.

- **5. To be honest, I am surprised that the transport differences do not result in larger flux discrepancies in Figure 4. How do these differences compare to the posterior uncertainty? Is this something that your system can easily calculate? This question arises again when looking at Figure 5. How significant are the differences between the different implementations of transport? Do they result in posterior flux estimates that do not have overlapping uncertainties? The information to judge this is not provided. A 5% range due to transport differences is significant if the uncertainty is 1%, but not if its 4%. Was the lifetime/OH sink fixed between simulations?**

First of all, please remind that only transport differences due to the different physical parameterizations implemented are considered here. Differences derived in estimated methane emissions due to the modelling of atmospheric transport were much larger in Locatelli et al. (2013), where we use different models (different resolutions, different parameterizations, different analysed winds, etc.). Then, we were not surprised to find a smaller spread in this study than in Locatelli et al. (2013).
In order to quantify the spread in inversions due to differences in parameterizations relatively to posterior uncertainties, we use the uncertainties reductions found in Cressot et al. (2014) as explained before in this review. Thus, on the Figure 5, we give the posterior error bars for BG-TD, EXT-TD and LEI-TD inversions, which correspond to the Table 2 of Cressot et al. (2014).
We also propose a discussion (see the Section 5.2) on the significance of the impact of parameterizations errors on inversions relatively to the posterior uncertainties. As stated before (general comment #2), the OH field was prescribed based on a MCF calibrated field coming from the full chemistry model LMDz-INCA (Szopa et al. (2013)).

Typos/language issues

- **p11854, line 15: total-column what? total-column abundances, or total column methane mixing ratios, etc., something is missing there.**

Ok, we clarified it in "total-column mixing ratios".

- **p11854, line 18: gradient → gradients**
  Ok.
- **p11855, line 1: relatively → relative**
  Ok.
- **p11855, line 12: supplement the issue? Or rather ameliorates the problem? Or they supplement the existing measurement network...**
  We decided to use "solve the issue".
- **p11855, line 14: become → becomes. Also, it was already a major issue, perhaps now it becomes the leading issue?**
  Ok.
- **p11855, line 19: satisfactory → satisfactorily**
  Ok.
- **p11856, line 1: SCHIAMACHY → SCIAMACHY**
  Ok.
- **p11856, line 5: carry on → carry out**
  Ok.
- **p11856, line 5-6: have also → also have**
  Ok.
- **p11859, line 19-20: "by Tiedke (1989) scheme" → "by the scheme from Tiedke (1989)" or "by the Tiedke (1989) scheme", similar with Yamada**
  Ok
- **p11859, line 24: "by Emanuel" → "according to Emanuel" or similar**
  Ok.
- **p11859, line 27: an → a**
  Ok.
- **p11860, line 5: "On the opposite" → "On the other hand"**
  Ok.
- **p11860, line 6: "has been also" → "has also been"**
  Ok.

- **p11860, line 7-10: Rework the sentence a bit. Perhaps: "The interhemispheric (IH) exchange, which is known to be too fast in LMDz-TD, agrees better with the indirectly measured IH exchange when using the Emanuel (1991) scheme, as is done in LMDz-SP and LMDz-NP.**
  Ok, done.

- **p11860, line 11: "which justify to test it as well" → "which justifies its inclusion"**
  Ok.

- **p11861, line 19: "that CO2" → "that the CO2"**
  Ok.

- **p11863, line 22: "that CH4" → "that the CH4"**
  Ok.

- **p11864, line 11: "Consequently, the inverse system derives lower methane fluxes with LMDz-19 to simulate lower tropospheric methane mixing ratio compensating the over-contribution of stratospheric methane mixing ratio to the total-column." → "Consequently, the inverse system derives lower methane fluxes with LMDz-19 to simulate a lower tropospheric methane mixing ratio, compensating the over-contribution of the stratospheric methane mixing ratio to the total-column."**
  Ok.

- **p11864, line 18: "modelling of" → "modelling of the"**
  Ok.

- **p11864, line 19: "reasons of" → "to determine the reason for", "need" -¿ "needs"**
  Ok.

- **p11864, line 25: fluxe → fluxes**
  Ok.

- **p11864, line 28: "we only focus and present results associated to " → "we focus on and present only results associated with "**
  Ok.

- **p11865, line 10: "which was estimated as a total transport model errrors" → "which was an estimate for "total" transport model errors"**
  Ok.

- **p11865, line 13: "although smaller than" → "although a smaller impact than"**
  Ok.

- **p11865, line 23: "on China methane flux estimates" → "on the methane flux estimates for China"**
  Ok.

- **p11866, lines 1 and 4: "simulated total-column" → "the simulated total column"**
  Ok.

- **p11866, line 8: "total-column" → "the total column"**
  Ok.

- **p11866, line 17: "have been" → "has been"**
  Ok.

- **p11867, line 23: "wrong repartition between Northern and Southern Hemisphere of emissions" → "incorrect repartitioning of emissions between the Northern and Southern Hemispheres"**
  Ok.

- **p11868, line 1: southern → Southern**
  Ok.

- **p11868, line 5: extra-tropics → the extra-tropics**
  Ok.

- **p11868, lines 10-11: reach 7.5 unitTg CH4 year**
  Ok.

- **p11868, line 14: impact strongly → strongly impacts**
  Ok.

- **p11868, line 20: than → that**
  Ok.

- **p11868, line 21: impacts → impact**
  Ok.

- **p11869, line 19: LMDz-SP and LMDz-SP → I guess this should be LMDz-SP and LMDz-NP, right? and also "the Emanuel"**
  Ok.

- **p11869, line 21: dependent ON**
  Ok.

- **p11869, line 23: "Then, LMDz-SP and LMDz-NP derives also" → "Thus LMDz-SP and LMDz-NP also derive"**
  Ok.

- **p11871, lines 6-7: "where modelling of boundary layer mixing impact much atmo- spheric methane levels" ¡- Im not entirely sure what is meant here, please reword it. Does boundary layer mixing have a large impact on the concentration of atmospheric methane? Or does boundary layer mixing impact the atmospheric methane concentration across several model levels?**
  Here, I explain that stations added in the EXT configuration are located closer to methane sources. Ok.

- **p11871, line 16: are ranged from → range from**
  Ok.

- **p11871, line 27: deriving → derive**
  Ok.

- **p11873, lines 4-7: "Indeed, inversions using Emanuel (1991) scheme (based on LMDz-SP or LMDz-NP model) have smaller interhemispheric 5 methane emission gradients than inversions using Tiedtke, 1989, scheme (based on LMDz-TD model), which are known to simulate too fast interhemispheric exchange (Patra et al., 2011)." → "Indeed, inversions using the Emanuel**

(1991) scheme (LMDz-SP or LMDz-NP) have smaller interhemispheric methane emission gradients than inversions using Tiedtke (1989) (LMDz-TD), which are known to overestimate interhemispheric exchange (Patra et al., 2011)."
Ok.

- **Figure 3, caption: profils → profiles**
  Ok.
- **Figure 4, plot: Physic → Physics; subscript of 4 in CH4**
  We redo the Figure 4 taking into account your comment.
- **Figure 4, caption: change "Leicester institute", remove comma after "and" (or move it before)**
  Ok.

Dear reviewer 1,

We are very grateful to you for reviewing the manuscript and for submitting helpful comments and suggestions to improve the text. Here we respond point by point to your comments and questions.
You can find in red the relevant changes in the revised version of the manuscript.

The co-authors.

General comments

- **1. The manuscript refers to an unpublished manuscript of Monteil et al. However, in most cases it would be better to refer to a JGR paper that has already been published (Monteil et al, 2013).**
  Yes, we now refer to the paper of Monteil et al. (2013) according to this comment.

- **2. That paper provides a quantification of the bias using the TM5 model, which would be useful to compare with the results obtained in this study using LMDz.**
  Monteil et al. (2013) have quantified the bias between surface measurements and simulated surface concentrations using fluxes coming from GOSAT-only inversions. They have found that "Full-Physic" and "Proxy" GOSAT-only inversions lead to an overestimation of global mean surface CH_4 mixing ratios by respectively 16.9 and 6.9 ppb. However, they give little information on the latitudinal distribution of these biases, even if they seem larger in the southern hemisphere (see their Figure 5). In our work, we quantify the bias using a similar approach (comparison between optimized and measured surface concentrations). After our different inversions, we find a global mean bias of -4, +40, +38 and +41 ppb for LMDz-19, LMDz-TD, LMDz-AR4 and LMDz-KE at the surface. Consequently, the bias found when using the TM5 model (which has 25 vertical levels) in inversions is larger than in LMDz-19 inversions, but lower than in the different inversions using LMDz with 39 levels (LMDz-TD, LMDz-AR4 and LMDz-KE). Some indications may be found in Patra et al. (2011), but they refer to an old version of LMDz (19 vertical levels and old parameterizations). We investigate here the possible cause of this large increase of bias when moving from LMDz-19 to LMDz-39 and point towards the quality of tropospheric-stratospheric exchange both in the model and in the retrieval. In the revised version of the manuscript, we add a

paragraph describing the different results found with TM5 and LMDz inversions.

- **3. In the conclusions, it is mentioned that transport model errors lead to flux errors up to 50% at regional scales, but I do need see that back in any of the presented results.**
  Actually, we wrote in the "Conclusions" that uncertainties in parameterizations lead to flux errors of 5.2, 10.7 and 8.2% for respectively BG, EXT and PR-LEI inversions. Locatelli et al. (2013) used 9 transport models (with different parameterizations, resolution, advection scheme, etc.) to estimate the total error due to transport modeling in inversions. They found that spreads in regional fluxes could range from 23% to 48% of emissions depending on the regions. By taking the ratio between this study (5-10%) and the previous study (23-48%) we estimate that the error due to the vertical parameterizations in one model (LMDz) explains, on average, 24% of the total transport model errors at regional scales, and that they can reach more than 50% in some specific regions. For example, the spread due to total transport model errors in inverted fluxes for South America reaches 48% of the emissions of this region. The spread in South America due to parameterization uncertainties reaches only 9.8% in PR-LEI inversion, which means that parameterizations explain 20.4% of total transport model errors.
  We have clarified this point in the updated text.

- **4. Looking at Figure 5, I wonder how significant the differences are, given the posterior flux uncertainties and the change from the prior. The figure shows a horizontal bar, which is not explained in the caption, but may actually be the prior. It is not only relevant to assess the uncertainty in the regional flux, but also the robustness of deviations of the inversion-derived fluxes from the prior (and their significance given the uncertainties).**
  We have improved Figure 5 by adding prior estimations and prior errors bars for the different regions. It gives a direct indication about the deviations of our different inversions from the prior. Moreover, in a grid-point-scale variational system, like the system we used here, the computation of posterior uncertainties is highly time consuming. In the revised version, we report the posterior uncertainties provided by Cressot et al. (2014) for an inversion configuration, which is very close to ours (similar observation data sets, similar prior covariance matrix, similar optimization algorithm, LMDz model, etc.). In the updated text and according to reviewer's comment, 1/ we give the posterior error bars for BG-TD, EXT-TD and PR-LEI-TD inversions on the Figure 5, 2/ we propose a discussion on the significance of uncertainties due to parameterization errors given the posterior uncertainties in each region.

- **5. Further information is needed about the treatment of the initial concen-**

tration and the atmospheric oxidation in the inversion. Are they optimized? If not, could an inconsistency between the initial concentration field at the start of the short-window satellite inversion compared to the longer window surface inversion explain differences in the derived global total? How about the global sink?

In our system, we optimize the weekly grid-point emission flux of CH_4 together with the initial conditions of CH_4 mixing ratio (in the form of 2-D scaling factor on the CH_4 columns). The bias between measured and optimized surface concentrations is explained both by a modification of the initial conditions and by a modification of the methane emissions. Here, we found a bias of 40 ppb at the surface after the satellite inversions (see Figure 2). After analysing the scaling of the optimized initial condition, we found that around 15-20 ppb of the 40 ppb are explained by an increase of the initial condition, the rest being explained by an increase in the CH_4 emissions.

OH and $O(1D)$ fields are prescribed (optimized in the state vector but with a small error bar of 1%). These fields come from a full-chemistry simulation of LMDz-INCA (Szopa et al. (2013)) and after a global scaling by methyl-chloroform observations made before inversions. The different characteristics of the OH field (global mean concentration of 11.5 x 10^5 molec.cm^{-3} between surface and 100 hPa) are in the range of the current knowledge on the radical hydroxil (see the ACCMIP experiment ; Naik et al. (2013), i.e. between 7.4 and 13.3 molec.cm^{-3}). No inter-annual variability is applied to the OH field used here. We added more details on this in the updated text.

- **6. Even if the oxidant fields are the same, the lifetime may be different due to differences in transport.**

  We agree that the lifetime may be different due to differences in the modelling of atmospheric transport. We quantify it in Locatelli et al. (2015) (see their Figure 10). They found that differences in CH_4 lifetime simulated by the 3 versions of LMDz could reach 0.2 years. Indeed, they found a difference of 25 ppb in the state equilibrium of CH_4 mixing ratio simulated by LMDz-TD and LMDz-NP, after two 39-years simulations.

  We now give more information in the text about the treatment of the initial concentration and the atmospheric oxidation in the inversion.

- **7. From the results it is clear that some representations of transport are more realistic than others. It would be interesting to know of this translates into optimized models that are more or less realistic. The comparison with HALOE in figure 3 is clear, but it is unclear whether improved performance can also be demonstrated in the troposphere which might relate more directly to the accuracy of the inversion-derived fluxes.**

  Locatelli et al. (2015) investigated the skills of the different versions of LMDz in the troposphere and discussed the implications for inverse modelling. In particular, LMDz-

NP improves the representation of PBL dynamics through the use of the thermal plume model. Large-scale atmospheric processes are better represented in versions using the deep convection scheme of Emanuel (1991). Then, we can expect more realistic inverted fluxes when using LMDz-NP and LMDz-AR4. We refer more clearly to this former paper in the revised version of the manuscript.

Specific comments

- **Page 11862, line 12: "Monteil et al, 2013"**
  Yes, we have included this reference.

- **Page 11864, line 5: Has the HALOE dataset been corrected for the CH_4 increase since those measurements were made?**
  No, the HALOE dataset has not been corrected for the CH_4 increase. Indeed, we are only interested here on the CH_4 gradient in the UTLS (Upper Troposphere/Lower Stratosphere) region, which is less sensitive to atmospheric increase than the absolute mean value.

- **Page 11864, last paragraph: I do not understand the second step of the inversion. In the second step the bias is quantified at each surface side, but how is that use in the second inversion step?**
  After a first inversion using GOSAT data, we compute the difference at each surface station between the CH_4 surface measurements and the simulated CH_4 mixing ratios based on the optimized flux coming from the first inversion. It quantifies the consistency between surface and satellite inversions. In our case, we found a positive latitudinal bias (about +40 ppb) between simulated CH_4 mixing ratios sampled at the surface and CH_4 surface measurements. It means that surface and satellite data are inconsistent.
  We consider that surface measurements are unbiased and we correct the satellite data accordingly using a latitudinal correction before performing a second inversion. In the paper, we suggest that most of this bias may come from the satellite data as LMDz-39 clearly improves the troposphere-stratosphere exchange compared to LMDz-19, which had only 19 vertical levels and only a 4 ppb bias with surface observations.

- **Page 11865 first paragraph: How is the global sink treated in the inversion?**
  We use a prescribed OH field coming from a full-chemistry LMDz-INCA simulation (Szopa et al. (2013)). We detailed it in the "general comments" section.

- **Page 11866, line 6: This could be, but it depends on where the surface measurements are made (it would not be the case e.g. for SPO).**
  Yes, we clarified this point in the text.

- **Page 11867, line 19: How do you define IH gradient here? Should not it rather be called "hemispheric difference"**
  Yes, it is the difference between CH_4 concentrations from the Northern and the Southern hemisphere. We change IH gradient into hemispheric difference in the text.

- **Page 11868, line 20: This conclusion is very sensitive to the relative weights of different measurement datasets in the inversion. If the weight of GOSAT is less than that of the surface network, that may also explain why the transport parameterizations have less impact on the fluxes.**
  We have based our inversion set-up on the work of Cressot et al. (2014), who have largely studied and optimized the error statistics of surface and satellite inversions. Consequently, we think that the GOSAT satellite data and the surface measurements are quite properly weighted in our system with respect to their own uncertainties.

- **Figure 3: Do the model contributions to the total column account for the averaging kernel of the satellite retrievals? This should be made clear.**
  We computed model contributions to the total column without accounting for the averaging kernel of the satellite retrievals. We specify it now in the legend of the Figure 3.

<u>Technical corrections</u>

- **Page 11856, line 1: "SCIAMACHY" i.o. "SCHIAMACHY"**
  Done.

- **Page 11857, line 4: "surface" i.o. "surrface"**
  Done.

- **Page 11859, line 28: "presented" i.o. "presenteed"**
  Done.

- **Page 11864, line 25: "methane flux" i.o. "methane fluxe"**
  Done.

- **Page 11868, line 20: ”that” i.o. ”than”**
  Done.

- **Page 11871, line 20: ”span” i.o. ”explore”**
  Done.

- **Table 2, caption: ”shown” i.o. ”showed”**
  Done.

- **Figure 4, caption: ”institute” i.o. ”institude”**
  Done.