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Atmospheric Measurement Techniques Discussions

Interactive comment on "Global distributions of CO_2 volume mixing ratio in the middle and upper atmosphere from MIPAS high resolution spectra" *by* Á. A. Jurado-Navarro et al.

Anonymous Referee #1

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Summary:

This paper describes a new attempt to retrieve CO2 VMR in the mesosphere-thermosphere region from the high resolution spectra in 4.3um measured by the MIPAS instrument. It appears this publication is actually part of previously published paper (Jurado-Navarro et al., 2015) although it is not clear from the text. In the latter publication several V-V and V-T rates relevant to the 4.3um non-LTE collisional scheme are treated as unknown along with the CO2 vmr. The iteratively linearized retrieval approach is described very briefly and insufficiently in section 3. In addition, the LOS is treated as unknown and retrieved from the MIPAS scans, although it is not defined what the LOS retrieval means here. The error analysis is also treated in section 5,

although also in a vague manner given the strong regularization that had to be apparently applied to this problem. Finally, the first MIPAS retrieved CO2 VMR latitude altitudes maps are presented.

The attention to these kind of publications, which finalize extensive studies and present its final product is particularly high. This attention is enhanced also by the product itself, which this paper presents, namely the mesospheric and thermospheric CO2, which is the product of particular interest for the broad scientific community.

I read this very short manuscript with all attention it deserved and am actually very disappointed with it. The information presented in the manuscript (and also the way how it is presented) does not support the paper conclusions. The paper looks as supplement to much larger and detailed previous paper by Jurado-Navarro et al, 2015, although in reality it is completely decoupled from it. The manuscript looks like a formal short progress report to a funding agency rather than the scientific publication. I will demonstrate it below in this review.

The paper is submitted for open discussion into journal whose main mission is defined as methodology of remote sensing. Therefore, I will give first of all the analysis of methodological aspect of the study presented.

More than 20 years ago when this study for MIPAS has begun we knew significantly less about mesosphere, ionosphere and thermosphere than now. MIPAS was a new generation instrument which was supposed to significantly extend this knowledge delivering high duality new observations. May the authors explain at least now, after more than twenty years passed, why the choice of methodology for the analysis of new quality measurement was made in favor of standard approach – straightforward inversion - for solving this extremely ill posed and ill conditioned problem (saying nothing about additional trouble - strong non-LTE), which required an a priori information and various regularizations to be involved.

The authors meanwhile indirectly give explanation of this choice - they wanted to obtain

physically understandable results. From my point of view this motivation is just the best way to science stagnation. May be I am wrong in this particular case, but let the authors try to convince me and other readers in a their true choice.

The paper can be published in ATM, however, only after major revision, in which all comments from below are addressed comprehensively. Since the AMT mission is to promote advances in the methodology of remote sensing the new version should describe in detail the current status of methodology applied to the MIPAS measurements instead of referring reader to half a dozed of previous methodological studies of this group, which made reading and understanding of this short paper really difficult ...

General comments:

1) The author state that "The retrieved CO2 has a much better accuracy than previous limb emission measurements, because of the highly accurate rate coefficients recently derived from MIPAS..." Jurado-Navarro et al, 2015 described with much more methodological details how these rate coefficients were retrieved. This paper states that these rates were retrieved together with the CO2, which was particularly important to provide their "high accuracy", for selected set of MIPAS observations during four days in 2010 covering solstice and equinox conditions in both hemispheres. Jurado-Navarro et al., 2015 shows also excellent signal fitting for this selected situations.

Can the manuscript author explain why the (Jurado-Navarro et al., 2015) methodology of retrieval was not applied in the presented study, but instead the the rate coefficient were fixed and applied for massive retrieval of CO2 alone from several year of MIPAS observations. I understand, that for these particular four days retrieved CO2 should coincide with that of Jurado-Navarro et al., 2015, providing same excellent signal fitting. But how about that for the rest of CO2 retrieved? The typical level of radiance misfit is not presented, neither is the number of iterations, stopping criteria applied, seasonal and/or latitude dependence, correlation among the variables is not discussed. The authors completely avoid discussion of these important methodological issues.

I will address this issue again below.

2) A priori info used for these retrievals is the daytime CO2 from the SD-WACCM model. How far the trieved CO2 is different compared to that of WACCM used as a priory information? The paper does not show and does not discuss this, stating only that "The retrieved CO2 shows the major features expected and predicted by general circulation models" and list this major features.

How much the obtained CO2 results depend on this a priory info? The authors write that WACCM CO2 for lower value of the Prandtl number 2 was used as a priory information for these retrievals and that this choice was made because it "... gives an overall better agreement with ACE CO and CO2 and MIPAS CO (Garcia et al., 2014)." However, what happens if CO2 for Prandtl number 4 (lower eddy) will be used? Jurado-Navarro et al., 2015 stated that retrieved CO2 and rate coefficient for 4 selected days did not depend strongly of this change of the a priory information. But how about the rest of retrieved CO2 for a few years of MIPAS observation.

3) The authors state they were aimed "to obtain stable calculations with a precision high enough to allow for meaningful physical interpretation of the retrieved CO2 abundance". Did they ever observed retrieval results which was difficult to explain? I suppose not, since with their retrieval methodology they were aimed at avoiding to even see these "non-reasonable" results, but I may be wrong. However, did they ever see some measured signals which were out of fitting in the coarse of their very constrained retrievals?

For instance, in the recent paper by Rezac et al, 2015 an interesting observational phenomenon is described: significantly increased daytime 4.3 um emission measured by SABER instrument in polar summer at tangent height around 90 km which is not predicted by the current non-LTE models. MIPAS and SABER are quite well overlapped in time of operation and globe coverage. Did the authors of paper ever observed similar mismatch between observed and simulated signals for polar summer? If not, can they

guess why?

If this effect, nevertheless, exists in MIPAS measurements, then how was it treated, what additional constraints were applied, etc? Does the set of retrieved rates, which are declared to provide extraordinary high accuracy of CO2 retrievals solves this problem? Given that MIPAS provide measurements of these emission with the advantage of spectral resolution this topic should be addressed to promote exposing new mechanism of the 4.3 um non-LTE emission generation. This is important for current and potentially future missions!

Meanwhile, my understanding is the SABER team was able to identify this phenomenon exceptionally because it applied completely different or, better to say, just opposite retrieval approach, namely the forward fitting of simulated radiation to observations without any a priory information and initially build-in constrains. Only after they rigorously defined and described this phenomenon, they imposed some simple constrains on the retrieval process exceptionally for these particular situations to get "physically reasonable CO2". This, however, resulted in a significant mismatch between simulated and measured radiances, which was stated by the authors of the SABER paper.

What is actually the degree of mismatch between measured and simulated signal for MIPAS retrieval presented in the paper? The authors avoiding even mentioning this to say nothing about the analysis of this extremely important issue. Missing this analysis gives the reader no chance to at lest guess of how good the retrieved CO2 (with all constrains imposed) is from the point of view of match between measurement and simulation at the final retrieval iteration. Or in another words - how much the retrieved CO2 is based on the measured signals (assuming the radiative transfer and non-LTE model are perfect) or it represents result of various constrains and chosen a priory information?

4) MIPAS covers broad spectral region where signatures of many atmospheric trace

gases are detected simultaneously. This renders unique opportunity of retrieving a number of atmospheric variables like pressure/temperature and trace gas densities simultaneously and self-consistently.

It is well known that IR emissions of MLT in a number of spectral intervals are significantly coupled, for instance CO2 emissions around 15 and 4.3 micron what was discussed in detail by Rezac et al., 2015. The latter strong coupling motivated these authors to work on and finally present results of simultaneous fitting both signals measured by SABER. This resulted in retrieving CO2 and p/T self- consistently. The authors of manuscript, however, avoid even mentioning this coupling. They present results of CO2 retrievals which utilize their earlier independent p/T retrieval based on the model CO2. From the point of view of simultaneous fitting of both spectral intervals this is actually the result of the first iteration of the p/T and CO2 retrieval. The natural question is: why the authors stop at this first iteration? What has prevented them to run simultaneous retrievals? My understanding is that algorithms of this group used for separate p/T and CO2 retrievals are identical. My guess is that in reality the current retrieval methodology is not able to provide simultaneous p/T and CO2 retrieval. I may be wrong, but let the authors provide the discussion of this important issue in the manuscript.

From this point of view the authors have generally two options: (1) to demonstrate the ability of their retrieval technique to simultaneous CO2 and p/T retrievals, or (2) should discuss in detail why current methodology does not afford straightforward application to two spectral regions simultaneously, or at least channel-to-channel iterations. The reply like "we will dedicate our next paper to this issue" will for me indicate the desire to increase the number of publication of this very productive group. But I will prefer to get the explanation of reasons for publishing these "first iteration" results now.

I foreseen that the reply to this comment may also be like "we discuss in detail the effect of the p/T uncertainty on the CO2 retrieval". I would like to prevent this reply already here: the authors of this manuscript are well qualified to understand that the

study of p/T uncertainty effect can not replace testing simultaneous retrieval from two spectral regions where signal are strongly coupled by hydrostatics.

5) The concept of micro-windows, as it is described in the paper is aimed at finding those spectral regions which provide contribution functions along LOS maximized at tangent altitude or nearby. This idea may be right for temperature retrieval from the 15 um emission. However, its mechanistic transfer to the CO2 retrievals, particularly from the daytime 4.3 um emissions seems quite wrong. In case of 15 um emission the deviation from LTE is moderate in MLT. In this case, if CO2 is supposed as known, the volume emission rate (VER) around the tangent point can be (at least partly) associated directly with the local temperature. This is true because inelastic collisions are the main source if this emission. The non-thermal part of VER is quantified in the iteration process when the non-LTE problem is solved at each iteration step for the undated atmosphere. These is what this research team did and described in their paper about temperature retrievals.

However, in case of the 4.3 um daytime emission the situation is dramatically different. The departure from LTE is very strong even for fundamental bands to say nothing about hot ones. In this situation VER at tangent point gives minimum information about local conditions, particularly about CO2, even if temperature is known. Additionally, the non-LTE is broadening the weighting functions, which is obviously seen in Figures 6(a,b), providing significant contributions even from below the tangent points.

Same idea looks also wrong from another point of view. MIPAS was a high spectral resolution instrument. Sure this instrument provided its users a number of significant advantages, however at the expense of one important quality, namely the noise. The SNR of non-cooled spectral instruments is in general much lower than that of broad-band radiometers. However, the idea to select from measured high resolution spectra micro-windows, which are occupied exceptionally by weak lines with minimal self-absorption, makes the situation only worse, since it is equivalent to selecting signals with the lowest SNR.

Final comment regarding the micro-windows. It is well known, see for instance Rezac et al, 2015, that overlapping of spectral lines influences simulated limb 4.3 um emission up to about tangent height of 90 km. Did overlapping was taken into account by selected micro-windows? The manuscript does not provide any information about this. The Figure 1 obviously shows spectral contributions of single lines. I have checked the HITRAN for the fundamental line (black curve) at around 2317.2 cm-1 which is presented in all panels of this figure, and found that it is surrounded by a half a dozed of other lines of hot bands of main isotope alone. However I do not see any contribution from these lines for any tangent height shown. Is this contribution negligible or is just simply ignored? Same question is also true for other lines shown in Figure 1.

6) The input atmosphere O, O1D, Tkin profile (MIPAS retrieval + NO estimated) are assembled from different sources, in rather seriously inconsistent way and are not substantiated to be accurate/reliable. This includes the Tk profiles. The O, O1D retrieval is touched upon but it is not demonstrated to actually be accurate (neither by comparison, nor through published literature). The way these profiles are sampled in time/space and altitude, what is the joining function and at which altitudes? How does the fact that profiles from these sources are inconsistent to each other affects the estimated error budget? It is unacceptable that in such a technical retrieval paper these points are simply skipped.

7) The title of the paper (and its conclusion) has to change to reflect that only daytime CO2 VMR is obtained. In this respect, I find a discussion is missing on the solar zenith angle dependence of the inversion accuracy (given that the selected microwindows hence S/N should show some dependence).

8) The regularization of this strongly non-linear problem is the most important aspect of the retrieval algorithm. I find the complete lack of detailed discussion of it little suspicious [The actual cost function, update step and any a-priori matrices should be explicitly given in precise and quantitative form when applicable]. The authors should provide the description and quantitative values where applicable for the entire approach so that

the results are reproducible, and second, that the results can be trusted. For instance, a linearization requires rather strong prior assumption to be met it to work. How close do you have to be from the solution for this method to still work, are the measurement and prior uncertainties Gaussian, what is the role of non-linearity on this approach, issues of stability and multiple solutions existence (how unique is the solution?). These points have to be explicitly treated in the text either way. I also find the regularization L curve missing, one of the most important aspect in the selected approach. The shape of matrices has to be discussed (is the H matrix diagonal, first or second derivative? Is there another term if a priory covariance?, etc). For such non-linear forward and inverse problem it is not true and not enough that iterative linearize approximation gets accurate results. This might be argued only for weakly linear problems. Please provide more details and justification that the presented selection of micro-windows avoid the strong non-linearity- it is far from obvious from the given presentation.

9) Error analysis is treated without providing source of the assumed uncertainty on O, O1D, and even T/P are in question as to why they are so small or if they extend down to ground (it is after all non-local problem). The inversion algorithm itself is not demonstrated to be robust enough to provide the accurate results as claimed (I wonder about the numerical derivatives of inverted profiles as well). I also noticed that only very smooth CO2 vmr profiles are tested with the starting condition very close to the true one (claiming that we know CO2 below 75 km that accurately. At the same point why the retrieval claims lower point at 70km is not clear if the inversion is left no freedom to fit?). The vertical profiles of CO2 (at least few samples) should be provided. As I have already mentioned above effects of inconsistent input profiles on the retrieval are not presented (CO2/T)... it is clear more than 20 years (Zaragoza et al., 2000, Mertens et al., 2003, and Rezac et al., 2015) that CO2/T in the MLT region has to be treated self-consistently, which not done in the current paper and it is not discussed. It is also not clear at all if the T/P errors are assumed only on the given retrieval grid or if this error is treated down to pressure z=0km (ground). I can hardly believe that the hydrostatic effect at 140 km of pressure uncertainty at z=0 is so small, I would like these points

to be considered as they are needed to interpret the error analysis. Is the role of ions and electrons on pumping CO2 above 110-120km taken into account? If not what are the uncertainty, can it be ignored, or roughly estimated? Going to such high altitudes and claiming robust accuracy of retrieval one should account for the hot O and O1D at these heights. This should be estimated and reflected in the error budget.

10) What about assumption of rotational LTE, is it justified under these conditions?

11) The dataset used in this work is indicated, but I could not locate it at the MIPAS data website (the mode v5r_CO2_622). Can the dataset be specified in more detailed or described so that it can be found for future reference?

Detailed points:

Title has to change as mentioned before, to reflect that these are only daytime measurements

Abstract:

line 1: the altitude range seems to start at 75km from the figures (3) as well as visible in the averaging kernels. Furthermore, there is no kernel that peaks at 142km. The results should be quoted over altitude range where they have reasonable accuracy and resolution, not the lower/upper grid point of the retrieval. The height of the input atmosphere should be also given in the description of the problem (perhaps in the introduction) and effect if any on neglecting atmosphere above 140 km.

line 3: This is not factually true, the SABER CO2 is retrieved above 120km, although relying on WACCM Tkin profile above 110km. Second, the error budget is overly optimistic in this paper as will be detailed later. This should be later reflected in the abstract and conclusions.

line 4: data set version should be provided a web link (even if it is not publicly available) or detailed enough so it can be found in the database later. I could not find this dataset anywhere.

line 5: The retrieval has been performed jointly with LOS: I do not understand this sentence as it stands. Please clarify (I will make this plea several time later)

Introduction:

line 30: This point is not supported/demonstrated in this paper. How was this conclusion reached? Supporting figure? What is the reference measurement at altitudes > 120 km for CO2 ?

Section 2:

line 14: Would be informative to know which bands/lines are meant here to be optically thin in the 4.3 um above 102 km and/or at which altitude this is really the case?

line 16: The SZA cutoff for daytime should be provided. Also if the measurements cover SZA > 80 the twilight effects on CO2 non-LTE populations should be mentioned whether they are treated or not.

line 19: How is a vertical resolution of 5-7km achieved from 120km to 102km (stated in the abstract) if the measured radiance sampling is 5km down to 102 km and only 3km below that. Also, how is vertical resolution estimated is not clear.

Section 3:

p3, line 24: What is meant here by "LOS altitude information"? Is it that the altitude grid itself is treated as unknown, or is it that only the tangent altitude itself is treated as unknown? This should be formulated clearly and precisely. What is the exact rationale for this would be very helpful have described. If the vertical grid spacing is fixed at specified steps, what are those? What role has this choice on the retrieval or forward modeling?

p4, line 2: This is a poor description of the way the a priori profile is provided into the retrieval. Is the a priori profile is varied with time? What is the sampling space/time? Is it daytime/nighttime or average CO2 profile from the WACCM? Is it collocated to

exact MIPAS measurement or some kind of zonal mean? Is additional scaling applied to fit the increasing trend of CO2 below 60km? Please address these points as to understand what is actually done.

p4, line 6: The retrieval is regularized by Tikhonov type regularization is just not informative enough to asses the quality of the results or support the latter claims on the error budget of the profiles. As stated before, provide precise description of the cost function, iteration step update formula, and are necessary prior information/matrices/regularization parameters and their numerical values. I would like to see a robust demonstration of the strength parameter applied in this problem (L-curve) and what kind of Tikhonov matrix was used (diagonal, 1st, 2nd derivative or other forms) and the justification of thereof. The non-LTE problem in 4.3um emission is strongly non-linear and one has to be convinced that not just a solution is obtained, but that the solution is accurate as claimed. I would recommend a well done self-consistency study: dependence on starting condition, a-priori, at least for few profiles that are not just typical but go 2-3 sigma away from the model. The point where the algorithm becomes unstable should be known to any investigator using matrix inversion and should be stated that such studies have been done, and the problem is understood in full. In addition, the stopping criteria are not provided, the typical number of iterations are not given, and level of fitting is not provided - please provide these to the reader.

p4, line 7: A strong diagonal constraint is added below 60km... seems too vagues/imprecise for a technical paper. The presented figures show that the retrieval is not allowed any freedom to fit below 75km (e.g fig 3,4,5 + avg. kernels). I think the authors should be open with the constraints they apply and provide quantitative description of what the inversion really uses. Are there really no off diagonal terms in the regularization matrix? Please state what is the diagonal terms quantitatively (or as close to it as possible if the units are arbitrary), not only for CO2, but for the other unknowns. What is the correlation among these variable (if I understand the VV, VT and CO2 are fitted at the same time? if not how they are consistent as claimed?)

p4, line 8: How well is CO2 known at 60km and below? (this is not supported statement)

p4, line 18: Why is the figure 1 not shown withing the 2320-2380 region as figure 2? Would be good to see the entire window used rather than a particular line as an example. I find the physics of 4.3 um line formation in non-LTE in the freq. range discussed very important, a figure of vertical profiles for the different microwindows could also be included. The fact that Jacobians are broad reflects the physics of the problem, please discuss the physics little bit as relevant to the MIPAS resolved spectra.

p4, line 24: That is a strange statement; that precisely at 102 km altitude FB and SH swap importance. Please rephrase to reflect the fact that there is no such a clear cut boundary, it most certainly depends on season, and SZA angle.

p4, line 28: A less accurate non-LTE modeling for the isotopologues is mentioned here. Again, this is a strange statement, since the non-LTE calculation has to include all these species properly as they interact collisionally and exchange photons among layers, and this fact cannot be avoided. If one cannot model 636 non-LTE one cannot hope to get accurate 626 populations, then how should one understand this statement? Change this sentence and better justify the intended meaning.

p4, line 30: This is a nice and informative figure (2). Can it be color coded which dots belong to which bands for even better clarity.

p4, line 34: a priori LOS was taken from 15um region does not make sense in the current context. Again, what is meant by LOS (tang. height, or vertical grid spacing, or?)

p4, line 35: what/which smoothing constraint was used? please provide description and quantitative value.

p5, line 3: for clarify please make enumerated or bullet list of what are exactly the unknowns in this inversion and how many of them (grid points). Do the unknowns

include the VV, VT rates of the earlier paper ?(in either case please discuss this to bring a clarity to this issue)

p5, line 5: ..."below around 100 km" Tk comes from 15 um MIPAS inversion... Please be precise on these technical points: where do you join the MIPAS Tk profile upper/lower points and smoothing function used to connect with the Tk derived profile from the 5.3um. a) Does the 5.3 um Tk come from the same dataset? b) does it have the same calibration as the data used in this paper (baseline removal, calibration, etc)? c) what about the fact that the 5.3um Tk profile inversion depends on different p/T below and different O profile than the one used here (MSISE). Please examine the assumptions in your approach, incorporate it into discussion for justification and the error budget explicitly. d) The MIPAS 15um Tk inversion also depends on the inputs that are different (O, P(T), CO2), plus obviously different rates in the non-LTE task than used in this work. How is this inconsistency dealt with? e) Why is not the Tk profile from WACCM used in self-consistent manner, for the same reason why the O, O1D profiles are not used since in the model case it will be self-consistent? Please provide justification of using your selected approach. The reader cannot accept the final conclusions otherwise.

p5, line 7: The pressure was determined from Tk profile and density? Where did density come from? Or the pressure was known and temperature and density was unknown? What molecular weight was assumed (dry air?) how did you deal with the fact that above 100km (turbopause) there is diffusive separation and one needs to know scale height independently for each species? How does this enter into your error budget? A technical paper should provide a precise description of technical issues !

p5, line 10: I mentioned this before several times; what is the relationship between this CO2 and the one used in the fitting the spectra in cited paper? The reader is left completely guessing here on this issue. If there are differences, or inconsistencies please acknowledge them and justify that it is reasonable to do as you did, or that there is another component of error here?

p5, line 10-15: Here the authors finally reveal to some degree first inconsistencies between the two publications, however, its not clearly presented if in this paper the VV, VT rates are adjusted simultaneously or not. If not, how would the other rates derived in previous publication changed due to different O1D used in this work (the non-LTE task must be self-consistently solved for the entire CO2 + isotopologues problem).

In general I do not find this approach sound (adjusting rates to any radiance misfit). Perhaps the calculated and measured differences are due to inconsistent input atmosphere profiles? Ultimately, one is left to wonder how the newly derived rates would fit spectra in other context (Mars, Venus) for which measurements exists, or whether this is really ad-hoc approach used only for MIPAS. I do not see any evidence presented here that the accuracy of CO2 is better (only systematic error are quoted smaller)? Can you provide evidence for this claim that this particular rates needed to be improved, even though the entire CO2 non-LTE is affected by the O1D.

p5, line 20-30: This discussion is ok, but leaves important details out. How are the profiles joined? Why is the WACCM O, O1D not used which is consistent with the apriori CO2 (how much does it differ from the one estimated in your simplified model?) How is the O derived in the model different from O presented in Kaufamnn et al., (2014) (how accurate is it)? Do you obtain realistic distribution as shown there? Another question arises how is the scale height of O2 determined (space/time dependent?)

Section 4:

p6, line 7: What is meant by LOS retrieval (tang height, or vertical step?) Can you provide description (optimally prior to this section since you discuss it earlier). Since its not clear what is meant exactly: if I assume its vertical spacing what are the effects of growing vertical step on the along the LOS integration of RTE? For the optically thick lines, even-though these are said to be avoided as much as possible, there is still this issue? On the similar note, I would like to see some details on the frequency resolution of synthetic radiances and overlapping of lines of bands, and isotopic bands. Is this

treated or not, and what is the technical setup? In the same way, is the FOV taken into account in the retrieval of LOS? What is the uncertainty in the fact that there is horizontal variability of inputs (provide estimate on the CO2 non-LTE modeling and retrieval, especially over poles).

p6, line 10-15: This implies that the expected true atmospheric profiles should fall maximum 30-35% within WACCM profiles probably around 90km it appears(?) (this again assumes perfectly known forward model, and input parameters). This is not a reliable assumptions. In the self-consistency study presented the code should be investigated to find out its full range of applicability. What is the largest starting difference the code accepts and retains stability? This provides clues later in the application and interpretation.

a) Can you show the assumed CO2 VMR variance (respectively sigmas) assumed in the inversion at each altitude (best overplotted over the a priori). Is there any other covariance matrix involved? What form and why? [As I mentioned previously the retrieval methodology need better presentation prior to this section]. b) can you show the fit in the radiance space? Can you comment if you achieve the same amount of radiance fitting in the application to the measured MIPAS spectra? c) where do the waves in the relative difference come from in figures 3a, 3b? (due to random noise?) d) Can you present a case where the true CO2 vmr is not as completely smooth profile as selected for the best-case scenario presented?

p7, line 2: Interesting that finally in the text the authors acknowledge what is visible in the figures, the inversion cannot proceed below 75km. This should then be quoted as lower boundary, similarly for the upper point of 140km which is clearly an average over the 25-35km altitude region. Showing these nice resolution matrices I wonder how is the vertical resolution estimation actually derived? It does not appear that 10-20km resolution is the correct number cited in the abstract and in the discussion in the following section.

Section 5:

p7, line 12: Now we are moving the the results discussion, however, from my point of view in the uncontrolled way. First, some less heavily averaged CO2 VMR vertical profiles could be should (better even instantaneous), and discussed in comparison to WACCM. Second, a comparison has to be made how much different the retrieved CO2 is from the a priori WACCM âĂŤ ie. effects of regularization on the actually results. The fig 7, is zonal mean average of 2 years, this should be stated in the figure caption.

p7, line 20: As already mentioned its not clear how the vertical resolution was estimated given that the resolution matrix rows show much wider kernels. Another point already mentioned, how does horizontal smoothing affects the vertical resolution, provide at least reasonable estimate of this effect.

p7, line 25: O, O1D, P(T), solar flux and MIPAS gain are the only source of systematic uncertainty used. What about pressure in z=0 km?

p7, line 27: The Tk profile uncertainties assumed are surprisingly low above 100km. What is the justification to claim that Tk is known within 15K above 120km (in reality this cannot be lower than 40K for instantaneous profile from modeling results). Similarly, the paper of Bermejo-Pantaleon et al., (2011). I think the Tk uncertainty estimate should be less conservative and reflect our current knowledge of the instantaneous profiles above 110km.

p7, line 29-30: What is the justification for using the quoted uncertainty in O, and O1D, especially above 105 km? It is well known that O1D uncertainty yields asymmetric response on the retrieved CO2 VMR (Kaufamnn et al., 2002, and Rezac et al., 2015), this fact should be mentioned and evaluated.

p8, line 2-5: Now this seems to be an important point, also mentioned quite late in the paper. Are the P/T and O, O1D uncertainties treated only locally in the retrieved range or the perturbed profiles is down to ground/troposphere? (this is necessary especially

for P/T, but also O, O1D which have significant non-local response on the forward, hence, inverse problem below 75km and would be affected). Can you provide another figure applying a bias to the entire profile for the parameters and treating this as a bias error rather than a local perturbation which cannot properly expose the inherent non-linearity of the problem. If you still choose to present both approaches (linearized local one + full profile bias non-linear) discuss errors when calculating the numerical derivatives of deltaCO2_retrieved / delta_parameter. I have serious doubts that the code converges to the same fit starting from different conditions, it should not if treated correctly because its tied inherently to the physics, this error is at least 2% as demonstrated by your own figure 3, but also discussed in Kaufmann et al., (2002), and Rezac et al., (2015).

p8, line 20-25: Discuss the fact that the uncertainties in T,P, O, O1D produce non symmetric response in inversion, even if you choose to show only one side of the range.

p8, line 30: I hope in the updated version of the manuscript it will be clear by now what exactly is done with the LOS retrieved and the error discussion will also reflect this.

p9, line 1: Do you mean here that in the solution covariance you choose to ignore the Saˆ-1 ? First, as noted previously the Sa matrix should be specified in the description of algorithm if thats the case (along with the L-curve for the regularization) and clean discussion about stability issues in this regard.

p9, line 5-20: The systematic error in previous studies studies are in fact larger than quoted here mostly due to their assumption that the inputs (not only rates, but also necessary atmospheric profiles) are known much less accurately. The next version of this manuscript should re-examine this point as mentioned before, and include realistic uncertainties above 100km for all their inputs (p,T, O, O1D), and also estimate error due to missing physics (hot O, O1D, electron pumping of CO2 levels) if the results can be considered reasonable.

-It is not shown why the rates derived from MIPAS treated as auxiliary fit parameters instead of making sure the inputs are correct are considered accurate. Do they improve fitting spectra in the 4.3um of other planets? How is the accuracy judged?

p 10, line 3: The current results are enough important without overselling, please remove this statement "(120-140km). 1) SABER CO2 is is retrieved above 120km, 2) the current resolution indicates that the vertical resolution does not even allow to discriminate properly these altitudes.

p 10, line 5: I disagree with the statement, the accuracy of the retrieved CO2 is not better or at least has not been demonstrated to be better. The systematic error bars of retrieved CO2 are quoted smaller since the input uncertainties are assumed smaller. The most important point is that MIPAS offers frequency resolution which allows to properly choose which bands lines to include at which tang. altitude. Please indicate this properly in the conclusion and stress this point.

p10, line 7, This statement is not shown to be true either. The paper as written gives impression that the input parameters T, P, O, O1D are 1) inconsistent with each other in drastic way, 2) same with the rates which are calculated with different inputs than in this work, and 3) the calibration for these different data set are probably different (different version of MIPAS L1). Update the conclusion to properly discuss these points. On the contrary, the most important point driving possible lack of accuracy of this results is that the inputs are inconsistent and taken from different sources. If the authors want to stay with the current formulation, please invert all the required inputs self-consistently in proper way.

p 10, line 14: Again it is the one of the weakest points of this work that the inputs are inconsistent on many levels (I made this point several times throughout the review) and stating the opposite is rather strange.

p10: line 20-30: This discussion in new revision should reflect the points outlined above to give better estimated uncertainties.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-63, 2016.