We thank the reviewers for very thorough and constructive comments. The quality of the manuscript has been improved by these comments and suggestions. The following are our responses to the comments. The response (in blue) follows each comment.

Reviewer #1 (amtd-7-C882-2014):

General comments:

The authors developed a retrieval algorithm using radiance ratios (pairs) at a number of wavelengths. They claim that this method gives more accurate CO2 column retrievals (page 2412, line 6). This statement is based on comparisons with results obtained via an "Optimal Estimation retrieval algorithm" (OE), which is not specified in detail. It is therefore not clear if new the algorithm is really better or if this is an artifact from the use of a non-optimal OE algorithm. For me it is not clear why the new algorithm should be better. I guess this is simply because of the use of a non-optimal OE algorithm. The authors have to provide more convincing analysis to support their claim. I recommend to use TCCON spectra and to apply the new algorithm to these spectra and to compare the results with the official TCCON CO2 data product.

Answer: We have used TCCON spectra in Tsukuba, Japan (36.0513N，140.1215E) and Bremen, Germany (53.10N，8.85E) to validate DOAS-like algorithm and compare the results with the official TCCON CO2 data product in section 3. The results of this DOAS-like has an obvious systematical bias and airmass-dependent artefact. After correction, the results agree very well with those of TCCON.

In addition, the description of "this method gives more accurate CO_2 column retrievals " is not accurate. Our algorithm is still a very preliminary results, and more test and validation are needed in the future, while GIFT is well developed and validated and used as operational data processing for the TCCON network stations. Other than using the whole band by spectral fitting method, we are trying to use less points and to remove the points which are more sensitive to the error source such as H_2O, temperature etc., which results in that the DOAS-like is less dependent on the temperature and water vapor error.

Specific comments:

Abstract, line 13: "the results agree very well with that of GOSAT". This statement is based on Fig. 8, which is of very poor quality and does not support this statement. Please use a better y-scale, e.g., 380-400 ppm, and better symbols than tiny points. Only 7 GOSAT observations are shown. The correlation between the FTS and the GOSAT data seems to be close to zero. This sparse data set and its limited analysis does not support the statement given in the abstract.

Answer: We deleted this part as commented above. We have used TCCON measurements in Tsukuba and Bremen in our retrieval and compared with that of TCCON official algorithm the results and comparisons shows in the figure 6, figure 7 and figure 8 in the revised manuscript. The y-scale is modified for clear statements.

Introduction, page 2406, lines 16-20: Which "many other reasons" ? Please provide examples and add references or remove this statement. "The disagreement about this issue": which issue? Please be more specific.

Answer: A specific description have been added in the first paragraph of section 1. "However, Easterling and Wehner (2009) pointed that the records of surface air temperature showed no warming trend or even slight cooling while greenhouse gas levels were still increasing"

Introduction, page 2407, line 2: Please be more specific with respect to SCIAMACHY and GOSAT. For SCIAMACHY add the achieved precision and accuracy as reported in Reuter et al., 2011, and for GOSAT please add which algorithm and version you are referring to and cite the corresponding validation paper where the GOSAT data have been compared with TCCON. Later in the paper please mention which version of the GOSAT data have been used here and give a proper reference.

Answer: Sorry for the unspecific description. The X_{CO2} single-measurement precision of SCIAMACHY compared to FTS measurements is 2.5 ppm (Reuter et al., 2011). While the biases and standard deviations of XCO2 from GOSAT SWIR L2 V02.xx retrieval algorithm reach −1.48 and 2.09 ppm (Yoshida et al., 2012).
In the revised manuscript of this paper, we used the TCCON data instead to validate our algorithm.

Introduction, page 2407, line 8: A major reason why the described ground-based observations typically have higher accuracy is because the light path is known due to direct sun observations, whereas for satellite retrievals a much more complicated problem has to be solved, because scattered light is used (and the light path depends on surface albedo, aerosols and cirrus, etc.).

Answer: Thanks for the clear comments, more sentences have added in the second paragraph of section 1 .

Introduction, page 2407, line 24: What is "model parameter error" and what about non-model parameter errors?

Answer: "model parameter error" refers to error of temperature, pressure, etc. More description have been added in the third paragraph of section 1 .

Page 2408, line 17: Provide evidence (or cite an appropriate publication) why the scattering term is negligible. What about cirrus and (other) thin clouds?

Answer: The scattering term is negligible because of small FOV (2.4 mrad). The same treatment is applied to TCCON (Wunch et al., 2011a). Even if there are cirrus clouds or other thin clouds (for cloud optical depth less than 3) in the sky, the scattering can still be negligible.

Page 2409, line 5 an following. Please explain the symbols used more carefully. What exactly is

NCO2. Definitely not "the number of CO2 in the atmosphere". Is it the vertical column in number of CO2 molecules per surface area?

Answer: Sorry for the inaccurate description. 'N_{CO_2}' in AMTD means the total number of CO_2 molecules per surface area.

Page 2410, line 2. No evidence is given for the statement that the proposed method eliminates scattering effects. Provide evidence or remove this statement.

Answer: For clear sky condition, the aerosol optical depth is often far less than 0.1 in the SWIR bands, which results in too small scattering in the small FOV of FTS to be negligible. We have rewritten the formula (Eq. 1-3).

Page 2410, line 7 following. Same remark: No evidence is given where the statement that the listed parameters are the "main factors", which determine accuracy, is coming from. Provide evidence or remove this statement.

Answer: Sorry for the unclear expression, we have rewrite the sentences in the first paragraph of section 2.3.

Page 2410, line 12: Which noise is meant here? If you mean instrument noise than SNR is not "related to this" but a means to quantify the noise.

Answer: Thanks for the clear comments. The noise here means instrument noise. We have rewritten the sentences in the second paragraph of section 2.3.

Page 2410, line 14 following: From Fig. 1 I cannot see the linear relation. Please provide more evidence for this statement.

Answer: The expression of "the linear relation" is not appropriate. the relation between inversion error and number of channel-pairs is not necessarily linear. The figure is removed in revised version, new paragraphs are added for detail descriptions of the channel selection.

Page 2410, line 16: "errors are acceptable". Compared to what? What is the requirement here?

Answer: In AMTD, we mean the errors are acceptable compared to the expected inversion accuracy.

Page 2410, line 19: "shift is consistent in a limited spectral range". Probably you mean that the shift is limited to a small spectral range.

Answer: We mean that the shift is a constant in a very limited small spectral range. We have removed this ambiguous expression in 4th paragraph of section 2.3.

Page 2411, line 2: The channels are not "demonstrated" but shown. What are the pairs used? One cannot see this from Fig. 3.

Answer: The new channel-pairs are shown in Fig. 5. The average of yellow rhombus located in the weak absorption are used as the weak one of the pair, where each blue rhombus is used as relatively strong absorption wavelength.

Page 2411, line 15 following: Adding a random error to the temperature profile seems not to correspond to realistic relevant scenarios. Why not using more realistic temperature profile variations?

Answer: It's a good suggestion. We have used realistic temperature profiles to validate the dependence of DOAS-like and OE on temperature profile in 5th paragraph of section 2.3.

Page 2412, line 19 following: Concerning systematic errors caused by noise and adding a 0.9 ppm bias. This sounds strange. Apparently your retrievals are biased low by 0.9 ppm and you have added an offset to deal with this. If this is true please simply state this. Concerning the cause: This seems to be a speculation. How can noise generate a systematic bias?

Answer: In this revised version, the TCCON spectra recorded in Tsukuba and Bremen have been used in our retrieval and the results are compared with those of the TCCON official algorithm, the results and comparisons are shown in the figure 6, figure 7 and figure 8, more detailed descriptions have added in section 3.