We agree with most of remarks and thank the reviewer who helped us to eliminate an important mistake in the text. We present hereafter the corrected sentences with modified text marked by yellow.

Scientific issues:

In various places of the text it is not clear that only along line-of-sight velocities are measured. E.g., add in line 95 "with along line-of-sight velocities"; line 180: the along-line-of-sight component of the wind...; since the method provides slant wind velocities, the calculation of the vertical wind speed requires knowledge of the horizontal components. Where are these taken from? This should be discussed latest in Section 3.3. (This is my most serious scientific concern).

In fact, any Doppler velocimetry techniques only provides velocity projection on the line of sight, with velocity component normal to the line of sight being inaccessible for Doppler effect. This is a natural limitation of the Doppler method and we cannot pretend to overcome it. However, in meteorology it is commonly accepted that horizontal wind component is typically much higher than the vertical one, due to very strong anisotropy of the atmosphere on the scale of tens km. Hence, with the accuracy of the method we propose, which does not exceed few m/s, we may neglect the vertical wind component and retrieve wind speed projection on the current direction of the Sun. Again, the method is unable to access the normal wind component, if any. Corresponding notes is added to the text, as recommended by the reviewer.

Line 90: Taking into account that LO linewidth has an order of 2MHz, the spectral resolution of heterodyne detection is sufficient to measure Doppler shift of the absorption line in the atmosphere due to air mass motion with along line-of-sight velocities greater than 3 m/s, provided high LO stability and sufficient accuracy of intermediate frequency (IF) signal analysis.

Line 184: This fact gives an opportunity of line-of-sight component of the wind profile retrieval based on analysis of the resolved line shape.

l 146-147: Not sure if this statement belongs here. I think it would fit in much better with the Conclusion section.

Absolutely correct. Done.

Line 285: Further development of this technique is possible by implementing of multichannel photodetector configuration. This could significantly decrease the integration time and increase signal-to-noise ratio.

l 167: The Boone reference seems to refer to a specific method to calculate line mixing. What about also including references on the discovery and measurement of the physical effect (e.g., Armstrong (1982), Bulanin et al. (1984), Strow et al. (1986) orHartmann (1989)? It will certainly not be necessary to include all of these, but I suggest to include at least one historically relevant paper. @article{ARMSTRONG82,

author = "R. L. Armstrong",

title = "Line mixing in the ν_2 band of {CO$_2$}",

journal= "Appl. Opt.",

volume = "21",

number = "12",

pages = "2141-2145",

year = "1982" }

@article{BULANIN84,

author = "M. O. Bulanin and A. B. Dokuchaev and M. V. Tonkov and N. N. Filippov",

title = "Influence of Line Interference on the Vibration–Rotation Band Shapes",

journal= "J. Quant. Spectrosc. Radiat. Transfer",

volume = "31",

number = "6",

pages = "521-543",

year = "1984" }

@article{STROW86,

author = "L. L. Strow and B. M. Gentry",

title = "Rotational collisional narrowing in an infrared {CO}$_2$ {{\it Q}} branch studied with a tunable diode laser",

journal= "J. Chem. Phys.",

volume = "84",

number = "3",

pages = "1149-1156",

year = "1986" }

@article{HARTMANN89,

author = "J. M. Hartmann",

title = "Measurements and calculations of {CO$_2$} room–temperature high-pressurespectra in the 4.3~μm region",

journal= "J. Chem. Phys.",

volume = "90",

number = "6",

pages = "2944-2950",

year = "1989" }

Reference included

Line 162: In calculations we used Voight profile and neglected line mixing and other fine effects that may affect line shape in its far wings (Armstrong, 1982, Bulanin et al., 1984, Boone et al., 2011).

l171-179: The radiative transfer model seems not to consider refraction. Is this important? How large is the related error?

This is second-order effect that may be neglected, however, we made it more clear.

Line 171: Minor effects of second order in terms of line-of-sight curvative, such as atmospheric refraction and sphericity of the atmosphere have been neglected as well.

same place: Is the numerical accuracy obtained from 100 uniform layers good enough? Is the integration of the radiative transfer equation based on arithmetic mean values of pressure and temperature of the upper and lower boundaries, or is a more sophisticated numerical integration scheme used, relying on mass-weighted mean values or any other integration scheme which takes into account that there are systematically more CO2 molecules in the path segment in the lower half of the layer than in that above? I think that this numerical integration issue deserves some discussion.

We employed a standard procedure of synthetic absorption spectrum calculation widely used for atmospheric retrievals. In particular, the model preserves the total amounts of atmospheric gases, which in the hydrostatic approximation is equivalent to the pressure profile. However, even provided some error in the net absorption calculation, wind retrievals are only relevant to the line shape rather than line depth. We added specific note to the text.

Line 172: The model does not pretend to provide precise calculation of the net absorption, as if were required for CO_2 column amounts retrievals, whereas it is line shape rather than line depth, that contains information about line-of-sight wind component.

l. 189: I think that the adequate reference for ill-posedness is Jacques Hadamard: Sur les problèmes aux dérivées partielles et leur signification physique. In: Princeton University Bulletin. Bd. 13, Nr. 4, 1902, ZDB-ID 1282693-5, S. 49–52. Reference to the Rodgers book can be made, e.g., in the context of Eq. (12).

Done:

Line 189: This term means that a solution is not unique and/or reveals instability versus small variations of data such a solution is based on (Hadamard, 1902)

Line 240: Averaging kernel which characterizes the sensitivity of a regularized solution to exact one is defined as (see Rodgers, 2000):

Technical and Language issues: Caveat: I am not a native English speaker. Thus mylanguage-related recommendations should be taken with care.

l. 1: ... from THE troposphere...l. 11: CO2: use subscript "2"

Agree

l. 14: ... provides AN unambiguous ...

Agree

l. 15: between THE offset ... and THE altitude ... where THE respective ...

Agree

l. 16: ...retrieve THE vertical ... with the vertical resolution of the retrievals...

Agree

l. 18: ... retrieved wind profileS ...

Agree

l. 30: Not only provide heterodyne ...

Not only provides….

l. 35: ... have been ...l. 39: Radiometric Doppler [remove "As"]... AN extraordinary...

Agree

l. 46: The heterodyne method...

Agree

l. 63: I am not sure if the term "experiment" is appropriate here. An experiment is often understood to be an observation under controlled conditions, and this is not typically the case for atmospheric observations. Perhaps better "instrument", "instrumental setup", or "measurement principle" or something similar.

Changed by

2 The instrumental setup

l. 82: The power spectrum

Agree

l. 88: ..., the heterodyne signal...

Agree

l. 94: ... that the LO linewidth...

Agree

l. 108: ...with A 5:95 ... According to THE antenna...

Agree

l. 115: both THE heterodyne and THE reference...

Agree

l. 118: ... with A pulse length ... A dead time ... and A stepwise...

Agree

l. 119: ...at A pressure ...

Agree

l. 127: ... through A consecutive ...

Agree

l. 128: ... The preamplifier circuit ... to A Rohde...

Agree

l. 130: THE LO pumping ...

Agree

l. 131: THE oscilloscope (or AN oscilloscope)...

Agree

l. 139: ... by THE Fabry...

a Fabry…

l. 141: have shown an root mean squares deviation ...

Agree

l. 151: Once THE heterodyne signal...

Agree

l. 152: ... eliminate THE baseline slope... Since THE heterodyne...

Agree

l. 155: of THE heterodyne ... and THE baseline...l. 155/156: I would suggest to turn around: The final transmission ... is the ratio ...".Or if you do not want to turn the sentence around, you might with to replace "is" with "renders"

This sentence is rephrased according to another reviewer's comment.

After subtraction of dark signal, heterodyne signal should be normalized by assumed spectral continuum (baseline) approximated by square polynomial to obtain the final transmission spectrum of the atmosphere.

l. 157: notice A baseline...

Agree

l. 161: ... LHS THE scattered ... (???) ...the simulation of atmospheric spectra is...

Agree

l. 163: in THE calculations.

Agree

l. 165: ... A model of ... (???)

Agree

l. 166: In THE calculations, we used the Voigt line shape

Agree

l. 172: THE upper boundary ... uppermost layer ....

Agree

l. 173: AN example of THE line shape fit...

Agree

l. 174: ... that THE line shape...

Agree

l. 175: The atmospheric line...

Agree

l. 177: collisional Lorentz broadening ... whereas A narrow...

Agree

l. 178: that PROVIDES information {???}

Agree

l. 180: ... with OTS line shape and...

with transmittance spectrum line shape…

l. 190: introduces A certain ... forces THE instability ...

Agree

l. 191: consider THE generalized...

Agree

l. 206: implies THE introduction

Agree

l. 217: THE vertical wind ...

Agree

l. 236: Here a reference seems not to be resolved.

Done

l. 238: THE averaging kernel...In general the notation seems not to comply with AMT format guidelines. E.g. Matrices and vectors should be bold face. etc.

Agree. Equation format will be corrected later on the editor's demand

l. 241: ...to THAT particular altitude where THE ... so that one may consider it as a point ...

Agree

l. 243: Therefore, the characteristic width of the main peak of the averaging kernel is a measure of the effective...

Agree