Author comments for Anonymous Referee #1

The authors thank the referee for her/his valuable comments and the positive overall evaluation of our manuscript. We carefully addressed all comments and accounted for them in our revised paper as stated below. The document is structured as follows. The original referee comment is provided in *italic*, followed by the author's response and author's change in the manuscript (deleted parts are ~~scored out~~ and added parts are **bold**).
(Remark: it seems that the referee used the initially uploaded version of the manuscript, before the typesetting was done. Therefore the page and line numbers given in the specific comments do not match the published AMTD version of the manuscript. We could clearly locate everything, however.)

Referee comment #1
By the very nature of the simulations carried out in this work, in which the same line parameters used to simulate the initial spectra are also used in the retrieval, the authors are neglecting systematic errors arising from uncertainties in spectroscopic line parameters. Such errors will add to the simulation errors reported in this work.
The spectral simulations are made using the Voigt line parameters from the HITRAN database. It is well know that these parameters can have errors up to 10% or even higher, and that the Voigt lineshape is only a simple approximation to the true lineshape, meaning that differences between forward-model-calculated and observed spectra are generally above the noise level. In order to extract winds from spectroscopic lines, very accurate line parameters are required. What are the spectroscopic requirements for real-world applications? The authors need to address this question.

Author's response #1
Yes, thank you, we agree that we should briefly address this issue also here. Harrison et al. (2011) wrote a paper on the spectroscopic requirements for ACCURATE. We now added a paragraph on the topic to the manuscript, including the Harrison et al. (2011) reference, plus an auxiliary citation of the Rothman et al. (1998) paper.

Author's change in manuscript #1
Starting on page 420, line 18: This RFM/HITRAN/FASCODE subsystem is integrated in the forward modeling sub-tool of the EGOPS/xEGOPS system so that realistic IR absorption computations along ray propagation paths are seamlessly possible. [we added:]
Regarding spectroscopic uncertainties, the HITRAN 2008 database used does provide indication of uncertainties of the spectroscopic parameters, with the line intensity and the air-broadened half width being the main contributors (Harrison et al. (2011); Rothman et al. (1998), Appendix A2 therein). Harrison et al. (2011) studied the spectroscopic requirements for an ACCURATE-type mission concept and concluded that the current spectroscopic knowledge on the targeted absorption lines needs improvement by new highly accurate laboratory measurements in order to meet the ACCURATE requirements (Kirchengast and Schweitzer, 2011). They find such accurate spectroscopic measurements feasible with state-of-the-art laser spectroscopy which targets single absorption lines so that spectroscopic uncertainties could be reduced to about 0.1 %, and line-center frequency uncertainties to within 10^{-8}, meeting the requirements. We therefore did not include spectroscopic error modeling here, since we can assume that highly accurate spectroscopy will be available at launch of an ACCURATE-type mission and that the spectroscopic errors will then be minor to other errors accounted for here (e.g., thermal noise and residual errors from

scintillations, as summarized below).

new citations added in References section:

Harrison, J. J., Bernath, P. F., and Kirchengast, G.: Spectroscopic requirements for ACCURATE, a microwave and infrared-laser occultation satellite mission, J. Quant. Spectrosc. Ra., 112, 2347–2354, 2011.

Rothman, L. S., Rinsland, C. P., Goldman, A., Massie, S. T., Edwards, D. P., Flaud, J.-M., Perrin, A., Camy-Peyret, C., Dana, V., Mandin, J.-Y., Schroeder, J., McCann, A., Gamache, R. R., Wattson, R. B., Yoshino, K., Cahnce, K. V., Jucks, K. W., Brown, L. R., Nemtchinov, V., and Varanasi, P.: The HITRAN molecular spectrsoscopic database and HAWKS (Hitran Atmospheric WorKStation): 1996 edition, J. Quant. Spectrosc. Ra., 60, 665–710, 1998.

Specific comments

Referee comment #2

Page 5, line 9: The problem in citing an ESA document here is that it is not peer reviewed and not in the public domain, meaning that the reader has no access to it. I would recommend either citing another source, or adding a summary of the main in an appendix / supplementary material.
Page 7, lines 17-18: Same comment as before. Noone has access to the referenced document. I would also prefer to see a more in depth discussion on the derivation of the Abel transorm, either in an appendix or as supplementary material.

Author's response #2

We are aware of the problem if peer-reviewed papers are not (yet) available for all aspects to be cited. However, the content of the relevant ESA report in question was meanwhile submitted to the Journal of Geophysical Research (JGR) (Syndergaard and Kirchengast, “An Abel transform for deriving line-of-sight wind profiles from LEO-LEO infrared laser occultation measurements”) where it is currently in review. The ESA report itself is part of an international report series of the institute (the “Reports to ESA” series established since almost 20 years) and it is Wegener Center's policy to put all such cited institute reports permanently online under the publications webpage, for ensuring access on a long-term basis. Many peer-reviewed papers include a few such citations, if found unavoidable, and this is also the case here. We will make sure we now cite also the JGR paper in review, together with the ESA report; in this way people will easily find the JGR paper in a couple of months after its publication.
Regarding more details on the Abel transform etc. as Appendix or Supplementary Material here: given we now have the JGR paper in the pipeline, which we co-cite with the ESA report, we prefer not to include this material also into this AMT paper, since it would be redundant.

Author's change in manuscript #2

We now added a reference to the JGR paper in review, i.e., we changed all relevant citations in the text from “Syndergaard and Kirchengast (2013)” to “Syndergaard and Kirchengast (2013; 2015)” and added the JGR paper in review to the Reference list (in line with the AMT “Manuscript preparation guidelines for authors” available online at the AMT website).

new citation added in References section:

Syndergaard, S., and Kirchengast, G.: An Abel transform for deriving line-of-sight wind profiles from LEO-LEO infrared laser occultation measurements, J. Geophys. Res. Atmos., in review, 2015

Referee comment #3
Page 8, lines 10-15: The l.o.s. Doppler shift is smaller in magnitude than the kinematic Doppler shifts. How do typical l.o.s. shifts compare with typical uncertainties in the larger shifts? Again, note that readers will not have access to the cited mission proposal.

Author's response #3
The wind-induced Doppler shifts are of relative magnitude 10^{-7} and the larger shifts show a typical uncertainty of smaller than 10^{-8}. This is discussed in the cited mission proposal but also in the Syndergaard and Kirchengast (2015) JGR paper in review.
The cited mission proposal was published as a scientific report of the Wegener Center Verlag and is therefore permanently available online via the URL cited in the Reference list (as is the case for all reports of the Wegener Center Verlag, which are ISBN-registered and also archived to these standards).

Author's change in manuscript #3
Starting on page 412 line 21: ...can be predicted and accounted for accurately in the instrument design, **leaving uncertainties smaller than 10^{-8} so that the l.o.s. wind is well observable.**

Referee comment #4
Page 8, lines 16-18: Give an estimate of the typical bias caused by the neglect of l.o.s. wind.

Author comment #4
For wind speeds < 10 m/s the bias induced is very small and within the accepted error level (~0.1 % or smaller). For typical wind speeds of several 10 m/s the bias is at the 1 % level (e.g., up to around 5 % for CO_2 given very high wind speeds of about 75 m/s, see Fig. 8; the bias increases non-linearly, since the more the channel absorption moves out of the absorption line center, the more curved is the line shape).

Author's change in manuscript #4
Starting on page 412 line 26: The remaining Doppler shift caused by the prevailing l.o.s. wind speed is, if not corrected for, adding a small bias to the retrieved GHG profiles in case of wind speeds exceeding 10 ms^{-1} (cf. Schweitzer et al. (2011b), Sect. 3.8.3 therein). **Typical biases for wind speeds of several 10 ms^{-1} are of magnitude 1 % in the VMR of trace species.**

Referee comment #5
Page 9, line 3: Please define the impact parameter.

Author's response #5
The impact parameter is the perpendicular distance between the ray path and the center of curvature in the occultation plane. Subtracting geoid undulation and the Earth's radius provides the impact altitude. To get the mean sea level altitude one has to consider the refractivity of the atmosphere.

Author's change in manuscript #5
Starting on page 413 line 13: There are some preparatory steps necessary to provide these transmission profiles as a function of impact parameter (“IR impact parameter”) and altitude (“IR altitude”), shown in Fig. 2 as gray box **(the impact parameter is the perpendicular distance between the raypath and the Earth's center of curvature in the occultation event plane; see Proschek et al. (2011) for details).**

Referee comment #6
Page 10, line 4: How does the reader access these derivations? Please include them in an appendix or in supplementary material.

Author's response #6
See our author's response #2 above.

Author's change in manuscript #6
See our author's response #2 above.

Referee comment #7
Page 16, lines 3-4: HITRAN 2012 has been out now for some time. Why has this not been utilised?

Author's response #7
HITRAN 2012 does not provide any relevant change for the selected lines; we carefully compared our target lines between HITRANN 2008 and 2012 and found changes minimal. Therefore we wanted to stay consistent with another recent paper also dealing with ACCURATE in end-to-end simulation.

Author's change in manuscript #7
Starting on page 420 line 13: We calculate the IR absorption with the RFM model, employing the High Resolution Transmission (HITRAN) 2008 (Rothman et al., 2009) database for supplying the spectroscopic line parameters. **We cross-checked the more recent HITRAN 2012 database (Rothman et al., 2013) and found that changes, if any, are very small for our selected lines and would not impose any relevant change to the results. We therefore kept consistency with the recent Proschek et al. (2014) end-to-end simulation study that also used HITRAN 2008.**

new citation added in References section:
Rothman, L. S., I. E. Gordon, Y. Babikov, A. Barbe, D. C. Benner, P. F. Bernath, M. Birk, L. Bizzocchi, V. Boudon, L. R. Brown, A. Campargue, K. Chance, E. A. Cohen, L. H. Coudert, V. M. Devi, B. J. Drouin, A. Fayt, J.-M. Flaud, R. R. Gamache, J. J. Harrison, J.-M. Hartmann, C. Hill, J. T. Hodges, D. Jacquemart, A. Jolly, J. Lamouroux, R. J. L. Roy, G. Li, D. A. Long, O. M. Lyulin, C. J. Mackie, S. T. Massie, S. Mikhailenko, H. S. P. Müller, O. V. Naumenko, A. V. Nikitin, J. Orphal, V. Perevalov, A. Perrin, E. R. Polovtseva, C. Richard, M. A. H. Smith, E. Starikova, K. Sung, S. Tashkun, J. Tennyson, G. C. Toon, V. G. Tyuterev, and G. Wagner (2013), The HITRAN2012 molecular spectroscopic database, Journal of Quantitative Spectroscopy and Radiative Transfer, 130, 4–50, doi:10.1016/j.jqsrt.2013.07.002.

Referee comment #8
Page 16, lines 11-22: I find this whole section somewhat unclear. Was the pressure shift parameter for the C18OO line set to zero for the initial simulation of the atmospheric spectra? If so, this should be clarified. Please briefly describe how the tunable Tx laser design would compensate for the observed pressure shift. To achieve the best signal, the Tx laser lines must sit at wavenumbers corresponding to the inflection points of the line. Given that the true lineshape is non-Voigt (not the simple Voigt assumed by HITRAN), how easily can this be achieved? There also needs to be discussion of how the delta(k0) term compensates for the observed pressure shift. Which of these compensations is more important? Would they contribute any error to the derived winds? I note that the authors claim that ignoring the pressure shift parameter for the C18OO line is a 'reasonable choice'. Without further supporting information, it is difficult to ascertain just how reasonable this is. Certainly this is not clear

to me, particularly as the entire point of this work is to demonstrate the feasibility of retrieving wind from observations made by an ACCURATE-type instrument.

Author's response #8

We agree that our pressure shift treatment in this study, or more precisely disregarding it in the simulations, should have stronger justification. Favorably, from our recently consolidated results in the Syndergaard and Kirchengast (2015) JGR paper such a justification is now more straightforward: the option of a tunable Tx frequency design is not needed, since it became clear that the pressure shift is anyway robustly accounted for by the Abel transform. Therefore the choice to disregard it just for technical purposes in this study is reasonable, since accounting for it would not change much (in particular so above 15 km, which is the altitude range of relevance for the wind retrieval). And we no longer mention the tunable Tx frequency option, since it would not be worth implementing in a real ACCURATE mission.

Author's change in manuscript #8

Starting on page 420 line 25: ...We therefore disregarded the pressure shift parameter for the $C^{18}OO$ line in the present simulations**, i.e., set it to zero. Syndergaard and Kirchengast (2015) confirmed that this is justified, since they fully included the pressure shift and found that the Δk_0 term in the Abel transform accurately compensates for its effects within the retrieval. Thus ignoring the pressure shift in the simulations here** is a reasonable choice...

Referee comment #9

Page 19, lines 14-17: It is clear that the Abel transform-type retrieval algorithm is better than the simple approach, which only assumes a constant l.o.s. wind speed. But is it the best? Have the authors looked into alternative schemes of extracting wind from spectroscopic measurements?

Author's response #9

Yes, indeed we see it to our best knowledge as the best scheme for active limb sounding (occultation) techniques, where local spherical symmetry around tangent locations of occultation events is a very good assumption. Any other reasonable scheme for this remote sensing type and geometry would, in one way or the other, also need to go for an Abel transform-type solution (although not necessarily implemented the integral directly but nevertheless using its underlying properties). Any other solutions at next-higher sophistication level will need some prior information added, e.g., regarding horizontal variability on top of local spherical symmetry.

Author's change in manuscript #9

None.

Referee comment #10

Page 19, lines 26-28: I would like to see some plots of the VMRs associated with these errors. How do the retrieved VMRs change with the inclusion of wind, not just their errors?

Author's response #10

Please see the following paragraph where Figure 8 is discussed. Figure 8 shows the change of the VMRs with the inclusion of the wind retrieval for two geographic locations and two GHGs. One event with rather strong and one event with weak wind speeds, to be representative. We consider this Figure 8 just to give a suitable indication and impression of the type and size of VMR errors that are typical.

Author's change in manuscript #10
None.

Referee comment #11
Page 20, lines 20-22: The authors claim that ECMWF short-range forecast wind fields can generally do a reasonable job. It is not clear to me exactly what they are referring to. To setup their simulation the authors used an ECMWF analysis field for the 'true' profiles, with a shortrange forecast field providing initial/background profiles for the retrieval. But are these two sets of profiles independent enough? Would these forecasts be accurate enough for a real ACCURATE-type mission?

Author's response #11
The "reasonable job" is referring to the fact that ECMWF analyses and forecasts are nowadays very good, with wind errors in the troposphere usually clearly smaller than 10 m/s. Yes, the 24h forecasts are sufficiently independent from the analyses (no concern for this application, the retrieval itself is fully robust even if starting with zero-wind, always converging fast to the same result) and, yes, already todays short-range forecasts would be accurate enough for a real ACCURATE-type mission, in particular in the troposphere (up to about 15 km) where good quality within 10 m/s is required. In the stratosphere errors increase (still usually they are not very large since mass field constraints, like from assimilating GNSS radio occultation observations that are accurate up to 35 km, help the wind field determination in the data assimilation and forecasting system). In the stratosphere, the wind retrievals will be clearly preferable and as we show in the study they are expected to be accurate from about 15 km upwards. We somewhat improved the relevant manuscript text to better reflect these considerations.

Author's change in manuscript #11
Starting on page 425 line 25: One general conclusion here is as well that for aiding the GHG retrieval with Doppler shift correction also ECMWF short-range forecast wind fields can generally do a reasonable job **in the troposphere** given their wind uncertainties typically **are smaller than 10 m/s. Above 15 km the retrieved wind profiles are clearly preferable, since the uncertainties of the ECMWF analyses and short-range forecasts increase into the stratosphere due to the more sparse observations available for data assimilation at these altitudes.**

Technical corrections
Referee comment #12
Page 3, line 1: Please expand the acronym ACCURATE. Page 3, line 20: contrast, not contrary Page 6, line20: Rayleigh scattering Page 15, lines 4-6: There is no need to explain the acronym EGOPS and xEGOPS twice since the names are so similar. Page 15, line 16: Planning Page 17, line 19: Remove "of the basis" Page 28-29: Define FOM and OPS in the Figure 2 caption

Author's response #12
Page 17, line 19: Remove "of the basis", was already corrected during the typesetting. We agree with the other change suggestions and corrected accordingly were needed, thank you. Note, ACCURATE is used as a proper name in the Kirchengast et al. (2010) proposal, i.e., is no acronym with a direct spell-out; but we make more clear now that the full name of the mission is ACCURATE, followed by a long hyphen, then followed by a key sentence telling the purpose of the mission.

Author's change in manuscript #12
Starting on page 407 line 1: This study is based on the mission concept **'ACCURATE—climate**

benchmark profiling of greenhouse gases and thermodynamic variables and wind from space' (Kirchengast et al., 2010).

Starting on page 407 line 21: In ~~contrary~~ **contrast** to RO, LMIO utilizes a LEO satellite actively transmitting limb-sounding microwave (MW) and infrared-laser (IR) signals to a receiving LEO satellite.

Starting on page 410 line 25: The forward simulations were done applying spherical symmetry of the atmosphere about tangent point locations and taking the following atmospheric effects into account cf. Schweitzer et al. (2011b): defocusing loss, medium load aerosol extinction, Rayleigh **scattering**, and scintillations; details on the simulation setup are given in Sect. 3.

Starting on page 419 line 14: We performed LMIO end-to-end simulations with the ~~eXtended End-to-End Generic Occultation Performance Simulation and Processing System (xEGOPS)/End-to-End Generic Occultation Performance Simulation and Processing System (EGOPS)EGOPS~~ **End-to-End Generic Occultation Performance Simulation and Processing System (EGOPS) and eXtended EGOPS (xEGOPS)** using realistic atmospheric conditions for MW and IR-laser signals.

Starting on page 419 line 25: These calculations were done with the Mission Analysis/~~Planing~~ **Planning** sub-tool of the EGOPS/xEGOPS system.

Added to caption of figure 2 (page 434, starting in line 5 of the caption): ...at the right hand side **where FOM denotes forward modeled and OPS retrieved delta-differential transmission profiles**), and the core algorithm...