Response to Referee #2

We thank Referee #2 for his/her helpful comments. We carefully addressed all comments and accounted for them in our paper as stated below, where we answer all comments. The original comments of the Referee are cited in *italic* font, our response is put below each comment in standard font.

The paper discusses the influence of the atmosphere on laser light propagation in the novel laser occultation technique. The paper deserves publication in AMT after a revision. Comments are below.

Comments on presentation

1) I think that the paper would benefit from a more clear and concise description. For example, it is stated three times in pages 2695-2698 (essentially, in one subsection) that the LIO propagation simulations were performed with EGOPS+xEGOPS system; a paragraph about "interesting" and other atmospheric effects in page 2695, lines 15-24, contains essentially the information that laser light is refracted, absorbed and scattered in the atmosphere (as all effects are discussed below), etc. I recommend the authors to revise the text, in order to clarify/shorten the description and avoid multiple repetitions and multiple references to the text below.

We streamlined the text by removing the multiple references to the EGOPS+xEGOPS system and by shortening the paragraph about the atmospheric influences mentioned above. In addition, we streamlined the abstract, the introduction (section 1) and the summary (section 4).

2) The "defocusing effect" discussed in the paper is known as "refractive attenuation"
or "refractive dilution" in the literature. The term "atmospheric loss" can be misleading. The term "extinction" is used in scientific literature for this effect.
I recommend using the standard terms.

We agree that alternative terminology such as suggested is possible, which is mainly used in optical literature and applied optics fields, typically dealing with UV/VIS/NIR wavelengths. However we prefer to keep the term "defocusing" here, since it is as well commonly used by the community for a long time already (e.g. Born and Wolf, 1964; Kursinski at al. 1997), in particular if there is relation to IR/MW/Radio wavelengths. Also for consistency with other recently published work on IR-laser and MW occultation it is meaningful we keep this terminology track.

We also preferred to keep the term "atmospheric loss", and more generally "<process> loss" (<process> = defocusing, species absorption, aerosol extinction, etc., up to total atmospheric), since our discussion is focused on the integrated attenuation of LIO signals along the ray paths, which results in a loss of intensity at the receiver. Hence, "<process> loss" directly expresses the factor by which the original transmitter signal has been attenuated due to a certain process when observed by the receiver telescope. That is, loss

is the reciprocal of transmission, in units dB conveniently exhibiting the same absolute value as transmission but a positive rather than a negative sign. So loss at the receiver is the variable of core interest in our study, while various kinds of "extinction" (like extinction from absorption or scattering) are interaction processes of matter and radiation that locally act on the radiation during the signal propagation, contributing to accruing the total losses finally seen at the receiver.

Clearly it is true that this type of terminology like "attenuation", "loss", etc., also the use of units dB, is more from the communities focusing on IR/MW/Radio wavelengths rather than those focusing on the "classical" optical domain of UV/VIS/NIR wavelengths. So just the IR (NIR/SWIR/TIR/FIR) is obviously the domain where one has to decide for some terminological preferences and given we deal with a joint SWIR/MW method here explains our decision.

3) Section 1. Logic of introduction: you mention that SWIR and MW signals are used together, then discuss SWIR measurements, then MW measurements, then again SWIR measurements. Maybe, it would be better (more logical) to write first about MW measurements, and then concentrate on SWIR measurements.

We decided to use this approach, because the SWIR technique is the main topic investigated in this paper; we think, mentioning it first supports the attention on it. That is we considered this way of flow didactically better, in particular since IR-laser occultation is novel and therefore not yet broadly known by the community. Shortly mentioning the MW technique next is then needed to have the connection to understand the combined SWIR+MW system, which is discussed afterwards in this section.

4) I recommend avoiding the scientific jargon like "grey literature" or "assessed from scratch".

We now use "scientific-technical reports" instead of "grey literature reports".

"assessed essentially from scratch" is not scientific jargon from our point of view since it is commonly used to express a start from the very beginning (as we needed to do since no previous literature was available because we just recently conceived the method).

Detailed comments on contents

1) It is important to note in page 2703 that MW refractivity in Smith-Weintraub formula does not depend on wavelength (otherwise it might look strange to state that MW and SWIR refractivity are compared, while comparing two different formulae for SWIR wavelengths).

We agree and enhanced the sentence as follows:

"The MW refractivity is represented by the Smith-Weintraub formula (e.g., Schweitzer et al., 2011), which is independent of wavelength since dispersion is negligible over the relevant LMO wavelength range, the SWIR refractivity by the…"

2) Authors state in page 2705 line 5 "The oscillating features in the profiles stem from strong temperature gradients around the tropopause". Indeed, the air density structure near the tropopause acts as a lens and produces non-monotonic profile of refractive attenuation (like green line in Fig.3). However, the oscillations like in red line are not expected (the red curve looks like erroneous). The refractive attenuation is related to smooth air density profile (excluding fluctuations caused by e.g. gravity waves). Another reason for oscillations on the red profile might be numerical instability of computations. I suggest checking that the refractivity profiles used in calculation of refractive attenuation are smooth.

The defocusing depends on the second derivative of the refractivity, i.e., the curvature. This is why also small variations in the refractivity profile around the tropopause, as is the case here, can result in defocusing modulations as shown in the red profile. We re-checked this and confirm it is no artifact but comes from the characteristic sharp vertical temperature structure around the tropical tropopause (as we write in the paper). In our study this is represented by the FASCODE "tro.atm" profile—an illustration of this one, also of the std.atm and saw.atm, can be seen in Figure S2 of Kirchengast and Schweitzer, GRL, L13701, 2011, online at www.agu.org/pubs/crossref/2011/2011GL047617.shtml.

While the black std.atm profile also leads to some small "secondary" oscillations below the primary tropopause-induced fluctuation (the one at ray tangent heights in the direct vicinity of the tropopause kink), the tro.atm profile has a much sharper kink. Thus it leads to stronger "secondary" oscillations, since these derive from the tropopause "felt" at inbound-half and outbound-half of rays with tangent heights below the tropopause. An additional small contributing effect of "wiggling" of the profiles comes from the natural cubic spline interpolation on the tabulated FASCODE height grid (this is seen also on the green profile). However, from a practical link budget perspective even the tro.atm "secondary" oscillations are small (about ± 0.5 dB amplitude or smaller) and, furthermore, defocusing cancels in all applications using differential transmission, like greenhouse gas retrievals, since its wavelength dependence is negligible.

3) page 2715: Was the total influence computed as a sum of all "losses" or it was estimated using the simulator with all effects included? Please clarify.

It was computed, not estimated. We re-checked that this is also made clear in the text.

4) P. 2692 l.7 and p.2724, l.14 : "turbulence strength" - what parameter is assumed here?

This can be, for example, the r.m.s. of the relative refractivity fluctuations, the refractive index structure constant, the turbulent kinetic energy. It would depend on the estimation algorithm applied to the high-frequency information contained in the LMIO transmission data and the interest of the user in a certain measure of turbulence strength. We have left this intentionally open at this point in time since this is part of the by-products, which can be defined in more detail later.

5) P. 2692 l.25 -> The sentence "This are(is) a self-calibration step..." The self calibration does not necessary guarantee "very accurate retrieval results": high signal-to-noise ratio is also required. It does not also guarantee "free-of-bias" retrievals, because a bias can result from other sources, e.g., from spectroscopic uncertainties.

We clarified this in the paper by modifying the sentence as follows:

"This together with a high signal-to-noise ratio and a self-calibration step in the retrieval algorithm, which is intrinsic…, is the reason why the LIO retrieval results are expected to be very accurate and essentially free of measurement biases…".

6) P.2695: Scintillations are also due to refraction.

We clarified this while streamlining the whole paragraph concerned, in response to a comment of Referee #1. The sentence addressing the scintillations now reads:

"Important refractive effects are bending of ray paths and defocusing, caused by the (primarily vertical) gradient and curvature structure of the atmospheric refractivity, as well as scintillations caused by turbulence, i.e., by small-scale random fluctuations of the refractivity."

7) P.2720: "Saturation occurs, when a signal is repeatedly scattered along a (long) propagation path". I do not understand this. Signal is scattered at all tangent altitudes in the atmosphere. What causes the saturation and what defines the altitude range where it occurs?

As described by Kolmogorov theory, the scintillation index (rms of relative intensity fluctuations) depends on the refractive index structure constant (refractive index fluctuations), the wavenumber (wavelength) and the atmospheric path length. In regions leading to strong scintillations (i.e., regions with strong refractive index fluctuations) or for long paths, the scintillations can reach the saturation level. This means that the scintillation index approaches unity and stays more or less unchanged, even if the refractive index fluctuations become stronger and stronger or the path length gets longer and longer. Hence the altitude range where saturation occurs depends on the observation geometry, the refractive index fluctuations and the wavelength.

We have now improved the sentence to express this more directly as follows, including a new reference providing further detail:

"This saturation level depends on the strength of the refractive index fluctuations, the atmospheric path length, and the wavelength of the signal (e.g., Andrews and Phillips, 2005)." (and the new ref was added in the References section)

8) P.2720: "scintillation noise" – Scintillation is not noise.

We corrected this by replacing "scintillation noise" by "scintillations".

9) P.2720: I suggest including also the references on laser occultation experiments (in addition to or instead of the reference on stellar scintillation measurements) and discuss applicability of these observations to ACCURATE mission.

> *• Takayama, Y., T. Jono, Y. Koyama, N. Kura, K. Shiratama, B. Demelenne, Z. Sodnik, A. Bird, and K. Arai (2007), Observation of atmospheric influence on OICETS inter-orbit laser communication demonstrations, Proc. SPIE 6709, 67091B*
> *• Löscher, A.: Atmospheric influence on a laser beam observed on the OICETS – ARTEMIS communication demonstration link, Atmos. Meas. Tech., 3, 1233-1239, doi:10.5194/amt-3-1233-2010, 2010.*

Thanks for this suggestion, we are aware of these papers and we considered this. But we decided not to go into more detail in this introductory paper with the similarities and differences to optical laser communication experiments. The reasons are: 1) opt. comm. systems typically face quite different difficulties with scintillation than LIO. For example, opt. comm. systems employ very narrow beams (order 10 µrad), which leads to challenges by beam wander. This is typically negligible for LIO because comparatively large beam divergences are used (order 1 mrad); 2) scintillations are only a side topic in this paper but a separate dedicated paper on scintillations is scheduled by Sofieva et al. That paper will certainly take into account the Takayama et al. (2007) and Loescher (2010) papers and address differences and similarities of opt. comm. and LIO.

10) Fig.8a: I do not see dashed lines.
There is no vertical line here at 0.25 dB as in the previous Figures, since Rayleigh scattering loss is so small (as discussed in the text) that the whole x-axis range reaches to 0.2 dB only. To help avoid that this small x-axis range is "overlooked" compared to Fig. 7, we have enhanced the relevant sentence in the Fig. 8 caption as follows:
"The layout of the panels is the same as in Fig. 7 (the x-axis ranges are smaller)."

We thank Referee#2 very much again for the careful review of the paper and for his/her helpful comments.