

Interactive comment on the AMT paper from S. Ishii et al.: «*Ground-based integrated path coherent differential absorption lidar measurement of CO₂: hard target return*»

The authors report an interesting study on coherent DIAL and IPDA measurements of atmospheric CO₂ in the region of Tokyo, Japan.

The present paper describes a comparison between CO₂ lidar measurements using two different methods: 1) DIAL measurements using atmospheric reflectivity and the slope method 2) IPDA measurements using a «hard target». Given that the authors already published two papers concerning coherent DIAL measurements of CO₂ using atmospheric reflectivity (Appl. Opt. 2010, JTECH, 2012), the reviewer suggests that the present paper is more focused on IPDA measurements as this is the only new aspect here and as it is written in the title of the paper.

The main issue of the present paper is the non definition and sufficient analysis of the «hard target» and the effect in coherent signal. Also the specific issue of IPDA CO₂ measurements are not sufficiently developed (especially instrumental calibration issue). This should be corrected in the revised paper. The reviewer assumes in his comments that the hard target consists in a mix of trees and ground surface as «swaying branches» is wrote in section 5.

Specific comments:

Abstract:

l. 11-12: «precision of 1-2 ppm». For what kind of application such a precision is needed? This will surely explain why a high PRF laser system is then needed for such measurements.

l. 15: «was about 5 ppm lower»: if the authors want to address an accuracy better than 1%, the accuracy of spectroscopic data and their fluctuations with meteorological data should be addressed.

l. 17: «no differences». I guess that the authors mean «no biases»

l. 23-25: «simultaneously conduct both hard target and atmospheric return measurements». This should not be a recommended objective of CO₂ lidar measurements as IPDA measurements are generally not used when range-resolved measurements are available. The reason for simultaneous IPDA and DIAL measurements should be more developed in the paper. Any IPDA calibration issue?

Introduction

l. 27: «it tends to overestimate the optical depth of aerosols...» This is not the optical depth of aerosols or clouds which is concerned but more the optical depth due to CO₂ absorption and the error due to aerosols and clouds.

Aerosols and clouds entails a reduction of the total integrated path of CO₂ measurements for passive sensors. In this way the total optical depth due to CO₂ absorption is underestimated and regional biases may occur.

l. 5 p. 8582 «is not affected»: unlike passive sensors, aerosols and clouds don't produce a reduction of optical depth for IPDA measurements. However, due to their associated extinction effect on the laser beam, they still affect the signal to noise ratio.

l. 26 p. 8582 «Gilbert et al.» please remove the «l» (Gibert et al.)

l. 3 p. 8583: «hard target» the authors may be careful using the term of hard target.

A definition of hard target is definitively needed in this paper. Some dense clouds can also be considered as hard target as only one temporal speckle will be seen in a temporal range gate. Also the surface might not be considered as a hard target as some propagation of the laser beam is still possible through the canopy.

2. Coherent 2- μ m differential absorption and wind lidar

3. Estimation of CO₂ and error analysis

l. 5 p. 8587: «temporal cross correlation coefficient as 0»: the authors should consider that the correlation coefficients are different for atmospheric and ground target return power signals. This might have an impact on the precision of the IPDA measurements comparing to the DIAL measurements (see for example the papers from Killinger et al, 1981, 1983 and others). This should be addressed or at least mentioned.

4. Ground-based in-situ measurements

l. 5: «a total error of 0.1% in the CO₂...DIAL measurements»: although the R30 CO₂ absorption line is rather insensitive to temperature fluctuations, one can claim that the fluctuations of temperature in the surface layer are larger than 1°C due to the heterogeneous radiative properties of the surface (building, river, forest, altitude variations) over 7 km as mentioned by the authors. This should be discussed by the authors.

5. Experimental hard target measurements

l. 21 «hard target surface»: please define what is this «hard target»: ground surface, forest, building... later, the authors wrote «swaying branches» so the reviewer assumes that the target consist a mix of trees and ground surface..

Fig. 2c: the reviewer thinks that Fig. 2c does not give more information than Fig. 2a. Instead, a similar zoom on the emitted and the reflected pulses should be shown. Please make a zoom in Fig. 2b and a similar zoom for the reflected pulse in Fig. 2c so that we will have some information about the change in the shape of the return pulse. This will help to characterize the «hard target».

l. 4 p. 8589 and fig. 3a and b: «speckle induced fluctuations»: how is calculated the range using the hard target return signal? any fit of the data?

Fig. 3c: Fig. 3c seems to show two different modes in the detection of the range of the hard target. This should indicate that the «hard target» is a mix of trees and ground surface. Please discuss that in the paper.

l. 23-25: please clarify

Fig. 5a: Fig. 5a shows that a negative optical depth is obtained at a range of 0 which raises the issue of calibration of absolute measurement of optical depth. This calibration is necessary to obtain accurate optical depth and CO₂ mixing ratio measurements with the IPDA method. Gibert et al. (JTECH 2008) used the DIAL technique and the slope method in order to correct instrumental biases in IPDA optical depth measurements in free troposphere clouds.

Please comment on this calibration issue and develop in this paper how accurate IPDA measurements of CO₂ are made here.

- l. 27-29: «The DAOD... 7 km»: please remove this sentence. The next sentence means the same thing and is more clear to the reviewer.
- l. 16 p. 8590: for a pure hard target, the reviewer expects to have a PDF in negative exponential and not a lognormal law. The reviewer suspects that two speckles are obtained due to the characteristics of the hard target (mix of trees and ground)
- l. 17: «the calculated Nc for atmospheric return». This depends not only on the pulse width but also on the turbulence in the atmosphere. The reviewer suggests that the authors make an experimental PDF calculation for atmospheric return with their data and compare it with the one of the hard target. Also, some theoretical considerations for Nc might be welcome here.
- l. 20: please clarify and rewrite the sentence
- l. 27: please indicate the duration of measurements that is considered here and for what kind of application.