Response to the review comments of the anonymous referee#2

We would like to thank the referees for their constructive reviews. We accounted for each of the comments by either modifying the manuscript or, by arguing and explaining our choice. We give below answers to each comment and chose to write them in italic characters, first after the general comment, then after the specific comments.

Review of "Improved information about the vertical location and extent of cloud layers from POLDER3 measurements in the oxygen A band" by Desmons et al.

General comments

The paper addresses to relevant scientific questions within the scope of AMT. The paper contains significant original material. The authors parameterize the differences between the POLDER retrieved cloud pressure and CloudSat/CALIPSO cloud midpoint and top pressures as a function of cloud optical depth and solar zenith angle. The parameterization improves the POLDER retrievals by noticeably reducing biases and slightly reducing RMS. A similar parameterization is proposed to relate the angular standard deviation of POLDER-retrieved cloud pressure and the cloud vertical extent from CloudSat.

The title is misleading because it can be interpreted as the paper deals with multilayer clouds. Actually, the paper considers single-layer clouds only.

The title has been modified. The word "monolayer" has been added.

The abstract does not summarize the paper properly.

It seems that reviewer#2 did the review of a former version of the submitted paper (February, 7th, 2013)). It explains that the line numbers given by reviewer#2 don't match with the accepted manuscript lines. The manuscript was actually accepted with corrections following suggestions of reviewer#1. A new version was uploaded the 7th of March. We don't think that this misunderstanding is dramatic. The main correction concerned the abstract that we made longer : it contains more quantitative results. We observe that reviewer#1 did not comment the new version of the abstract.

The standard of English usage is not satisfactory.
Sometimes, it is hard to understand what exactly the authors want to say. Spelling and syntax errors are too many to be listed. The paper should be shortened by reducing a number of figures (see the specific comments).

We acknowledge that the first version of the manuscript contained too many errors of english language and style. We corrected them with care.
We also globally improve the manuscript to make it more concise and clear. We did it by rewriting in particular section 2 in order to avoid redundancies, and by removing some figures.

Symbols used throughout the manuscript were redefined and clarified. For exemple the cloud geometrical thickness is denoted by h, while H means the CPR/CALIOP cloud geometrical thickness. We made it clear that P_O2 means with no ambiguity the angular average of POLDER directional oxygen pressures.
We chose to remove the figures that were not necessary. Thus, we removed two figures (Figure 11b and 15) and replaced one (Figure 10) by another one. We no more show the variability of the slope of the linear regression between sigmaP_O2 and H, but only the spatial variability (with histogram of values) and temporal evolution of the correlation coefficient. We think these changes make the paper easier to read and clearer.

However, we chose to keep Figure17 although reviewer#2 suggested to remove it. We not only decided to keep this figure but to add another panel showing the CTP-H diagram for liquid cloud over oceans. This figure shows that while our results are preliminary and can certainly be improved, we already obtain from a passive sensor some climatological feature about cloud covers we think are interesting, with informations about their vertical occurrence, which is new.

Concerning the demand of the two reviewers to add color scale for 2D plots, we chose not to follow the recommandation. We think that it would not add a very valuable information as we don't use quantitatively these plots but qualitatively. We added some texts to better explain the figures.

The authors claim in Section 4 (subsection titles and elsewhere in the text) and Conclusions (Line 510) that they get unbiased estimates of cloud top and midpoint pressures. That is not true. Figures 6 and 8 clearly show the presence of significant biases. For stratocumulus, the cloud midpoint pressure biases can be as large as 264 hPa and the cloud top pressure biases are up to 281 hPa.

We followed the reviewer comment and decided to not use the adjective "unbiased", but only "estimates". While the estimates of the inferred pressures appear unbiased for the whole cloud population (the mean of the difference (CTP_inv – CTP) is close to zero; the same for CMP), it is true that it is not unbiased by class of ISCCP clouds. The bias given on Figure 6 and 8 prove it.

Specific comments

Title and elsewhere: A hyphen in the A-band is more common in the literature.

We followed some important bibliographical references that don't use hyphen , like Koelemeijer et al, (JGR, 2001), Fischer et al (1991, JAM), O'Brien and Mitchell (1992, JAM), so we make the choice to keep this form.

Introduction: Lines 44-45; 74-76. The sentences are hard to be understood.

We clarified.

Section 2.1: Line 103. Does "a perfect reflector" mean a Lambertian surface with albedo of unity? If yes, why surface albedo of unity is assumed? Why not 0.8, as in Koelemeijer et al. (2001).

Indeed, a "perfect reflector" means a cloud albedo equal to unity. P_O2 is an operational product that comes from a modeling in which the scattering within clouds is not accounted for because not known. POLDER scientific team was very much more aware of this assumption. We prove here that thanks to this crude hypothesis, we can have the ambition to get an estimate of CTP and more from POLDER A band measurements because of the sensitivity of POLDER angular oxygen pressures to

cloud geometrical thickness.

Section 2.1: Line 123. “cloud pressure value affected to a super-pixel . . .”. What does it mean?

The oxygen pressure P_O2 is a Level-2 POLDER Product. Level-2 products are derived from Level-1 measurements at POLDER native resolution. They are given at a spatial resolution close to 18.5 km x 18.5 km (1/6 deg. x 1/6 deg.), i.e. 3 x 3 pixels of the Level-1 grid. This is why they are called "super-pixel".

Section 2.1: Line 128. “cloud fraction”. How is it derived from POLDER measurements ?

Cloud fraction are defined at the super-pixel resolution from the classification of each of the 9 pixels that it contains. POLDER pixels are classified cloudy or cloud-free thanks to a series of sequential tests applied to each individual pixel and for every viewing directions. Four tests aim at detecting clouds while two additional ones are used to identify cloud-free pixels : one uses the R_{763}/R_{765} ratio as indicator of the cloud contamination; one from the level of the reflectance at 865 nm; two tests based on polarization at 443 nm and at 865 nm are applied; two test are based on threshold applied to reflectances at 865 nm and 670 nm, and applied to the R_{865}/R_{670} ratio.

Classification between cloud-filled and cloud-free pixels is then followed by the derivation of the cloud cover of each super-pixel. First the cloud amount is determined direction by direction and then the averaged cloudiness is computed. The Cloud Cover is defined as the number of cloudy pixels divided by the total number of pixels.

cf. Buriez, J., Vanbauce, C., Parol, F., Goloub, P., Herman, M., Bonnel, B., Fouquart, Y, Couvet, P and Sèze, G.: Cloud detection and derivation of cloud properties from POLDER, Int. J. Remote. Sens., 18, 2785-2813, 1997.

Section 3: Line 180. “at a horizontal resolution of 5 km”. A POLDER pixel size is different from this value of 5 km. Please provide some detail of how the collocation of POLDER pixels to this spatial resolution was performed.

We clarified in this section the notions of horizontal resolution and of spatial sampling. The collocation of POLDER, MODIS and CloudSat pixels to CALIOP ones was realized with the nearest pixel approximation to the CALIOP lidar shots every 5 km. For POLDER information, it comes from the super-pixel that contains the lidar shot.
A detailed description of this dataset together with its creation strategy can be found at the ICARE website: http://www.icare.univ-lille1.fr/projects/caltxtract/.

Section 3: Line 196. How was the data filtering carried out to select single layer clouds?

We selected single layer clouds thanks to the dataset "number of cloud layers n" from the CPR/CALIOP 2B-GEOPROF-LIDAR.V04 product.

Section 3: Line 202. “Thanks to a deeper sensitivity study, . . .”. Please reference this “deeper study”.

We changed the text.

Section 3: Lines 210-215. How do those results compare with data reported by Joiner

et al. “Detection of multi-layer and vertically extended clouds using A-train sensors”, AMT, 2010. A reference to this paper should be obviously added.

In their paper, Joiner et al present a method to detect multi-layer and vertically extended clouds with passive sensors. It is not what we intend to do it. More over, the dataset used by Joiner et al covers at the maximum a period of one month. We think that it is thus difficult to compare the climatological feature that we provide here with the one given in this paper. We chose not to cite this paper.

Section 4: Fist sentence. Please reword to clarify.

Sections 4.1 and 4.2. See the general comments.

Section 4.2: Line 331. “. . . statistically not so far from . . .”. This is quite subjective statement and should be avoided.

We clarified the text and corrected some expressions.

Section 5: Line 344. “the scene’s geometrical conditions”. What does it mean?

It means the viewing angle of the instrument, it has been changed in the manuscript.

Section 5.1: Line 353. “in order to optimize the correlation”. In what sense?

We chose the width of the bins in order to make the correlation coefficient the highest.

Section 5.1: Line 359. The correlation coefficient has been denoted as “ro” (see Fig. 6 & 8).

We denoted the correlation coefficient by "r" in the entire article.

Section 5.1. Negative and low values of the correlation coefficient in Fig. 10 which are not discussed and explained in the text. Figure 10 can be taken out.

This figure 10 has been removed.

Section 6: Line 431. “Results are syntheized in Table 2”. Maybe “summarized”?

Done.

Section 6: Table 2. No values of the mean cloud vertical extent are provided in Table 2. It would be interesting to compare the standard deviation of vertical extent estimates with the mean cloud vertical extent.

We don't give values of the mean cloud vertical extent because this parameter is varying over a broad range of values. An average would smooth the variations, that's why we show the mean differences.

Section 6: A general question: how often the POLDER-retrieved cloud bottom pressure appears to be higher than the surface pressure?

Section 6: Line 461 & 463. “ deltaH = 1365 m . . . standard deviation . . . close to 5000 m”. Do the retrievals with so high biases and standard deviation make sense?

These two last comments are interesting. While it would be very interesting to study this parameter, the retrieval of the cloud bottom pressure (CBP) was not one of our goals. So we cannot discuss the accuracy or the significance of its retrieval from the dataset than we produced. We would have to obtain a particular parameterization for the CBP retrieval, which is an interesting idea. Moreover, we haven't use the surface pressure and it would be a consequent work to include it now in our study.
We agree that if we consider the mean cloud extent (1000 m for liquid clouds and 9000 m for ice clouds) and the mean cloud top pressure (950 hPa for liquid clouds and 250 hPa for ice clouds), we can expect some cases for which the bottom pressure is higher than the surface pressure but it wouldn't be most case : we obtain DeltaH=1365+/-5000 m: these values are not so high compared to the mean value of H for ice clouds (9000 m).

Section 6: Line 483. “As for cloud top pressure estimates, we compute the score obtained by the estimate of H.” Confusing, please reword.

We reworded.

Conclusions: Line 510. “. . . (CMOP) which are unbiased estimates . . .”. See the general comments.

Conclusions: Lines 514 & 517. “results are very interesting” & “estimates are interest-ing” Interesting conclusions!

We thank the reviewer for these comments. As said above, we don't use the adjective “unbiased” anymore, and we took more care to let the reader make his own opinion about the interest of our results.

Conclusions: Lines 530 & 531. “. . . ten parameterizations for liquid water clouds over ocean and six over land . . .”. Those parameterizations were never specified in the text. Maybe it is not so important for a potential reader to learn how many parameterizations were proposed.

The parameterizations were given in section 5.3 and illustrated in Figure 12. We chose to keep this information in section 5.3, but to withdraw it in the Conclusion where it is not necessary.

Conclusions: Fig. 17. Please clarify what exactly this figure adds to the conclusions. Please consider removing this figure.

We decided to keep it. See our answer after the general comments.

Figures 4, 6, and 8. The color scale is not specified.

We answer this comment in our general answer. We chose to not add color scales. We think that it would not add a very valuable information as we don't use quantitatively these plots but qualitatively. We added some texts to better explain the figures and simplify their reading.