Atmos. Chem. Phys. Discuss., 7, S9703–S9714, 2008
www.atmos-chem-phys-discuss.net/7/S9703/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measurement of the water vapour vertical profile and of the Earth's outgoing far infrared flux" *by* L. Palchetti et al.

L. Palchetti et al.

Received and published: 28 March 2008

Reply to second review, Anonymous Referee 1

GENERAL REVIEWER COMMENT

Still I see a major problem in publishing this manuscript in ACPD, primarily because the authors apparently misuse this journal (intentionally for publications in atmospheric science) for mostly technical stuff that could be published elsewhere. I base my statement on the refusal of the authors to draw any conclusion from their study potentially relevant for the atmospheric science community. In the second review I leave all comments from the first review to which the authors did not sufficiently react. In total there are 10 new comments.

REPLY

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Interactive Discussion
Discussion Paper

The Reviewer often makes disparaging remarks rather than constructive comments. Because of this, it is sometimes rather difficult to identify the desirable editing. Fortunately useful suggestions came from other Reviewers and the overall upgrades that have been performed may meet the requests of this Reviewer.

Therefore, we decided to reorganise the final part of section 4 where the data analysis is discussed. The new part is:

"The REFIR-PAD profiles retrieved during the flight are compared with the ECMWF correlative data. The comparisons are shown in Fig. 5 for temperature and in Fig. 6 for water vapour. The differences in temperature were generally small: namely, they seldom exceeded 2%. On the other hand for the water vapour VMR, as shown in Fig. 6, the retrieved profiles are characterised by an upper troposphere that is drier, by more than a factor 2, than ECMWF estimates. The differences observed at the beginning of the flight, for sequence numbers equal to 13, 14 and 22, are probably due to a pixel contamination produced by the presence of clouds.

The time series of the retrieved temperature values for the lower troposphere can be seen in Fig. 8. A skin BT increment due to solar irradiation is detected, starting from the sunrise occurring at sequence #19. A small increase in temperature is also observed in the lowest layers of the atmosphere.

Apart from the warming of the lowest atmospheric layers, the other retrieved values do not show a detectable trend, so that an average profile can be calculated for the atmosphere observed during the flight. Table 2 and 3 show the average retrieved profiles of temperature and water vapour, respectively, together with the standard deviation of the ensemble and the average retrieval error. The latter two quantities are in good agreement, even if larger values are observed for the standard deviation at low altitudes and for the retrieval error at high altitudes. We note that, while the random measurement errors contribute to both error estimates, the atmospheric variability contributes only to the standard deviation and the systematic calibration error contributes only to the

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retrieval error. This suggests that superimposed to the random errors, for which the two error estimates provide consistent evaluations, we may be observing the effects of atmospheric variability at low altitudes and of systematic calibration errors at high altitudes.

The measurement error that has been verified by this procedure can be compared with the difference between measured and ECMWF profiles, given in the fifth column. In order to facilitate the comparison, the ratio between the values of the fifth column and the largest between the values given in the third and fourth columns is given in the sixth column. Since the measurement error is the main source of uncertainty, the largest of the two errors is an approximate, but reasonable estimate of the overall error budget that includes measurement error, calibration error and atmospheric variability. The qualitative comparison made in Fig. 1 and Fig. 2 can now be discussed in a more quantitative manner.

TABLE 2, Average profile of temperature and errors.

Altitude [km]	Mean Temp. [K]	Retrieval Err. [K]	Std.Dev. [K]	REFIR-ECMWF [K]	REFIR-ECMWF Max.Err.
33	236.0	1.9	0.8	2.6	1.4
27	222.7	2.0	1.6	0.2	0.1
21	207.3	2.7	1.5	2.1	0.8
17	193.3	2.7	2.1	-2.5	-0.9
13	209.9	2.9	1.7	-4.4	-1.5
11	228.3	2.9	2.7	-2.0	-0.7
9	246.5	2.7	2.3	-0.2	-0.1
7	255.5	2.2	2.2	-6.3	-2.9
5	279.6	2.0	1.8	5.6	2.8
3	283.6	1.8	2.5	-1.3	-0.5
1	287.4	1.9	2.6	-5.5	-2.1

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TABLE 3, Average profile of water vapour and errors.

Altitude [km]	Mean H_2O [ppmv]	Retrieval Err. [ppmv]	Std.Dev. [ppmv]	REFIR-ECMWF [ppmv]	REFIR-ECMWF Max.Err.
17	3.1E+00	1.2E+00	1.1E+00	-1.4E+00	-1.2
13	11.5E+00	3.8E+00	5.2E+00	-51.2E+00	-9.8
11	9.4E+01	1.9E+01	1.2E+01	4.3E+01	2.3
9	9.3E+01	5.7E+01	4.6E+01	-6.5E+01	-1.1
7	30.3E+01	11.1E+01	8.7E+01	-17.2E+01	-1.6
5	13.2E+02	2.0E+02	2.9E+02	-5.9E+02	-2.0
3	7.8E+03	1.4E+03	2.3E+03	2.4E+03	1.0
1	14.2E+03	2.8E+03	3.2E+03	-2.9E+03	-0.9

In the case of the temperature profile consistent results are observed. Indeed the differences are only marginally larger than the error of the retrieved profile and can be explained by the following external errors: ECMWF errors (estimated to vary between 1 and 3 K), spectroscopic errors (estimated to be equal to a few percents), and smoothing errors due to the discrete sampling of a distribution with a vertical variability.

In the case of the water vapour profile different considerations apply at different altitudes. In the lower atmosphere (at 11 km and below) consistent results are observed and the few large differences can be explained by the smoothing error. Indeed for water vapour, because of its large vertical variations, the smoothing error is expected to have more pronounced effects than for temperature. On the other hand, a large disagreement is observed at 13 km. In order to explain this disagreement, the effect of cirrus clouds, water vapour continuum and spectroscopic errors have been investigated.

When cirrus clouds were included in our atmospheric model, the retrieved column of ice particles turned out to have an average value throughout the flight of about 1 μg/cm^2 with an r.m.s. of 16 μg/cm^2. No detectable change was observed in the retrieved water vapour and temperature profiles when cirrus clouds are fitted. Therefore, the error

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introduced by the assumption of no cirrus clouds is negligible.

A stringent validation is lacking for the water vapour continuum absorption model, however the Jacobian calculations indicate that this quantity does mainly influence the retrieval of water vapour below 7 km where smaller discrepancies are observed. The retrieved value at 13 km directly depends on the spectroscopy of water vapour; however the spectroscopy cannot account for a difference as large as the one observed. Also considering that the chi-square test provided values that varied between 0.9 and 1.5, we conclude that no evidence can be found in our measurements for a significant unaccounted systematic error that can explain the discrepancy observed at 13 km. This suggest the possibility of an over estimate of water vapour in ECMWF model at high altitudes.

The agreement between observation and retrieval model, that is suggested by the chi-square test, is confirmed by the behaviour of the residuals. In Fig. 7, we report the mean values of the residuals of the fitting process (red line) compared with the mean value of the measurement error over the flight. The residuals are generally well within the mean measurement error, proving that systematic errors give a negligible effect in the fitting procedure of a single spectrum. The isolated exceptions of the peaks at around 460 cm^{-1} and 590 cm^{-1} are due to the non-fitted concentrations of, respectively, HNO_3 and N_2O which were assumed to be equal to the climatological value (Bianchini et al., 2007)."

1. REVIEWER COMMENT

Authors reply: a) We believe that we have reported the evidence about the good quality of our measurements. Probably further statements in this direction do not improve the science. b) This evidence suggests a possible shortcoming of ECMWF data, but it cannot be our task to discuss ECMWF data; c) Unfortunately our measurement, which has the merit of being a completely new measurement, because of its novelty does not have the statistic that can be used to characterise the ECMWF artefact. In conclusion,

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we believe that strong facts have been presented and further speculations would not improve the scientific content. FIRST NEW COMMENT FROM THE REVIEWER: If I assume that everything you are stating above is correct, then it is even more questionable why you are not comparing modelled spectra using the best input data available (e.g. a measured T profile) with measured spectra to demonstrate the quality of your measurement.

REPLY

The quality of our measurement has already been demonstrated in a previous paper (L.Palchetti et al., Atm.Chem.Phys. 6, 5025-5030,2006) where calibrated spectra measured by REFIR-PAD were compared, in the spectral region of overlap, with those of the well-characterised IASI-balloon instrument. The comparison showed that the residual difference is within the instrument noise that we have used for the forward model analysis.

As far as the comparison with measured profiles is concerned, see reply to comment 2.

As far as the comparison with forward model calculations is concerned, see reply to comment 4.

2. REVIEWER COMMENT

PREVIOUS REVIEWER COMMENT: Furthermore I have more specific questions: - Why are the inferred T-profiles and humidity profile not being intercompared with corresponding profiles measured on-site by meteorological sondes? OUR REPLY: Operative radiosonde measurements exist and could be included in Fig. 1 and 2. However, there is not a good coincidence in time and space with these measurements so that only a qualitative comparison can be made. For this reason, ECMWF, which includes the assimilation of these radiosondes, has been used for a quantitative comparison. SECOND NEW COMMENT FROM THE REVIEWER: A very weak argument consid-

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ering the small day-to-day variability of the meteorology in the tropics, and the small diurnal variation of in temperatures above the boundary layer in the tropics. Your refusal of using real measured data, e.g. the measured T and H2O profile, and to draw any scientific conclusion from your measurements relevant for the readership of this journal cast doubts whether you submitted the paper to the right journal.

REPLY

As already announced in our previous reply, radiosonde measurements have been included in Fig.1 and 2. Accordingly the figures are now presented in the revised text with the following comment:

" The temperature and the water vapour profiles measured by REFIR-PAD around noon UTC are compared with existing nearby radiosonde measurements and ECMWF estimates in Fig. 1 and Fig. 2, respectively. In the case of temperature the radiosondes provide a consistent set of measurements that is well reproduced by ECMWF estimates. The temperature profile obtained by REFIR-PAD is in reasonable agreement with the ECMWF estimates. On the other hand in the case of water vapour the radiosondes provide oscillating profiles that are limited to the altitude range 0–10 km. In this altitude range the ECMWF estimate agrees with either the average or the largest values of radiosonde measurements. Above 10 km no radiosonde measurements exist for a comparison with ECMWF estimates. The water vapour profile obtained by REFIR-PAD is in reasonable agreement with the ECMWF estimates below 10 km, but differs significantly from it at higher altitudes. Since ECMWF provides a representation of the atmospheric state that is more complete than that provided by radiosounding, the former will be considered in the subsequent analysis."

3. REVIEWER COMMENT

REVIEWER COMMENT: - What are the impacts on (sub-visible) cirrus clouds frequently found in the tropics on the reported measurements? OUR REPLY: Cirrus clouds was one of the objectives of our measurement, but no evidence was found of

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cirrus during the flight. THIRD NEW COMMENT FROM THE REVIEWER: This finding is astonishing since all optical remote sensing instruments deployed at the Teresina campaign have indication that sub-visible clouds in the TTL may have been present during June 2005. So how you came to your conclusion that 8216;no evidence was found of cirrus during the flight8217; ? What is the sensitivity of your instrument for light emitted by cirrus clouds ? For subvisible clouds in the tropics e.g. see Popp et al., ACP, 6, 601 - 611, 2006. I admit, however, that instruments operated at lower wavelengths than your instrument are more prone for sub-visible cloud detection. In return, your statement that you have no evidences for sub-visible clouds but the fact that any similar statement is missing in the manuscript can again be regarded as indication that you are not trying to sell any science in your paper, which could be of interest to the readers. So please answer the following question: Why anyone else than your research group and probably your funding agency should read the paper ?

REPLY

The sensitivity of our instrument and an upper limit to the vertical columns of ice have been quantified. See text in the general comment reply.

4. REVIEWER COMMENT

REVIEWER COMMENT: Minor comments: 1.) In order for any reader to get a flavour on the quality of the measured and modelled spectra, I miss a Figure where both type of spectra are plotted on the same scale (and probably shifted by a certain constant offset) for bare eye inspection. OUR REPLY Measured and modelled spectra are shown in other referred papers (Palchetti et al., 2006 and Bianchini et al., 2007). A quantitative assessment of the quality of measured and modelled spectra is given by Fig. 7. FOURTH NEW COMMENT FROM THE REVIEWER: Again you the stand-alone criteria of any scientific manuscript would largely benefit from including such a Figure.

REPLY

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A figure with both types of spectra plus their difference will be added in Sect. 3.2

5. REVIEWER COMMENT

REVIEWER COMMENT: 2.) At many places, the English does not meet the standard required for a scientific publication. For example, the manuscript contains many sentences that are too long to be understood, and other shortcomings (typos, usage of wrong words, et cetera8230;). Therefore I largely recommend proofreading of the manuscript by a native English speaker before resubmitting. OUR REPLY Proofreading by native English speaker will be performed before the final submission. FIFTH NEW COMMENT FROM THE REVIEWER: This statement is a tall order to anyone, e.g. the Reviewers who tries to make sense and to judge on your manuscript. So I demand to polish the manuscript before resubmitting it.

REPLY

We are even, because also this Reviewer does not write a clear and correct English.

6. REVIEWER COMMENT

REVIEWER COMMENT: 3.) In equation (1), the l-dependence is missing ! OUR REPLY We do not understand this comment. SIXTH NEW COMMENT FROM THE REVIEWER: Unfortunately the ACPD word processor did not recognize the original lambda but put an l instead into the text ! So the comment reads: In equation (1), the lambdadependence is missing!

REPLY

The lambda dependence is not missing in Eq. 1; we have defined the wavenumber (the reciprocal of lambda) and this is the quantity used in Eq. 1.

7. REVIEWER COMMENT

REVIEWER COMMENT: 6.) Citation from the paper: The Fig. 9 shows that the OLR flux differences in the FIR are in the range of 28211;3.5W/m2, larger for the warmer

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atmosphere. Problem 1: Larger as compared to what? OUR REPLY The statement will be modified into: -The Fig. 9 shows that the OLR flux differences in the FIR are in the range of 2- 3.5W/m2, where the largest difference is for the warmer atmosphere observed during the day- SEVENTHTH NEW COMMENT FROM THE REVIEWER: Warmer as compared to what (night ???). In an proper comparison (e.g., warmer) usually something is compared to something else (e.g. day vs night et cetera?) !

REPLY

Something of the ”copy and paste” procedure was lost. The right statement is: ”The Fig. 9 shows that the OLR flux differences in the FIR are in the range of 2-3.5W/m2, where the largest difference is for the warmer atmosphere observed during the daytime with respect to the nighttime”.

8. REVIEWER COMMENT

REVIEWER COMMENT: Problem 2: The sentence is in conflict c.f. with your statement on page 17750, c.f., Since the atmospheric state is sufficiently uniform in time and location along the flight, the retrieval standard error OUR REPLY The sentence on page 17750 addresses the question of whether the variation of the observed atmosphere is small enough to ensure linearity for the mean standard error calculation. This is not in contrast with the fact that the atmospheric variation is large enough for us to detect a change in the OLR flux. EIGHTH NEW COMMENT FROM THE REVIEWER: Come on, you turn the arguments around according to you wishes, e.g. why you do not use the same argument than when it comes to measured (rather than assimilated) temperature profiles?

REPLY

The comment of the Reviewer is not very clear. However we can try to provide a more quantitative reply.

At p. 17752 the statement is made that the FIR OLR flux varies by 1.5 W/cm2 when

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going from the beginning to the end of the flight. This change is 1.2% of the total FIR flux and is small enough to be consistent with the statement, made at p. 17750, that the atmospheric variability does not prevent the calculation of the average profile.

However, also in the light of the next comment of the Reviewer, the statement made at p. 17750 (lines 18-23) can be improved with the addition of a table and with a more detailed discussion as it has been done in the new text reported in the reply to the general comment.

9. REVIEWER COMMENT

REVIEWER COMMENT: 7.) page 17750: Citation from the paper: This allows to consider the mean standard error of the mean measurement, which resulted to be less than 0.5 K for temperature mean profile, and about 38211;5 OUR REPLY The asked question is missing in this comment. NINTH NEW COMMENT FROM THE REVIEWER: Here comes again the comment (which I found in my original review): It is impossible to understand the essence of this sentence.

REPLY

In the sentence the random errors of the mean profile were quoted. However, the new discussion (made in the revised text and shown in the reply to the general comment) is now focused on the single measurement rather than on the average. Therefore, these errors are no longer quoted.

10. REVIEWER COMMENT

REVIEWER COMMENT: 13.) Conclusion: I see no particular reason to stress that the measured and modeled outgoing radiative fluxes depart by 3.5 W/m2 and8230;.. that is comparable to or even greater than the estimation of the radiative forcing of the CO2 increases since pre-industrial time8230;.as long as it is not attempted to research on the potential reasons (see above). OUR REPLY As explained in our reply to the -Major comments-, in this paper we report new measurements that well agree with the model,

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but disagree with ECMWF data. No indication exists for unaccounted systematic errors in this new measurements, and the error budget indicates that the difference with ECMWF is larger than the measurement errors. All the -potential reasons- that can be ascribed to the new measurements have been investigated. On the basis of this investigation, the conclusion stresses the fact that the scientific understanding has not yet reached a consistent description of all the parameters related to the Earth radiation budget with an accuracy better than the forcing effects that we want to model. TENTH NEW COMMENT FROM THE REVIEWER: No ! Since you are using input data (from ECMWF - which are presumably far worse than you would probably need to explain your 8216;high quality measurements 8217;) how can you conclude to that statement 8216;All the -potential reasons- that can be ascribed to8217; This is very serious issue, because a reader can8217;t decide ad-hoc whether this statement is true or not, simply because you are comparing apples (your inferred T profile) with pears (the assimilated T profiles from ECMWF), with the result that a noticeable (and for science purposes relevant) discrepancy exist between measured/inferred and assimilated Ts?

REPLY

We may have not understood the Reviewer comment that uses the metaphor of incommensurable quantities (apples and pears) to indicate commensurable quantities derived from different observations and models. Furthermore, the Reviewer comment is focused on temperature discrepancies, while the main differences observed with REFIR-PAD are about water vapour.

However, if the criticism is that we do not demonstrate that all potential reasons of the observed differences have been investigated, in the new text, reported in the reply to the general comment, we provide more explicit statements.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 17741, 2007.

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