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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.

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REPLY TO REFEREE #1

REVIEWER COMMENT:

Overall merits:

The paper reports on mid to far-IR infrared spectroscopic measurements of the outgoing radiative flux in the tropics. From the measurements profiles of water vapor and temperature of the underlying atmosphere are inferred. The latter profiles are compared with corresponding analysis from ECMWF fields. The authors conclude that the major achievement of their study is (a) to have used an un-cooled detectors detector providing a peak-to-peak noise of ´s0.3 K and (b) that, the measured total radiative flux was found to agree by 2 8211; 3.5 W/m2 ´s 0.4 W/m2 with corresponding

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ECMWF predictions. While I find such measurements extremely important to monitor the impact of green-house gases on the radiative budget of the Earth atmosphere, I doubt that the authors put enough emphasis on demonstrating the scientific relevance of their study. I therefore recommend a major revision of the manuscript with more details given below.

Major comments:

(1) In the study the authors inter-compared inferred temperature and H2O profiles with corresponding ECMWF predictions with the results that systematic departures are found among both data sets, c.f., for T around in the UT and TTL and for water vapor around 10 km and 2.5 km. Several questions arise which should be addressed in each relevant scientific study: - What are the reasons for the systematic discrepancies (modeled vs measured)? - Are the discrepancies due o to deficits in the ECMWF data o to instrumental artifacts o or to deficits in water vapor line parameters, or in the continuum model? If feel that by including an investigation of potential reasons would much improve the scientific content of the present the manuscript.

OUR REPLY:

The questions asked by the reviewer are very pertinent and indeed are the underlying questions that motivated the paper.

The good agreement between measured and simulated spectra makes us certain that the measurement and the model are not affected by artefacts. Indeed we have experienced that small approximations in the model (ILS model, frequency calibration, different versions of the spectroscopic database, consistency between water vapour continuum and spectroscopy, CO2 line mixing) show up with great sensitivity as an increase of the chi-test value. One should consider that our capability of detecting instrument and model artefacts is enhanced by the very wide spectral interval that is fitted. Furthermore, a rigorous calculation of both random and systematic errors (based on the use of full variance covariance matrices) was made both in the fit and in the forward

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model ensuring a correct estimate of the errors affect the observed differences.

In the paper we underline that the chisquare test provided a value close to unity (page 17747, line 25) and that the average of the residuals are generally well within the measurement error (page 17750, line 10 and Fig.7). This last statement is also supported by reference to a paper (Bianchini et al. 2007) in which a detailed study of the residuals is made. We believe that we have presented the evidence that all the errors of our measurements are correctly modelled and no unaccounted effect of our analysis can be responsible for the observed difference with ECMWF. On the other hand, since ECMWF data have poor information for high altitude water vapour and are not made for the calculation of the Earth radiation budget, it is likely that we are observing a shortcoming of ECMWF data. However, it is not our wish to discuss measurements made by other organisations. Furthermore, as stated in the conclusions (a -measurement that is limited in time and space cannot be representative of a bias in ECMWF analysis-) from a single measurement it is not possible to assess whether the differences are systematic or random. This is indeed our motivation for making in the future further measurements at different latitudes and in different seasons.

Therefore:

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, we believe that strong facts have been presented and further speculations would not improve the scientific content.

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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.

REVIEWER COMMENT:

- Or, what is impact of various water continuum models (the different CKDs, Tipping et al., 8230;.) on the systematic departures found for H2O around 10 km and 2.5 km ?

OUR REPLY:

Errors in the model can cause systematic errors in the retrieved profile and in narrow band observations the effect may not be shown by the residuals. However in wideband measurements, it is very unlikely that a high correlation exists at all frequencies between the modelling error and the retrieved parameter. In Fig. 7 we have shown that the systematic effects present in the residuals of our fits are small compared to the measurement errors. There is therefore a good agreement of our observation with the adopted MT_CKD_1.2 model of the water vapour continuum. This is not a surprise given the semi-empirical nature of this model. Furthermore, the continuum at 10 km does not contribute to the observed signal and cannot be the cause of the observed difference at 10 km. This discussion will be added in the revised paper.

A systematic comparison with different continuum models was not made and can be the subject of another paper.

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REVIEWER COMMENT:

- Or, what is the reason to adopt a modified Voigt and Van Vleck-Weisskopf water line shapes?

OUR REPLY:

We used a Voigt line shape modified according to Van Vleck-Weisskopf because this is the most rigorous model of the convolution between collisional and temperature broadening. This is a correction that is important only at long wavelengths and is not relevant for all the fitted spectrum, however, given its small computational cost, it was used at all wavelengths.

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 cirrus during the flight.

REVIEWER COMMENT:

Major comment (2) Moreover I see no particular reasons to strongly argue in favor for or against using an un-cooled detector as long as the detector noise is much smaller than the photon electron shot-noise. In a scientific paper this can once be shown to be true, but as a fact does not particularly justify a scientific study to be relevant for a wider scientific community, and thus be worth to be published.

OUR REPLY:

A detector that operates at a temperature higher than that of the source cannot have a noise smaller than the shot-noise. Therefore, uncooled detectors have a much greater

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noise than cooled photon noise limited detectors.

As a matter of fact uncooled detectors are not used for the measurement of the atmospheric thermal emission and in particular for FIR measurements (apart from some historical measurements with the Golay-cell). It is important to inform the scientific community that with new pyroelectric components associated with broad band FTS spectroscopy it is possible to attain valuable information about the atmosphere. Furthermore, despite the relative large noise of the detectors it is important that the error in the calculation of the flux is not limited by detector noise.

The scientific conclusions of our paper do not depend on the use of uncooled detectors and we do not want to publish a paper in order to report the use of uncooled detectors. However, the advantages in terms of cost and operability that these detectors have make the feature worth of being reported and underlined.

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.

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

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speaker before resubmitting.

OUR REPLY

Proofreading by native English speaker will be performed before the final submission.

REVIEWER COMMENT:

3.) In equation (1), the l-dependence is missing !

OUR REPLY

We do not understand this comment.

REVIEWER COMMENT:

4.) Explain all acronyms (ECMWF, IPCC, .) and abbreviations (OLR, BT, TOA, NESR ?)

OUR REPLY

ECMWF (already explained, see page 17744, line 19), IPCC (it will be explained), OLR (already explained, see page 17743, line 3), BT (already explained, see page 17746, line 11), TOA (it will be removed), NESR (already explained, see page 17746, lines 2-3). We will also check all the other acronyms and abbreviations.

REVIEWER COMMENT:

5.) Citation from the paper: The FIR spectral region from 0 to 600 cm8722;1 is here considered in detail because in this spectral region new observations are obtained by REFIR-PAD and low altitude clouds have a negligible effect on the TOA radiance. This statement is certainly incorrect for all considered wavelengths!

OUR REPLY

The statement will be rewritten as:

-The spectral region from 0 to 600 cm-1 is here considered in detail because new

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observations are here obtained by REFIR-PAD, and it is less affected by low altitude clouds than the window region between 800 and 1000 cm-1. In practice no significant emission is observed below 4 km in the spectral region between 0 and 400 cm-1.-

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 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-

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 error8230;..

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.

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

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mean profile, and about 38211;5

OUR REPLY

The asked question is missing in this comment.

REVIEWER COMMENT:

8.) page 17744: In this contest, in June 2005 we performed 8230;..which contest (context) ?

OUR REPLY

Yes, this was a misspelling. No contest is involved, we meant context.

REVIEWER COMMENT:

9.) page 17744: The final reduced chi-square close to one indicates the agreement à A reduced chisquare close to unity indicates the agreement

OUR REPLY

The correction will be performed.

REVIEWER COMMENT:

10.) page 17748: rotovibrational band à change to rovibrational band

OUR REPLY

The correction will be performed.

REVIEWER COMMENT:

11.) page 17749: The skin BT is retrieved with an errore of about 0.4 K. à The skin BT is retrieved with an error of about 0.4 K.

OUR REPLY

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The correction will be performed.

REVIEWER COMMENT:

12.) page 17750: The evaluation of the OLR by using directional non-spectral measurements, such as satellite single view observations, is affected by an error due to the angular distribution model used for the calculation of the emission anisotropy factor in the radiance-to-flux conversion, see e.g. the ERBE and CERES experiments 8230; this sentence is far too complicated to be understood correctly !

OUR REPLY

This and the following sentences at page 17751 will be corrected as:

-The evaluation of the OLR by using non-spectral single view observations is affected by errors due to radiance-to-flux conversion. The angular distribution model that this kind of measurements use for the calculation of OLR, see e.g. ERBE (Earth Radiation Budget Experiment, Suttles et al., 1992) and CERES (Clouds and Earth's Radiant Energy System, Wielicki et al., 1996) experiments, is typically affected by an error of about 4.6W/m2 (Clerbaux et al., 2003).-

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, but disagree with ECMWF data. No indication exists for unaccounted systematic errors in this new measurements, and the

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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.

REVIEWER COMMENT:

11.) References: Bianchini et al., 2006:The reference for Bianchini et al., 2006 is incomplete.

OUR REPLY

The information will be added:

- Bianchini et al., 2006 is a paper published on -Proceedings of SPIE, Systems and Next-generation Satellites XII 6361, R. Meynart, S.P. Neeck, H.Shimoda, Eds.-.

REVIEWER COMMENT:

- European Commission 2000: I doubt that the European Commission is the first author of a final research report for which you received funding.

OUR REPLY

- We will ask the Editor; we may use the name of the Project P.I.

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