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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

Anonymous Referee #1

Received and published: 3 February 2008

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.

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, 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 modeled spectra using the best input data available (e.g. a measured T profile) with measured spectra to demonstrate the quality of your measurement.

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 considering 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.

REVIEWER COMMENT: - What are the impacts on (sub-visible) cirrus clouds fre-

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quently 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.

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 ‘no evidence was found of cirrus during the flight’ ? 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 ?

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.

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.

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 lambda-dependence is missing!

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-

SEVENTHTH NEW COMMENT FROM THE REVIEWER:

Warmer as compared to what (night ???). In an proper comparison (e.g., warmer)

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usually something is compared to something else (e.g. day vs night et cetera?) !

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?

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.

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

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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 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 ‘high quality measurements’) how can you conclude to that statement ‘All the -potential reasons- that can be ascribed to’

This is very serious issue, because a reader can’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?

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

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