**Atmospheric
Measurement
Techniques
Discussions**

Interactive comment on "Total column CO_2 measurements at Darwin, Australia – site description and calibration against in situ aircraft profiles" *by* N. M. Deutscher et al.

N. M. Deutscher et al.

nmd03@uow.edu.au

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Response to referees' comments – Deutscher et al., AMTD 3 pp989-1021

Firstly, we would like to thank the anonymous referees and Dr Yurganov for their comments.

Below are responses to the comments made by the referees:

Anonymous Referee 1

Comment: - long-term precision of the FTS measurements. Unfortunately, the paper is very sparse in showing actual data although the FTS instrument has been operative

since 2005 (p.994, l.2). I would be interested in a time series of CO_2 total column measurements that covers several months or years. This could be useful to illustrate the long-term precision and to highlight instrument problems (and their resolution) such as mirror degradation (p.995, l.19). A comparison with the collocated in-situ data time series (p.996, l.11) could help to emphasize the different (and complementary) nature of ground-based in-situ sampling and total column measurements.

Response: - we agree that showing a time series of the column FTS measurements is informative. As such we have included the first 14 months of X_{CO2} measurements. Unfortunately the precision and instrumental problems are not clear from these data, so we have also included a panel showing the time series of retrieved continuum levels, to show the effect of instrument problems on the signal level.

We have not included the in situ data time series as this is outside the realm of this paper – it was only mentioned to highlight the fact that these complementary measurements are being made. The comparison will be made in detail in a separate forthcoming paper.

Comment: - the airmass dependent correction. Appendix A describes an airmass dependent correction that is applied to the retrieved total column CO_2 data. The manuscript refers to this correction only very shortly (p.999, l.5) although accuracy is one of the key points of the paper. The correction is estimated to 1% (p.999, l.5) which is 10 times larger than the estimated precision 0.1% (p.999, l.24). Therefore, I recommend to add a discussion on the origin of the observed airmass dependent bias beyond a mere mentioning of "spectroscopic deficiencies". What about instrument related uncertainties such as uncertain knowledge of the ILS? What about a lightpath effect due to a wavelength-dependent shift of the apparent brightness center of the sun from the bright center towards the darker limb at high airmass? I am sure that the authors are aware of these and a lot of other potential causes. I would consider it very insightful to discuss these.

Response: - Every source of systematic error that we have investigated has an air-mass dependent component, with the exception of the integrated band strength. This includes spectroscopic problems (for example line widths, the neglect of line-mixing, in-consistencies in strength) and instrumental problems (zero level offsets, ILS errors due to misalignment, continuum curvature). While it would be preferable to find physics-based fixes for each error term, this is not currently feasible. We have therefore imple-mented an empirical correction that represents the cumulative effect of all the various airmass-dependent biases. The functional form of this correction is a cubic in (SZA − 45°) and it is valid up to SZAs of 80°. The similarity of the airmass-dependent correc-tion from site to site argues that the spectroscopic airmass-dependent terms are larger than the instrumental ones.

There is some further discussion on the correction in a paper recently accepted for publication that introduces the TCCON. We have included a reference to that paper [Wunch et al., 2010], and point out to the reviewer that possible factors contributing to this "airmass dependent bias" are already discussed in Appendix A. The lightpath effect suggested should ratio out with O_2 in the calculation of XCO_2.

The value of 1% larger at noon than sunrise/sunset (p.999, l.5) is of course variable, because of the variability of solar zenith angle at noon and hence the range of zenith angles throughout the day, so estimating this with higher precision is ill-founded.

In any case, applying this beta value results in a correction of -0.13% to XCO_2 at 0° and +0.42% at 80°, the range over which it is valid. An uncertainty of up to 10% in the estimation of beta therefore causes differences in XCO_2 comfortably below the 0.1% level. The between-site and time differences in beta are of the order of 10%.

We have included some further discussion of the airmass correction in the appendix, particularly with reference to the possible bias introduced by uncertainty in the calcula-tion of beta, whether this be a time variable or inter-site effect.

Comment: - the application of the FTS averaging kernel to the in-situ validation profile

(p.1003, l.12). The total column CO_2 data are inferred from the FTS observations by scaling a first guess CO_2 vertical profile. It is not obvious to me how one calculates and applies the averaging kernel for such a scaling retrieval. To my knowledge, this case is not covered by the standard references Rodgers, C., World Sci., 2000, and Rodgers, C. and Connor, B., J. Geophys. Res., 2003. A more explicit discussion on this, eg. By including the exact formulae how the averaging kernel is applied, could be enlightening to me and others.

Response: - The equations in Rodgers and Connor [2003] apply to all retrieval meth-ods. In fact, the second sentence of their abstract states: "We develop the methods required to do this, applicable to any kind of retrieval method, not only to optimal es-timators." The calculation and application of an averaging kernel for a profile scaling retrieval is the same as a full profile retrieval. One perturbs the vmr at a particular level and sees how the retrieved vmr changes as a result. The profile scaling retrieval is simply a regular profile retrieval with an infinitely strong inter-level smoothing con-straint. The results in a nice simplification. The rows of the resulting averaging kernel matrix all have the same shape, that of the a priori vmr profile, since any change to the retrieved profile is always a scaling of the a priori. Thus the NxN averaging kernel matrix only contains N pieces of information and can be reduced to a N-vector. This averaging kernel vector is what is illustrated in, for example, Washenfelder et al. [2006].

Finally, we point out that in the case of CO_2, where the a priori profile is probably better than 1% at all altitudes, the fidelity of the averaging kernel is not so important. This is because the error in the retrieved column is the product of: (1) the difference between the true and a priori vmr profiles; and (2) the deviation of the averaging kernel from 1. If the former is close to zero, this relaxes the accuracy to which the averaging kernel must be known. For other gases (e.g. N_2O, CH_4) which have large stratospheric uncertainties, however, the averaging kernel is more critical.

We have included a reference to Rodgers and Connor [2003] and stated that the av-eraging kernels are calculated and applied following that method, and included a brief

discussion.

Comment: - Further, I find the description of the airmass dependent correction in appendix A (p.1006) confusing and sometimes hard to follow due to the use of confusing notation ("y_i" vs "X_{CO2}", "$\theta''vs$"θ_i"). In particular, I wonder how equations (A1) and (A2) fit together. I might be missing the point here, but following from equation (A1), XcorrectedCO2 should be given by

$X_{corrected}$(CO2) = X_{CO2} - β x S(θ_i),

Which is not identical to equation (A2). Please clarify the manuscript. Is it possible to motivate from physics considerations the functional form of S(θ)?

Response: - This is a fair point. In the derivation of the airmass dependent coefficients, they are assumed to be additive so that the problem is linear in the retrieved quantities (Yi, α, β) and can be solved in one iteration. But during the correction of the data, the airmass correction is assumed to be multiplicative. Of course, with CO_2 increasing at just 0.5% per year, this inconsistency will not make a noticeable difference for another decade. By then, hopefully spectroscopic improvements will have rendered the airmass correction obsolete. But the reviewer is correct that there is an inconsistency, which should be addressed, no matter how small it may be numerically, because it is confusing. We have therefore replaced equation A2 with the one suggested by the reviewer.

The references to y have been updated to read XCO_2 (or similar), though this equivalence is already defined.

Unfortunately, as previously mentioned, the airmass dependent correction is purely empirical. We tried fitting the symmetric X_{CO2} variation as a linear function of airmass, as a quadratic, cubic and a quartic. The cubic gave the best fit, i.e. the smallest χ^2 in equation A1. We cannot think of any physical reason why this should be the case.

Comment: - p.991, l.24: Vertical transport modeling errors and their impact on inverse

estimates of CO_2 have been discussed in eg. Gerbig et al., ACP, 2008, which might be a reference to add. Besides referring to the advantage of total column measurements being little sensitive to vertical transport, the authors should mention the disadvantage that total column measurements are less sensitive to sources and sinks at the surface than in-situ sampling.

Response: - this recommendation has been acted upon. We included the reference to Gerbig et al, and mentioned the disadvantage.

Comment: - p992, l.11: The manuscript should distinguish between precision and biases. To my knowledge, the 2.5 ppm requirement refers to precision, systematic biases must be less than "a few tenths of a part per million" (eg. Chevallier, F. et al., J. Geophys. Res., 2006)

Response: Yes, this is a fair point. We have addressed this by including an extra statement emphasizing that the systematic biases must be small and referred to this paper, though we assume that it is in fact the Chavellier et al, 2007 JGR paper to which the reviewer refers.

Comment: - p.992, l.28: "The TCCON can provide these data." Given that there is only one tropical TCCON station operational, this statement seems questionable. How many future tropical stations are foreseen?

Response: It seems our intention here has been misread. We are not trying to suggest that the current TCCON network would be sufficient, only that it is a means of supplying tropical column measurements. To downstate this we have reworded "The TCCON can potentially provide these data". At least one more tropical TCCON site is planned and should be operational later this year (2010).

Comment: - p.993, l.17: I would consider carbon stocks in the tropical rainforest, deforestation, and tropical wetlands more prominent reasons to investigate the tropical CO_2 flux budgets than "savannahs" and "biomass burning".

Response: Our intention here was to outline budgetry issues regionally important to Darwin, and we can understand how this was misinterpreted. These other factors have been addressed.

Comment: - p.998, l.7: What is an "absorber weighted" gravitational acceleration?

Response: - essentially this is the column-average value of gravity, which is the value of g at one scale height above the surface. We have rephrased this as "column-average gravity". The g(p) appearing here should not in fact be a function of pressure, and so has been replaced with $\hat{g}$. Also, on p1001, g(p) is indeed a function of pressure, but is not absorber-weighted, and so the phrasing in the corresponding legend has been updated simply read "gravitational acceleration".

Comment: - p.999, section 6: It seems awkward to me that "precision" can improve when ratioing two quantities given that "precision" contains only random errors. One could replace "precision" by "standard deviation" except for the last occurrence in l.23 which indeed should refer to a purely random error contribution (given all biases cancel in the ratio).

Response: - We agree that it is counter-intuitive that ratioing two quantities can result in a smaller fractional standard deviation than on either of the two initial quantities. Clearly, in this situation, random noise is not the dominant error term. More likely there are short-term biases (e.g. clouds, pointing errors) that are common to the CO_2 and O_2.

Nevertheless, "precision" is a misleading term. What we are effectively referring to here is "reproducibility" and as such, the instances of the word "precision" have been replaced by "reproducibility" and a reference to the GAW glossary included.

The word "improved" is perhaps misleading, and we have replaced this with "better". The two quantities (X_{CO2} and X_{O2}) are calculated relying on different measures of the total dry column. In X_{O2} the measured pressure contributes to the calculated value. In

X_{CO2} the measured pressure is not used directly in the calculation.

Comment: - p.999, l.14: Please, clarify what you mean by "pressure transducer variability".

Response: - it means the variability of the measured pressure within the precision of the pressure transducer, that is, 0.3 hPa.

The technical comments raised by Referee 1 have been addressed.

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Anonymous Referee 2

Comment: 1.) The chapter on the precision of O_2 and CO_2 is relatively short. A few points could be discussed in more detail, for example: - Since the residuals of Figure 2 show a typical variability of 0.5%, why is it possible to achieve a precision of < 0.1% for the total column? – The diurnal variability as a function of the solar zenith angle is < 0.2%, even when applying the airmass correction. How does this coincide with a precision of < 0.1% for the total columns? Regarding both points, I assume all calculations are correct, but a more detailed discussion would be helpful.

Response: Possibly the reviewer is confusing the RMS uncertainty achieved by the spectral fit and the precision of the measurement. Since the fitted CO_2 windows contain approximately 100 lines, it is possible to achieve a precision in the retrieved column that is much higher than the RMS spectral fit.

The reason that the RMS residuals are larger than the precision is because many of these are systematic effects (hence the need for calibration), and common to all fits. They are therefore invariant from spectrum-to-spectrum and affect the raw accuracy, but not the precision, of the measurement. A paragraph discussing this has been added.

Regarding the second point – I am not sure what point the reviewer is trying to make

here. In Figure 3, it is true that the size of the uncertainty bars is 2x the quoted precision, because these bars represent the mean $\pm$ standard deviation. We have updated the reference to Figure 3 in the body of the text to clarify that the error bars are $\pm$ standard deviation.

Comment: 2.) It would be interesting to see how the aircraft data really improve the total column results. Assumptions must be made for the vmr- and uncertainty a-priori profile of CO_2. The aircraft campaigns help to reduce the uncertainties. An aircraft going for example up to 4 km will still leave large uncertainties for the column above. An aircraft going up to 12 km is much better. A separate chapter and/or a table where this is discussed would be very helpful.

Response: - to date we haven't included these data, as to determine the uncertainty given high precision and accuracy sampling over a fraction of the column is a fairly simple calculation. We could include a table, something like the following, to highlight this.

Table x. Uncertainties in aircraft integrated columns sampling over specific altitude ranges

Sampling range	Uncertainty in integrated X_{CO2} (μmo
$0 - 4$ km	1.3
$0 - 12$km	0.5
These flights	0.7
$0.3 - 14.7$km (these flights w/ no missing data)	0.4

. Yurganov

Comment: - I strongly support a request of the Reviewer 1 for a more detailed consideration of the convolution procedure for aircraft profiles. Rodgers Connors paper really concentrates on layer-by-layer retrievals, but also considers total column data that are

integrated from retrieved profiles. Do the authors use the standard averaging kernel technique of layer-by-layer perturbations? The layers in their forward model seem to be different in partial columns (and geometrically constant). Do they perturb the layers by equal VMR or equal partial columns?

Response: - Initially, the GFIT code used a finite-difference perturbation analysis in which the VMR at each atmospheric level was perturbed and the resulting retrieved column compared with the original. This was done separately for each level. More recently, it has been realised that it is more accurate and computationally efficient to solve the matrix equation relating the column Jacobians to the single-level Jacobians. This is how the TCCON averaging kernels are computed

The perturbations are a constant fraction of the a priori VMR, so in terms of partial columns they decrease with altitude due to the decreasing total number density.

It is true that the layers are geometrically constant.

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