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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diagnostics of the Tropical Tropopause Layer from in-situ observations and CCM data" *by* E. Palazzi et al.

E. Palazzi et al.

e.palazzi@isac.cnr.it

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General answer to referees/editor comments

Dear editor, dear reviewers,

authors kept in regard your comments, amendments and suggestions and thank all of you for many useful remarks, that have allowed to widen and improve the analysis presented in the paper.

We do hope that the manuscript is now more readable and fluent, and the conclusions less generic and more quantitative, than in its previous version.

Since the requests to sharpen the manuscript and extend the analysis were in common

among reviews, we include this general answer within each reply, to summarize the major changes done on the manuscript.

The paper presents additional analyses, consisting in the discussion of the vertical temperature and static stability (N2) profiles, the relative vertical CO gradients, and an extension of the H2O-O3 joint PDFs to the whole observational database. The vertical profile of N2, and the relative vertical CO gradients, in addition to the ozone gradient, allow to calculate the top and bottom bounds of the tropical transition layer and provide a precise metric to accurately and quantitatively compare the model and the measurements. All new analyses have allowed to improve the evaluation of the model capabilities in the TTL.

The most important differences among the campaigns arisen from the tracer analyses have also been deepened, though they were not among the principal objectives of the paper. This bring to a deeper evaluation of the model capability in reproducing the TTL structure and its thickness, and also allows to better analyse the factors leading to the model-measurements discrepancies. Additional figures on that point have been included in the specific answers to referees.

In order to highlight the objectives and the results in the text, the abstract has been changed to describe the main findings of the paper; the introduction has been substantially modified presenting the results of previous aircraft, satellite and model studies carried out in the UT/LS region, and clearly stating the aims of the work. The section “Methodology” has been re-structured to better describe the diagnostics used (Tropopause coordinates, vertical tracer gradients, and tracer-tracer correlations) and how data have been handled to perform the model-measurement comparison. One table (Table 3) is added to resume the observed and simulated values of the TTL thickness; Figures 4 and 5 (now Figures 6 and 4) have been modified to show, respectively, the vertical temperature profile, and the relative vertical CO gradient and N2 vertical profile. PDFs analysis has been improved taking into account the vertical distribution of the number of observations, and extended to all the measurement campaigns. The

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conclusions have been rewritten to summarize our findings and the new quantitative results.

Answers to referee #3

General comments: This study compares recent aircraft measurements of ozone and water vapour in the tropical tropopause region with data from the chemistry climate model ECHAM/MESSy in free running mode using different recently developed diagnostics mainly for use in the extra-tropics. The application of these diagnostics to tropical aircraft data is new and interesting not only from a model-measurement comparison point of view and makes the study certainly worth publishing. The manuscript, however, needs some improvement before I can recommend publication in ACP. In particular, the authors need to be clearer in their conclusions about what the new results are and where their results just confirm already published results. E.g. they should clearly state, what we learned about the model's performance, and if the diagnostics previously applied to the extratropics are also valuable for validation in the TTL. In particular I would try to get an answer, if their approach to use the QBO phase instead of a multi-annual mean helps to improve the comparison. Improvements are needed along the comments stated below along with a list of technical comments, asking for some changes in the language used, but which is far from being complete.

These remarks have been partially raised also by other reviewers. Most of the modifications and additional material have been done to follow these comments. Please refer to the general answer above. The specific comments (useful and pertinent), deepening the general comment have been addressed below.

Answers to referee #3

Specific comments

P11660 L22-26: This sentence to me belongs into the introduction, so to motivate why you perform this analysis. Here in the abstract, on the other hand, you rather need to

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say based on your results, if the model does represent the TTL structure accurately, or if the application of the new diagnostics can be regarded as useful or not.

The abstract an introduction have been rewritten to include a synthesis of the results and clarify the objectives of the paper.

P11663 L20-23: I do not think you investigate and show with your evaluations that these couple tracer profiles are representative for the whole TTL mean fine scale structure. Rather your results seem to show that the measurements are influenced by the particular sampling during the aircraft campaigns. You need to improve your evaluations or take this claim out of the manuscript. (see also comment further below)

Introduction has been rewritten and the sentence is re-phrased. This is an important and complex issue since the representativeness of local observations is difficult to assess. We consider that the discrepancies between model and obs. can help to evaluate the variability during a single campaign. As mentioned above, it is questionable to filter observations to select data that are taken in meteorological situation comparable with model circulation. So we have decided to analyze the model 2d fields to identify and report in the paper the processes likely leading to discrepancies.

P11665 L12: add '...along with their accuracy and precision'

Added

P11666 L22: I did not really follow your approach here. First, 'temporal location' sounds awkward. Rather use something like 'temporal sequence'. However, the campaigns are not marked in their real temporal sequence (TH and TR should be before SC). Is this because of your selection according to the QBO?

That paragraph has been rewritten and the approach followed should be (hopefully) now more clear. QBO phase has been estimated during the Geophysica aircraft campaigns and model data. So, we selected model years to have the closest match with QBO during observational campaigns.

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P11667: Methodology section: this section might be improved by structuring it a little more. E.g. different subsections for different diagnostics used plus a section on how you handle the data (QBO-classification).

The Methodology section has been re-structured according to the referee suggestion.

P11670 L16-26: I think this is an interesting approach to sample according to the QBO phase. Has this been done before? If so give references, if not, you should explain your motivation for using this approach. Separate for QBO or filtering

To the authors knowledge, the proposed approach based on QBO clustering (or filtering) has not yet been applied in CCM evaluation studies, though the effects of the QBO on transport and chemistry of trace gases have been discussed in observational and model studies (see for instance a recent paper of Punge and Giorgetta and references therein, "Net effect of the QBO in a chemistry climate model", ACP, 2008). Authors stress the fact that the QBO influence on the TTL tracer distribution is not a major aim of the paper. Nevertheless, the reason for the QBO-classification has been explained in the methodology section (subsection "Data handling"), and a couple of references on the QBO have also been added there.

P116672 L6-8: This seems unnecessary/wrong information. The profiles are plotted relative to the tropopause heights, so the dashed-dotted line corresponds to the thermal tropopause of both, the measurements and the models. You did plot the model data also relative to the thermal tropopause, no?

Yes, it is. The sentence has been modified to "[...] and the dash-dotted black line denotes the thermal tropopause pressure (0 hPa)"

P11672 L10-14: Interesting, and you can see the same feature even in the N2O profiles, where the N2O values are higher than expected across the tropopause. A cross-link between this finding and the N2O section would be helping the reader to tie the conclusions together. Also, rather than just describing what Hector is, the manuscript

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would improve if you said more directly, that ECHAM Messy just can't resolve this particular feature. In the end the conclusion here tells me that the SCOUT -Darwin measurements are not representative for the mean fine-scale structure of the tropics, so you might want to add or change this in the conclusions.

Both sections discussing ozone and N2O vertical profiles have been partly rewritten and cross-linked, to explain the model-measurement discrepancy in the case of SCOUT-Darwin. In addition to what explained in the manuscript concerning that point (subsections 4.1 and 4.3) authors provide some figures to answer the question raised by the reviewer, that are detailed below: 1) Fig.1 and Fig.2. The former shows the vertical profiles of log(H2O), N2O, O3, CO (all the measurements and their mean superimposed) during two different periods of the SCOUT-Darwin campaign (you can find a brief description and explanation in the text and in the references therein), essentially before and after 28 November. The latter compares the observed mean profiles of N2O and O3 with the model profiles. It can be noticed that N2O observations are very sparse during SCOUT-Darwin, which has been highlighted in the conclusions, as a reason for the less smoothed observed profiles compared to the model profiles. So, both figures attached corroborate the existence of two different circulation regimes, leading to a different origin of the air in the TTL and lower stratosphere over Darwin, and different chemicals amounts. 2) Fig. 3 and Fig. 4 describe the O3 model field at 395 K and 410 K with superimposed the measured O3 mixing ratios (same color code). They are a confirmation of the lower O3 mixing ratio concentrations found in the observations with respect to those reproduced by the model.

P11674 L25-30: This is written in an obscured way. It is well-know that there is a seasonal cycle in tropopause/lower stratospheric H2O due to the seasonal cycle in temperature. Please include some reference.

The sentence has been shortened including a reference to Mote et al. 1996.

P11675 L2: The main sink of N2O (90 percent) is photolysis. Please add a statement.

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Added

P11676 L18: I suggest trying to color the data points according to potential temperature bins. This is because 12 hPa at different altitudes may span different widths in km due to differences in the mean height of the tropopause, which may make your comparison worse than it really is.

This is a sharable remark. However, authors would prefer to keep the pressure notation for a twofold reason: (1) to be coherent with the vertical coordinate used throughout the paper, and (2) because pressure is a direct output of the model and the tracer mixing ratios, as well as the dynamical variables are actually given on pressure levels. However, in order to partially answer and follow the reviewer suggestion, the TTL top and bottom boundaries are given both in P-TP and potential temperature values (reported in the conclusions).

P11677 L0-4: I think this is also not a new finding, please include some references.

We have included the recent reference Hegglin et al., 2009

P11677 L2: what do you understand by a 'less pure tracer'? you may want to add at least that 'since H2O is influenced by microphysical mechanisms'.

Yes, pure stands for passive, and H2O has a shorter lifetime then CO. The sentence has been modified.

P11679 L15-22: It would be nice to show the PDFs for all the campaigns.

We agree. All campaigns are added, and Figure 9 modified accordingly. The calculation of PDF has also been modified to treat the observational sampling bias in the joint-PDFs calculation. At this regard, see also the last answer to referee #2

Technical comments

The reviewer technical comments and suggestions (listed below) have been all considered and applied to the paper.

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- P11660 L3: change ‘tracers profiles’ to ‘tracer profiles’ and ‘tracers gradients’ to ‘tracer gradients’. Also put ‘using tropopause coordinates’ right after ’tracer gradients’, since tracer-tracer relationships cannot be used in tropopause coordinates. - P11660 L7: Suggest to use ‘were obtained during four’ instead of ‘come from the four’. - P11660 L21: delete ‘first’ - P11661 L3: Please use a different reference here than Fueglistaler et al. (2005), there were earlier papers on this feature (e.g. Highwood and Hoskins 1998; Folkins et al. 1999; Gettelman et al. 2002) - P11661 L11: write ‘transports mass’ instead of ‘transports the mass’ - P11662 L25: suggest to write ‘... showed that strong gradients in tracer distributions and mixing across the extra-tropical tropopause can be highlighted...’ - P11662 L28: correct sentence, use ‘In this paper we perform a joint analysis of in-situ high-resolution measurements...’ and ‘...Geophysica during four tropical ....’ - P11663 L3: you need to replace the comma before ‘and the ECHAM Messy’ by a dash. - P11663 L9: write ‘The diagnostics are based on the analysis of vertical profiles... and of relative vertical ozone gradients...’ - P11663 L11-14: these are awkward sentences, please improve. E.g. use ‘...chemical species over the whole TTL depth...’, replace ‘peculiar’ by ‘different’, and start next sentence rather with ‘Here we choose O3, N2O, H2O, and CO in order to characterize the TTL’ - P11665 L8: write ‘... can be considered to be the dataset least influenced by convection.’ - P11668 L11: change to ‘tracer transition’ - P11668 L21: change to ‘and for process-oriented validation of CCMs’ - P11668 L26: suggest changing ‘enhances’ to ‘emphasizes’ - P11671 L21: use ‘details’ rather than ‘specificity’ - P11672 L28: delete ‘actually’ - P11673 L25: delete ‘in the main’ - P11676 L8: write ‘found in the literature’ - P11676 L16: write ‘all the points...’ - P11679 L23: suggest to write ‘of interest because it is the interface...’ - P11680 L6: write ‘due to the difficulty to understand and correctly...’

Answer to other technical comments:

- P11660 L8: what do you mean by ‘directly compared’? “Directly” has been eliminated

- P11661 L19: do you really mean ‘validated’ or ‘found’? “Validated” has been changed with “observed”

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- P11669 L6: The Randel et al. (2007) reference seems to be wrongly cited here.

Yes, it was a mistake. The correct reference is “Randel, W. J., F. Wu, and P. Forster (2007), The extra-tropical tropopause inversion layer: Global observations with GPS data, and a radiative forcing mechanism, J. Atmos. Sci., 64, 4489–4496.” The referee can find this reference cited in the Methodology section (subsection Vertical tracer gradients)

- P11673 L2-5: local effects may not be the only reason for this. You might want to indicate numbers of measurements at each altitude bin in Figures 3-6, this would possibly explain the lack of smoothness in most of the measurement profiles.

This sentence has been moved from that section to the Conclusions, where it is stressed the fact that the non-homogeneous observational sampling can be one reason for the lack of smoothness in the observed vertical profiles. Nevertheless, the choice of the [50,-50] dp interval is done to have a resonable number of observations per relative-pressure bin. The Fig. 5 attached shows that in the whole pressure range considered, the number of measurements within each bin is always larger than 100/200. The figure refers to the SCOUT campaign but it is representative of the typical M55-Geophysica vertical sampling.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 11659, 2009.

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Interactive Comment

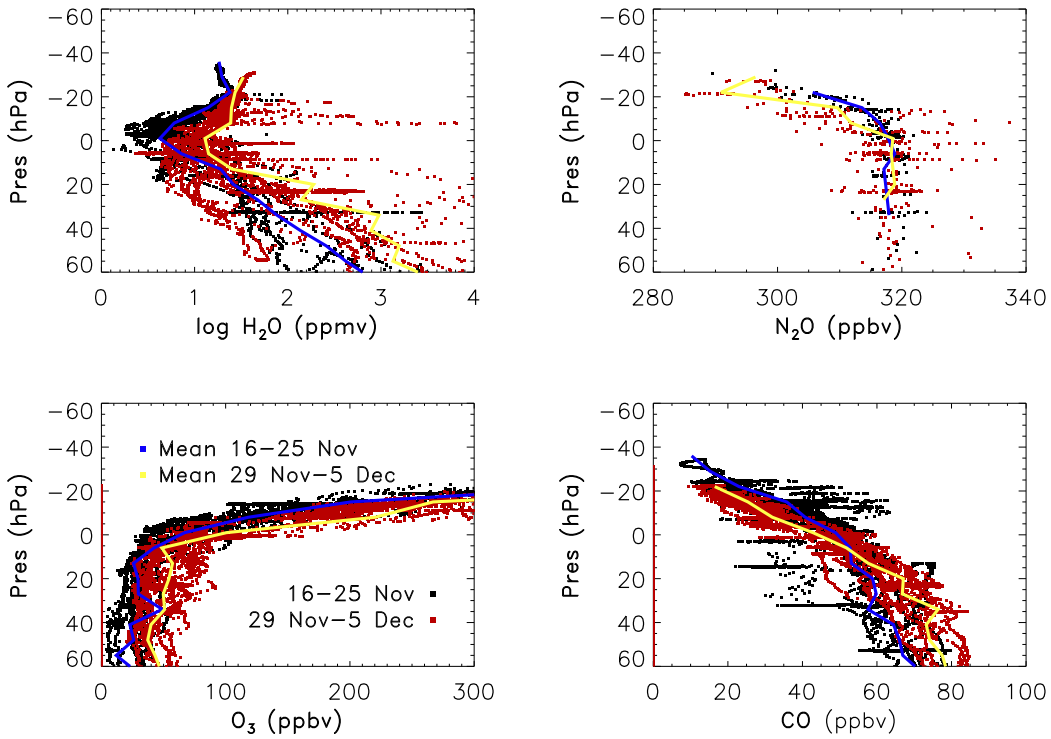

Fig. 1. The vertical profiles of log(H2O), N2O, O3, and CO (all the measurements and their mean superimposed) during two different periods of the SCOUT-Darwin campaign (see the legend and the text)

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Discussion Paper

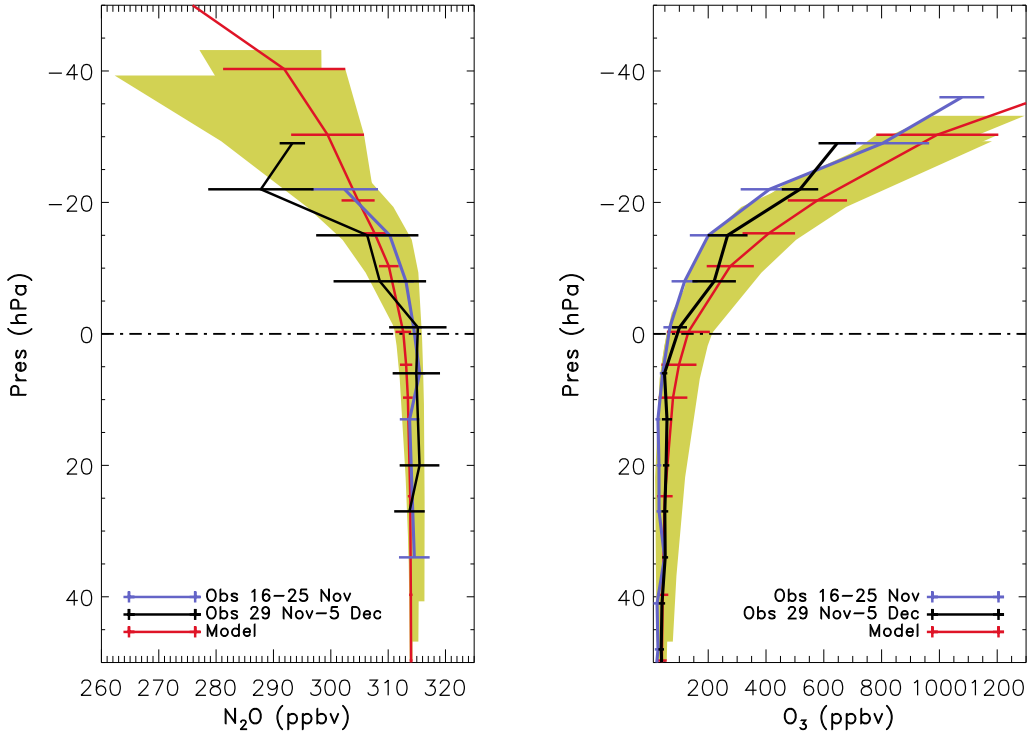

Fig. 2. Observed and modelled mean profiles of N2O and O3 during SCOUT-Darwin (for the observations, the mean vertical profiles for the two SCOUT-Darwin time periods are considered separately)

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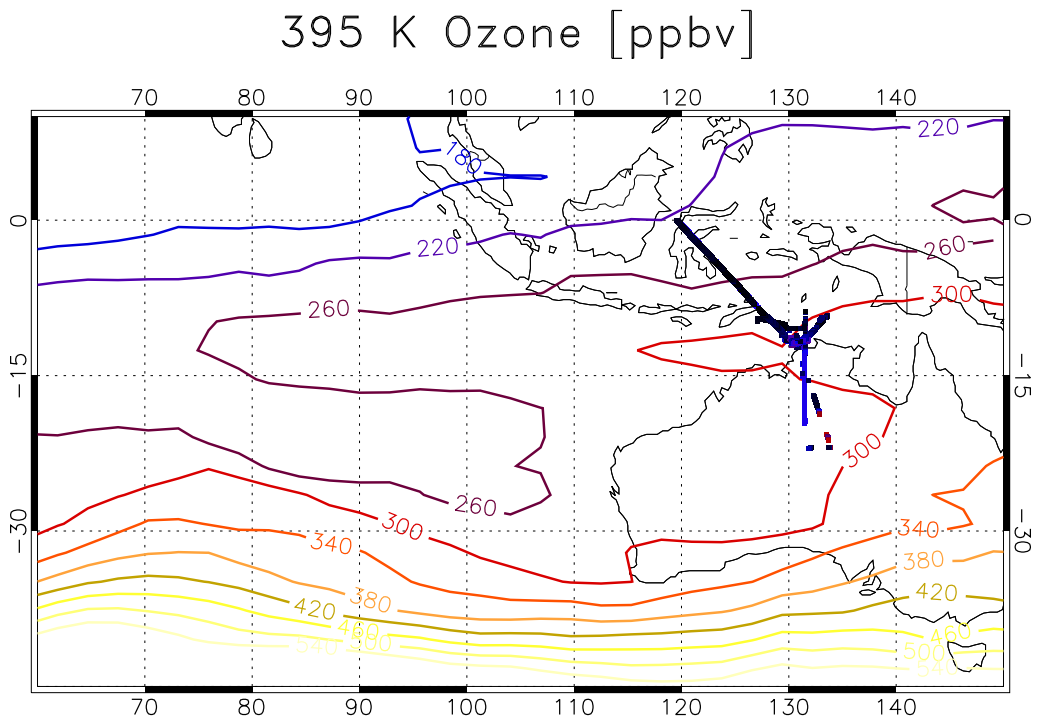

Fig. 3. O3 model field at 395 K with superimposed the measured O3 mixing ratios (same color code), for the SCOUT-Darwin campaign.

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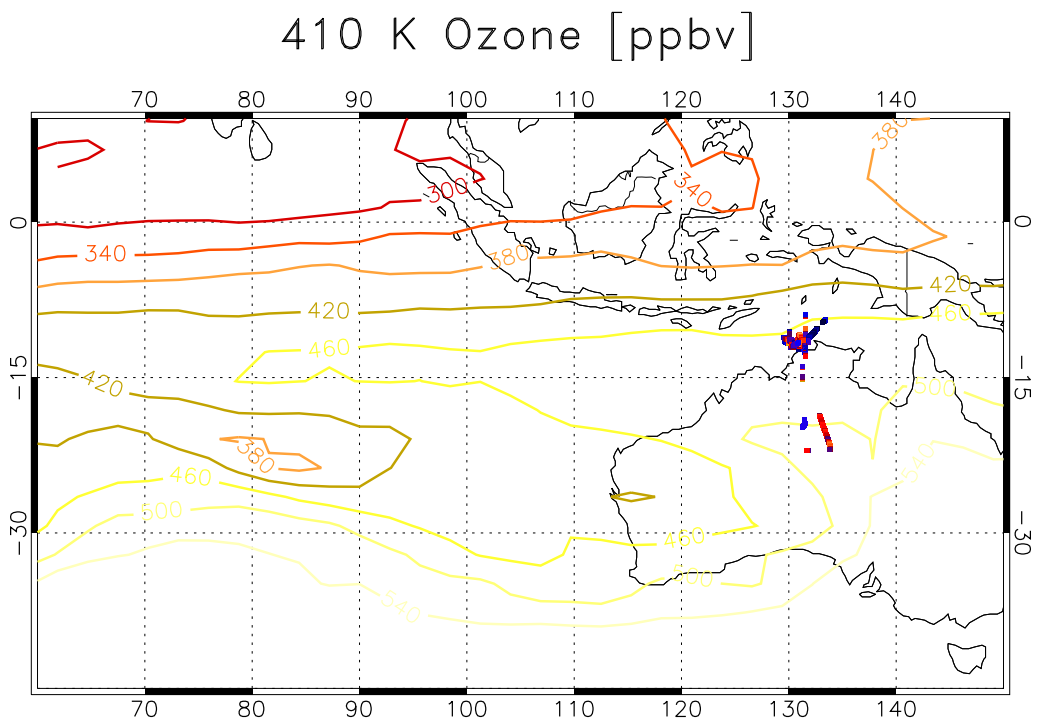

Fig. 4. As in Fig. 3, but at 410 K

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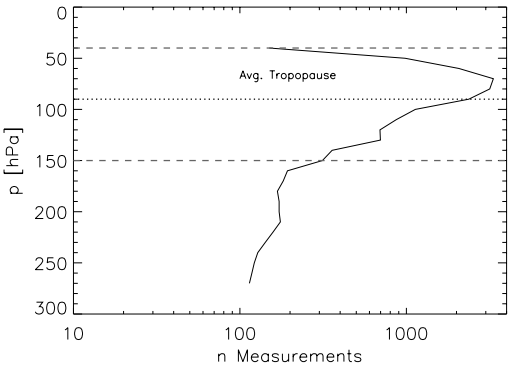

Fig. 5. Observational vertical sampling during the SCOUT-Darwin campaign

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