Reply to referee 1

The author would like to thank referee 1 for the useful comments which have helped to improve the original manuscript.

We first address some of the general points raised by referee 1 (listed as General Comment), followed by replies to the specific comments (listed as Comment).

General Comment a

"Ideally any such comparison of satellite observations for the purpose of investigating moistening of the TTL needs to be multi-annual in order to strengthen the conclusions regarding differences in regional behaviour which appear in the measurements."

> **Reply.** The aim of including water vapour at 150 hPa in Fig 2, is to investigate whether a correlation exists between the seasonal cycle of convection and the seasonal cycle of water vapour mixing ratio at 150 hPa. This is because water vapour can be considered as a tropospheric tracer and its seasonal variation at 150 hPa could indicate a convective influence on atmospheric composition up to that height, which is a significant level as it coincides with the level of zero radiative heating (~14-15km). Most of the analysis in this paragraph (pag 19486 ln27-29 and pag 19487 ln 1-26) focuses on the strength of this correlation for the different regions, and we believe this information is valuable. (see also additional reply to specific points in comment 5, Pg 19487 ln 15).
>
> However, we agree that based on our analysis we can only infer regional differences in moistening the TTL for the specific year 2005. We have therefore removed text referring to moistening of the TTL on pag 19487 ln 26-29 and pag 19488 ln 1-16, and substituted it with the following:
>
> "For South America both water vapour datasets suggest that the seasonal variation of water vapour mixing ratio at 150 hPa is modulated by the strength of convection; for West Africa and the Maritime Continent the different water vapour datasets disagree on the extent of the correlation, possibly due to temperature control. Although this analysis suggests that convection can modulate water vapour concentrations up to 150 hPa (for the specific year 2005), further studies including several years of data and additional information on temperature, are necessary to understand the role of convection in moistening the TTL, and to assess whether the correlation we observe between convection and water vapour concentrations at 150 hPa is due to direct vertical transport by convection or to indirect effects of convection on the vertical temperature profile."

General Comment b

"*This fact that the intercomparison of the selected satellite products is rather crude is acknowledged by the authors themselves [......] Surely making comparisons against an ensemble mean of the satellite data (including the standard deviation) would be more beneficial anyway which would remove the need for comparing each product individually and allow more space to be dedicated to the reasons for the differences between different models??"*

> **Reply.** We believe it is best to plot each different satellite product separately. A mean and standard deviation would suggest that the mean is the best estimate which is often not the case. Given that different satellite datasets use different resolution, temporal sampling, measurement techniques, etc., we believe that the explicit use of different datasets is more appropriate and can provide the informed reader with valuable extra information.

General Comment c

"Either a stronger link should be made between the distribution of WV fields in the models and the associated observations or the WV part should be completely removed."

> **Reply.** Based on a similar comment from referee 2 we have decided to expand the analysis of WV to include comparison with the models.

General Comment d

"The comparison of the precipitation rates should be expanded following the comparisons of the cloud-top heights as it is currently weakened by using only one of the regional domains for a single month. This would allow the reader to assess potential shortcomings of each model for land/sea regarding this diagnostic."

> **Reply.** We do not fully understand this comment. The comparison of modelled and observed precipitation rates includes different domains and different times. Fig 3 shows the seasonal cycle of precipitation rates for all 3 domains. Additionally, global latitude longitude plots in Fig 4 provide comparison of annual mean precipitation rates. Therefore the reader has enough information to assess potential shortcomings in each model representation of the seasonal cycle (Fig 3) and geographical distribution (Fig 4) of precipitation rates for all regions under investigation.
>
> However, if the comment refers to our choice of the Maritime Continent region for the detailed comparison in Fig 5, we believe that this choice is justified by the

large differences in observed and modelled precipitation rates for this particular region, while differences in precipitation are less marked for West Africa and South America (as shown in Fig 3 and Fig 4). Furthermore, the latter two regional domains are covered almost entirely by land and no land/sea contrast or any other interesting feature can be observed by detailed analysis that cannot be already inferred from Fig 4.

General Comment e

"I also have reservations about the treatment of the MODIS data which needs to be addressed before publication. Some type of screening is needed concerning the cloud types due to the large fraction of cirrus which is included in the observations. Is cirrus included in these models?? I expect so and probably defined by the Ice Water Product of the meteorological data. However, comparisons are made against cloud-top height in the models, where potential only included liquid water cloud rather than a mix of cirrus and LWC as in the measurements."

Reply. The referee is right to point out that MODIS observations include cirrus clouds while ISCPP and model data do not have such a contribution, (incidentally we already mention this on pg 19481 ln 3-5 and pg 19492 ln 6-10 and the manuscript already points to large fractions of cirrus clouds in tropical regions as a possible reason for the bias between MODIS and ISCCP data on pag 19492 ln 10-14; pag 19495 ln 12-20). Since further comparison of ISCCP and MODIS data in the modified Fig 2, shows only one instance where MODIS data is potentially affected by cirrus, i.e. the Maritime Continent in November (see more details on the specific reply to comment 5), we have decided that it is preferable to provide the reader with the relevant information to put the MODIS data into context for this specific case rather than attempt to screen cirrus clouds from the MODIS dataset. We have therefore changed the dataset description in section 3 to stress this point even further by referring to the MODIS data as an "upper limit" for observed cloud top heights. We also acknowledge that some of the discussion of Fig 7 and conclusions in section 4.3 (which focuses on the vertical extent of convection) could be misinterpreted unless more emphasis is explicitly given to the differences between MODIS and the other data. The text on this section has been modified, where necessary, to rectify the problem, but we believe that the general conclusions on the cloud analysis are not affected. With regards to the models, we use convective cloud top heights whenever available, and cirrus are therefore not included.

General Comment f

“The link to the second part of the study (Hoyle et al, 2010) should be clarified as only a subset of the models which are included in the second part are included in this first part.”

Reply. We have now modified the manuscript to rectify this problem.

General Comment g

“Some sections such as the abstract need to be completely re-written as they currently do not provide the reader with the necessary information regarding the model comparisons. The model description is not currently informative enough for the reader to easily determine the sources of e.g. cloud top pressure.”

Reply. We have addresses these concerns by rewriting the abstract (see comment 2 below) and expanding the cloud top definition in section 2.

SPECIFIC COMMENTS

Comment 1, pag C7273

“Title: Include the word “Satellite” before observations”

Reply 1. Based on the referee’s recommendation and also on additional comments from the referees of Hoyle et al 2010, we have now changed the title of the manuscript to “Representation of tropical deep convection in atmospheric models – Part 1: Meteorology and comparison with satellite observations”

Comment 2, pag C7274

“The abstract does not currently summarize the main findings of the study for the prospective reader with respect to the model comparisons and should be rewritten to address this.” + further specific comments on the abstract (pag C7274).

Reply 2. The abstract has now been rewritten accordingly, including specific suggestions (see below).

“Tropical convection is an important atmospheric process acting on the water cycle, radiative budget of the atmosphere and air composition of the upper troposphere. The fast vertical transport by convective plumes can efficiently redistribute water vapour and pollutants up to the upper troposphere and it has been suggested that convection could also have a significant impact on the composition of the lower stratosphere. In this study 8 different atmospheric three-dimensional models are compared within the framework of the SCOUT-O3 (Stratospheric-Climate Links with Emphasis on the Upper Troposphere and Lower Stratosphere) project. The models range from the regional to the global scale, and include numerical weather prediction (NWP) models, chemistry transport models (CTM), and chemistry climate models (CCM). Due to the interplay of chemical and dynamical processes, it is difficult to evaluate the model convective transport of chemical species by direct comparison with observed chemical fields. For this reason, the characteristics of tropical convection in this set of models, such as seasonal cycle, land/sea contrast, strength and vertical extent, are compared using satellite observations of meteorological variables as a benchmark for model simulations. The observational datasets used in this work include precipitation rates, outgoing longwave radiation, cloud top pressure, and water vapour from a number of independent sources (TRMM, GPCP, CMAP, NOAA, AIRS, AURA-MLS, MODIS and ISCPP). Most models are generally able to reproduce the seasonal cycle and strength of precipitation for continental regions (such as West Africa and South America), but show larger discrepancies with observations for the Maritime Continent region in South East Asia; further analysis shows that this is due to the difficulty in representing local precipitation maxima over islands and peninsulas. The frequency distribution of high clouds from models and observations is calculated using highly temporally-resolved (up to 3-hourly) cloud top data. The percentage of clouds above 15 km (which coincides with the level of net radiative heating, above which air parcels can be transported upwards by radiative heating into the lower stratosphere) varies significantly between the models and for different tropical regions, with some models consistently under or over estimating observations. Finally we discuss the implications of our findings for the convective transport of very short lived species, such as halogenated hydrocarbons and isoprene, in tropical regions."

Comment 3, pg C7274 and further comments on the introduction (pg C7274-C7275)

"The introduction needs to be expanded. There is currently no mention of using satellite products for the purpose of diagnosing regions of strong convective activity which constitutes a 30% of the content of this paper or past work conducted in this area."

Reply. Several studies using satellite products to investigate convection are currently mentioned in the introduction. However, we have changed the text to explicitly stress the use of satellites where appropriate and we have also added more references as suggested by the referee.

We address below the specific recommendations on the introduction.

- Pg 19474 ln 12 and 15-17: a clearer explanation is given about the simulation rounds for the model intercomparison and an explanation is given for the discrepancy between the models used in this study and those used in Hoyle et al. 2010.

- Pg 19474 ln 20: use of “Maritime Continent” is widespread in tropical meteorology (often appears in the title of scientific publications). However, we have now introduced the “Maritime Continent region” in the abstract and specified its location to avoid confusion.

- Pg 19474 ln 23-30: Although we use observations as a benchmark for model simulation, a small but non negligible part of this paper focus on the analysis of the different observations and what they tell us about the strength and seasonality of convection. We therefore believe that this paragraph expresses clearly the scope of this study.

- Pg 19475 ln 2-8: we believe that the justification for the use of only 1 year of data and the reasons for choosing the specific year 2005 are too important to go in section 2, as they might otherwise be overlooked.

- Pg 19475 ln 8-10: the paragraph has now been rewritten to improve readability.

Comment 4, pg C7275 and further comments on section 2 and 3 (pg C7275-C7276)

“All the differences between the tracer transport schemes should be comprehensively outlined in this paper as the continual reference to the “second paper” does not provide the reader of this paper with enough details. To be sequential, the second paper should refer to the first for the model description concerned with precipitation and clouds. Moreover, this paper should be able to be read on its own thus should contain all relevant information needed to digest the results presented in later sections.”

Reply. The referees of Hoyle et al 2010 have also suggested that the second paper should contain all relevant information to be able to be read on its own. Since we do not analyse tracer transport in this paper, we believe that the detailed description of tracer transport schemes is better suited to Hoyle et al. 2010. We have also removed from this section the continual reference to the second paper as it is distracting.

We address below the specific recommendations on section 2 and 3.

- Pg19476 ln 2: a clearer explanation of the different models and which study they contribute to is now given in the introduction. Furthermore we have modified text here accordingly.

- Pg19476 ln 25: Where models are very similar or identical, we still prefer to identify the data with both models rather than just one. This is not only for consistency with Hoyle et al. 2010, but also to provide information that people using both models can refer to in future studies.

  "Figs 4-6 have missing panels so both the TOMCAT/p-TOMCAT average and the OSLOCTM2 and FRSGCUCI average should be replaced by individual distributions":

- to be consistent with line plots, we prefer to have only one panel for similar models to avoid confusion. Missing panels in figures 4-6 can be eliminated by rearranging the plots in the final submission.

- Pg 19477: details on treatment of precipitation for the CTMs is already present in the manuscript. We have now added a description of the precipitation schemes used by online models.

- Pg 19478 ln 26: as the referee points out, water vapour fields in CTMs are prescribed using analysis and are not calculated online. Since we want to see how water vapour in the models is affected by convection, we cannot use this water vapour fields from CTMs since it is not modified by the model. However, in order to extend analysis of water vapour and compare observations to all our models, we have decided to use an "idealised water vapour tracer" for CTMs. This tracer is initialised to a climatological water vapour distribution and constrained to the same climatology below 7km for the duration of the simulation. The tracer is subject to transport (including convective transport) and is removed if its concentration reaches the saturation mixing ratio with respect to ice (liquid droplets formation is assumed to be negligible above 7km).

- Pg 19478 ln 7-8: to make the paper more consistent we have now added CATT-BRAMS limited area simulations for the 3 different domains in Feb, Aug and Nov. The model description and the information on Table 1 have been updated accordingly.

- Pg 19479 ln 14: we use GPCP daily dataset V1.1 which is equivalent to the monthly dataset V2.1 described in Huffmann et al. 2009. The standard reference for the daily dataset V1.1 is Huffmann et al. 2001 (suggested at http://precip.gsfc.nasa.gov/gpcp_daily_comb.html). We have now specifically mentioned V1.1 in the dataset description to avoid confusion.

- Pg 19481 ln 3-5: a reference to Ackerman et al 2008 has been included
- Pg 19482 ln 9: the 20% accuracy for water vapour measurements by AIRS is from Susskind et al. (2003), IEEE Trans. Geosci. Remote Sens., 41, 390– 409. This reference has now been added.

Comment 5, pg C7277 and further comments on section 4 (pg C7277-C7281)

Reply. We address below the specific recommendations on section 4.

- Pg 19482 ln 19: “tropical warm pool region” has been changed to “Western Pacific tropical warm pool region (located east of Australia and Papua New Guinea)”.
- Pg 19482 ln 20-24: Hoyle et al. 2010 includes comparison with in-situ campaign measurements. Furthermore, the 3 regions were chosen for the measurement campaigns on the basis that they often exhibit deep convection and are therefore well suited for the scope of this paper.
- Pg 19482 ln 27-28: a reference to support the influence of soil moisture on convective development over West Africa and Northern South America has now been added (Koster et al. 2004, Science, Vol. 305 no. 5687 pp. 1138-1140 and references therein)
- Pg 19483 ln 1-3: references to support the influence of sea breeze convergence on island convection and its importance for the Maritime Continent have been added (Saito et al. 2001, Mon. Wea. Rev., 129, 378–400; Neale and Slingo, 2003, J. Climate, 16, 834–848)
- Pg 19484 ln 7-9: we have modified this sentence to “The use of different observational datasets for each of the analysed variables allows us to estimate the range of variability in the observations.”
- Pg 19484 ln 14: the inter-annual variability of precipitation data for the different tropical regions is already addressed in Fig 2 where the seasonal cycle of precipitation for the specific year 2005 is compared to a long-term climatology for the year 1979-2000.
- Pg 19485 ln 1-8: references have now been added as suggested by the referee.

- Pg 19485 ln 12-14: this statement is based on Fig 2 (therefore no reference necessary).
- Pg 19485 ln17: this is beyond the scope of the present study.
- Pg 19485 ln 19: the sentence contains two typos i.e. “monthly” should be “daily” (twice). We start from daily cloud top height and only use gridboxes for which the daily data is greater than zero to calculate the monthly mean data averaged over the domain of interest. We apologise for the confusion. The sentence has now been corrected.
- Pg 19485 ln 22: We have now added ISCCP monthly mean values for Feb, Aug and Nov to fig 2, and modified figure caption and discussion accordingly.
- Pg 19485 ln 24-30: the addition of ISCCP data to fig 2 is beneficial since it supports the statement that high cloud top heights from MODIS are generally representative of convective clouds. The only case where we find that ISCCP has a low bias with respect to MODIS (indicating large fraction of cirrus affecting the MODIS data) is for the Maritime Continent in November. We believe that this issue is therefore resolved.
- Pg 19486 ln 6-13: we have removed the paragraph as suggested.
- Pg 19487 ln 2-6: we have moved this paragraph to the introduction as suggested.
- Pg 19487 ln 15: in Fig 2, precipitation, cloud top height and OLR are all used to determine the seasonal cycle of convection. Water vapour at 150 hPa is used to investigate whether or not convection has a significant influence on moistening the atmosphere up to 150 hPa. For this reason a more quantitative approach is required to assess the correlation between cause (convection) and effect (moistening of the atmosphere up to 150 hPa). Previous studies have looked at the co-location of convection and water vapour in the upper troposphere (e.g. Savtchenko, GRL, 2009 and Liu, JGR, 2007), but only use one observational dataset (AIRS and MLS respectively) at different heights (300 and 100 hPa respectively) and for different years (2007 and 2005 respectively). Given the large uncertainties in water vapour retrievals in the UTLS, we decided to use the two water vapour datasets in a consistent manner to assess whether or not a large influence of convection on water vapour at 150 hPa is supported by both datasets.

- Pg 18487 ln 20-29: for West Africa, the MLS correlation might be affected by measurements issues (since the correlation is strongest with cloud top and worse with all other data). For the Maritime Continent, the reason for the poor correlation between AIRS and convective activity is stated in the paper and it is mainly due to temperature control. Analysis of the seasonal cycle of temperature at 150 hPa (from AIRS), clearly shows a drop in temperature of ~1 K from September to November which would result in a decrease in water vapour despite the increase in convective activity.

- Pg 19488 ln 3-7: discrepancies between MLS and AIRS for the two regions are hard to pin down exactly.

- Pg 19488 ln 14-16: we already refer to Liu 2007 on the effect of convection and moistening of the upper troposphere. We have added Savatchenko 2009 (see above) since we believe it to be more relevant for this study.

- Pg 19488 ln 23-26: the sentence regarding the grouping of models has been removed as suggested.

- Pg 19489 ln 4: "this set of models" has been substituted with "the set of models in the current study".

- Pg 19491: we disagree with the referee's comment. The only models using offline precipitation from ECMWF are the CTMs OSLOCTM2 and FRSGCUCI, all other models calculate precipitation online, based on their microphysics parameterisation schemes (including the other set of CTMs TOMCAT, pTOMCAT and pTOMCAT_tropical). Our statement that resolution is important is supported by the fact that UMUKCA_UCAM_nud and UM_UCAM_highres use identical parameterisation schemes and still they produce completely different results! The importance of sea-breeze convergence for initiation of island convection is well known, and the sea breeze flow cannot be produced in the model if the dimension of the island is comparable to the dimension of the model grid.

  With regards to the suggestion of calculating correlation coefficients between models and satellites, we have done this in separate studies (Hosking et al Atmos. Chem. Phys., 10, 11175-11188, 2010; Russo et al. in preparation), but we think is beyond the scope of this paper.

- Pg 19491 ln 24: as described in section 2 of the manuscript, FRSGCUCI and OSLOCTM2 use ECMWF precipitation and cloud definition, while all the other CTMs (TOMCAT, pTOMCAT and pTOMCAT_tropical) use their own parameterisation schemes for precipitation and convective clouds.
- Pg 19492 ln 4-24: we have moved this paragraph to section 4.1 as suggested
- Pg 19493 ln 2-5: We believe this does not have any impact on the comparison between data and models, since the satellite and ECMWF geopotential height are independent variables. Furthermore, only FRSGCUCI and OSLOCTM2 use ECMWF meteorology directly for the definition of cloud top height.
- Pg 19494 ln 17-23: this has now been moved to conclusions as suggested
- Pg 19496 ln 27-29: we will add a mention in the introduction as suggested
- Pg 19497: we have tried whenever possible, to give reasons for the poorer performance of some of the models. However, detailed sensitivity studies are necessary to pin down exactly the reasons for such poor performance. We think this is beyond the scope of this study.

Comment 6, on Conclusions

"Some mention should be made as to whether the offline models perform better than the online models. The description of the differences are currently rather vague and the best overall model is not identified."

Reply. Based on this analysis it is not possible to make a general judgement on whether offline or online models perform better. Besides, the scope of this paper is not to identify the best or worse models, but to give an idea of the strengths and weaknesses of each model and what are the implications in using each model for tracer transport by convection, and more generally chemical modelling of short lived species.

Comment 6, on Grammar, typos and spelling

Reply. We have corrected the text where appropriate.