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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropical deep convection and its impact on composition in global and mesoscale models – Part 1: Meteorology and comparison with observations." *by* M. R. Russo et al.

Anonymous Referee #1

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General Comments:

This study by Russo et al. concerns a comparison of selected diagnostics for tropical convective activity measured by a range of different satellite instruments for three tropical regions with those output from various types of model, including CTMs, GCMs and NWPs. It essentially comprises of two main sections. The first focuses on a seasonal intercomparison of e.g. precipitation rates available from different satellite datasets. The second focuses on the agreement between the associated model output with some of the diagnostics included in the first section. Although both subject areas

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are appropriate for ACP I feel that the manuscript needs to be modified before publication to address the inconsistency which currently exists between the title of the paper and the rather limited analysis that is included concerning the reasons for the differences in the performance of the various models. In that the authors attempt to cover both subjects in one manuscript results in some of the evidence which is presented being rather weak for the conclusions which are reached. 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. Moreover, similar comparisons of such observations against independent data from e.g. lidar observations are already available in the literature, which typically provide a more robust validation of any data product, but these are often not referenced (see suggested references in this review for examples). This fact that the intercomparison of the selected satellite products is rather crude is acknowledged by the authors themselves who state "We use more than one observational dataset for each variable in order to assess differences between instruments and platforms and provide a rough measure of the uncertainties . . ." in the introduction. 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??

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. It is currently used in a tangential manner to the main focus of the paper in order to draw the conclusion that South America moistens the TTL more effectively than either West Africa or the region around Indonesia using a single years worth of data from a single satellite instrument. This analysis is currently not robust enough and should be part of a separate study rather than being bundled into this paper.

The comparison of the precipitation rates should be expanded following the compar-

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isons 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. 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.

There is currently only a limited differentiation between online and offline models which needs to be expanded on. Some indication must be given for the possible reasons as to why the model outliers are performing as they are. 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. Also it needs to be stated explicitly that the convective mass fluxes are compared in Hoyle et al (2010) and that the comparisons made here are due principally concerning with the physical

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. There should be a stronger use of the previous literature concerning the satellite products. There are many grammatical errors throughout the paper and it should be proof read by one of the authors for which English is their native language.

Specific Comments:

Title: Include the word “Satellite” before observations.

Abstract:

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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.

Pg 19471: ln 15: What is OLR?? Define acronym. ln 16: “geographical preferences for convection”. You only study the tropics therefore does this pertain to land/sea differences?? This should be replaced by “regions of active convective transport in selected tropical regions”. ln 16: “impact on water vapour”. Vertical distribution or the resident mixing ratios of water vapour at 150hPa?? ln 17: Observational data from satellites I assume. Which instruments?? ln 19: “numerical models” should be e.g. “small to large scale three-dimensional atmospheric transport models.” ln 21: Is this predominantly a modeling or observational study??

Introduction:

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. Also, more content needs to be included on the potential reasons for differences which exist between the different convective parameterisations employed across various model types. In particular how such parameterizations use meteorological data fields which drive atmospheric transport models (e.g offline vs online convective mass fluxes). Some previous work has been done on this and it should be acknowledged and placed into the context of this study.

Pg 19472: ln 9: Does deep convection really determine the composition of the lower stratosphere?? It is more likely to be limited to an ‘influence’ on the overall composition as you already stated that the direct penetration into the LS occurs at a rather low frequency. ln 11-12: How deep is the TTL?? Is this a few hundred meters or a few km?? ln 12: Is there a standard definition of the TTL which exists now rather than different definitions that have been introduced over the last few years?? What definition

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do the authors use here??. Please define.

Pg 19473: In 7: what is the 1-2 deg in km?? Ln 25: How does this difference in the treatment of advection affect convective transport?? lns 24-26: There are other differences between models which can also cause variability in the simulation of convective transport which should be mentioned (e.g.) use of online and offline convective mass fluxes as discussed in e.g. Rasch et al (1997) and Olivie et al (2004). In 29: This is not strictly true as tropical convection in CTMs and CCMs have been compared for some tropical regions e.g. Barret et al (2010). The novel aspect of this paper is that both small and large scale atmospheric models are compared, including NWPs, and this novel aspect should be made clearer in the introduction.

Pg 19474 In 12: The difference between the two sets of simulations being what ?? lns 15-17: "For this purpose .... first round of simulations". Does the reader really require such information to understand the findings of this study?? If so rewrite this sentence to improve clarity as currently the differences in both set of simulations are not explained. In 20: Maritime continent?? Is this around Indonesia?? Figure 1 shows that it is but the term Maritime Continent is rather vague and not a well known definition. Why not use SE Asia?? The geographical limits of the three tropical domains should be introduced here. lns 23-30: This paragraph needs to be re-written so that the reader understands that model output is what is being compared with the satellite observations.

Pg 19475 In 2-8: These details should be moved into either sections 2 or 3. In 8-10: This paragraph needs to be rewritten to improve readability.

Description of the model simulations:

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

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relevant information needed to digest the results presented in later sections. That the CTMs use offline dynamics and the smaller scale models use online dynamics should be significantly highlighted as this is important for the correct interpretation of similarities between models. The subset presented here is not completely conversant with the range of models included in Hoyle et al (2010) e.g. where is KASIMA and the CCM's?

Pg19476 In 2: But only a subset of the models are actually included rather than the full suite. "In the second round of the . . .". Why bother mentioning this?? This becomes confusing for the reader not involved in the actual study and I am not convinced they need to know this technical detail. If the "first round" of simulations are used in the second paper then why not define these as the first round instead. In 25: If these two models are virtually identical what is the motivation for using both results?? Perhaps as an internal check that they are nearly identical and have not diverged recently. The description states that the chemical component has changed but that will not effect the diagnostics used in this paper. Thus the authors should state that to be harmonious with the second paper inclusion is warranted (if this is indeed the motivation). 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.

Pg19477 I don't see details of how UMUKCA-UCAM_nud, UM-UCAM_highres and WRF calculate precipitation rates, which helps to understand the analysis presented in Sect 4.1.

Pg19478 Ln 26: But Water Vapour can be output from most CTMs quite easily and is commonly prescribed within the meteorological data used to drive the model and therefore not calculated online. I assume a WV field would also be available from the smaller scale models participating in the study. The use of the WV product seems to be unconnected to the model comparison which questions why it has been included in the paper (see general comments section).

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Observational datasets

Pg19479 Lns 7-8: Is there a valid reason why CATT_BRAMS only makes a token contribution to the results whereas the other NWPs manage at least four months?? It could be argued that this model should be removed from the comparisons as you cannot gauge the performance for the majority of the domains/months of the year (in fact your analysis of precipitation rates seems rather constrained by the availability of data from CATT_BRAMS). Ln 14: Which version of the GPCP dataset?? Hopefully this is the version of the data discussed in Huffman et al (2009).

Pg 19481 Lns 3-5: Some details of the quality of the cloud top products should be included using other studies available in the literature e.g. Ackerman et al (2008)

Results

Pg 19482 Ln 9: Accuracy of 20%?? Please provide a reference. Ln 19: Where is the Tropical Warm Pool region?? Provide Lat and Lon for clarity or a reference where an established definition has been given. Lns 20-24: You state you have chosen to use satellite measurements because of the seasonal and global nature of the datasets, which provide better statistical comparisons. Now you say the three regions you select are partly motivated by in-situ measurement campaigns. This is somewhat contradictory. Does paper 2 include in-situ comparisons?? Lns 25-27: This sounds a better motivation for selecting the three domains rather than lines 20-24. Lns 27-28: Please provide references to support this statement.

Pg 19483: Ln 1-3: Please provide references to support this statement.

Pg 19484: Ln 7-9: It shows the variability between satellite datasets selected in this paper rather than providing an assessment of the uncertainty (i.e. accuracy) of the data product. For that you need to compare against a truly independent dataset such as monthly mean averages of e.g. temperature and WV provided by e.g. the MOZAIC in-situ measurement dataset in the UTLS. The accuracy of such products is most probably

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available in the literature already and should be stated. Ln 14: How does the TRMM regional variability in the tropics for this year compare to previous studies e.g. Petersen and Rutledge (2001)??

Pg 19485: Lns 1-8: A more robust use of the literature should be used for the reader to be able to assess how good e.g. TRMM actually is. For instance, there have already been comparisons of TRMM precipitation retrievals vs ocean rain gauges (Bowman, 2005) which reveal some biases that could be included in the discussion. Moreover, previous comparisons of climatologies have been made between e.g. TRMM and GPCP which should be used in the discussion e.g. Adler et al (2009). Lns12-14: Please provide references to support this statement. Ln 17: The AIRS dataset could be coarsened and a further comparison made to prove this point. Ln 19: Given that each grid box is ∼100km x ∼100km are there a lot of cloud-free instances in the tropics in a monthly mean using a threshold of zero?? Ln 22: But the ISCCP data is used in the discussion further on in Sect 4.2 anyway. Therefore the corresponding values should be shown for the months which are available. Lns 24-30: How can you ensure that all the high cloud top heights are representative of convective clouds?? Hong et al claim that only ∼20% of high clouds in the tropics detected by MODIS are in fact deep convective clouds using the ISCCP classification scheme, with ∼80% being cirriform. Using the ISCCP climatology in the same fashion would significantly strengthen this part of the manuscript and the associated comparisons later on. Cirrus fields are typically diffuse and can occur far from regions which exhibit deep convection (Toon et al, 2010) therefore potentially exaggerating the strength of convective acivity. Another method would be to use the cloud optical depths from MODIS (where convective clouds have larger OD's). Large scale CTMS usually include cirrus parameterizations to account for e.g. the conversion of N2O5 into HNO3 on the available surface area. This issue needs to be addressed before cloud top height can be trusted as a good diagnostic for the performance of convective parameterizations in atmospheric models as used in this paper.

Pg 19486 Ln 6-13: Either include the entire African region as a domain or remove this paragraph. Ln 29: very small values?? Provide examples in ppm which have been measured in the TTL.

Pg 19487 Lns 2-6: This should be moved to the introduction as a motivation for including the WV product in the paper. Ln 15: This is the first time correlation co-efficients have been given for the comparison of two satellite products. Why for this product and not the precipitation rates and cloud top heights?? Lns 20-29: The correlation co-efficients between the AIRS and MLS WV products and other datasets vary with respect to the three chosen domains. The best correlation exists over South America and the authors postulate that this shows a net moistening of the TTL by convection above this domain, which is stronger than for the other domains. However, no discussion is included to explain the possible reasons for the worse correlation exhibited for the other domains. There was previously some mention of the impact of aerosols on the cloud-top height values from MODIS over West Africa and the symmetrical positioning of the Maritime Continent around the Equator. Is this influencing the correlation co-efficients maybe??

Pg 19488: Lns 3-7: If the vertical resolution issue has a dominating influence between instruments then surely the correlation co-efficient for AIRS should be worse across all domains compared to MLS?? Ln 10: Does the temperature distribution from MLS show the same artifact?? You argue that the MLS WV measurements support the hypothesis of moistening of the TTL by convection. Then you use the AIRS temperature data to support your point which seems a little illogical. Both AIRS and MLS temperature products have the same accuracy in the tropical TTL (Schwartz et al, 2008; Susskind et al, 1998). Ln 14-16: Somewhere in the text you should inform the reader that other authors have already proposed a link between deep convection and the moistening of the upper troposphere e.g. Hovath and Soden (2007). Lns 23-26: You already mentioned this grouping of results in a previous section so it doesn't need to be included here.

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Pg14989: Ln 4: What set of models?? CTMs or NWP?? Ln 4-7: Most probably this shows that the ECMWF meteorology is better constrained around Africa and SA compared to the Maritime Continent as this drives ~50% of the models, possibly due to the assimilation of more measurements between the different regions.

Pg 19491: The discussion here is flawed in that the authors assume that the differences in performance are solely due to model resolution whereas a subset of the models uses offline dynamics for precipitation from ECMWF, whilst another subset uses online dynamics which adopt different parameterizations (although the specifics are not currently provided in Sect. 2.). This will be as important to the model performance as the differences between the horizontal and vertical resolutions employed between models. The weakness of focusing on one region for one month is that the reader cannot asses whether a model generally has a tendency to over-estimate over land regions or not (i.e. the analysis is not currently statistically robust concerning the comparisons of precipitation rates). This could be presented as correlation plots of precipitation per grid cell for each region between the various datasets (i.e. re-bin the observations onto the horizontal resolution of each model) or differentiating between land and ocean for seasons DJF and JJA which would improve the validation of the models significantly. Ln 24: At this point I am not totally sure which models have the microphysics active due to the sparse details provided in Sect 2. I assume the CTMs do not (the micro-physics are implicitly in the ECMWF meteorology).

Pg 19492: Ln 4-24: This section of text discusses the comparison of the observational datasets once again and therefore should be moved to section 4.1. This motivates the authors to include the ISCCP monthly mean cloud top heights on the figures to provide a more robust comparison. Ln 9: convective outflow possibly away from the regions exhibiting deep convection. See my comments on differentiating between the fraction of cirrus clouds and deep convective clouds from MODIS above.

Pg 19493: Ln 2-5: The cloud top height is determined using the ECMWF geo-potential height (Pg19481) so how will this affect the comparisons against models which also

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use ECMWF meteorology (or conversely those that don't)??

Pg 19494: Lns 17-23: This summary is probably better in the conclusions.

Pg 19496: Lns: 27-29: This statement weakens the whole motivation for performing the analysis as presented here. What you are in fact comparing is the cumulative effect of all differences throughout the model ensemble, therefore the end products should be directly comparable. To fully understand why some models perform worse you would need to dig into the approaches to gain any insight I agree. Some mention of this would be useful in the introduction.

Pg19497: This discussion could be significantly improved by introducing reasons as to why some algorithms are performing worse. This would need section 2.1 to contain details of what is going into each algorithm and the parameters which the performance is most sensitive. This is possibly already available in the literature regarding each model but needs to be summarized here to aid the reader.

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.

Grammar, Typos and Spelling

Pg 19471: ln 24: "feature" seems inappropriate. Replace with "process". ln 26: ".. has long been debated ..". Replace with e.g. "has been debated extensively in the literature ".

Pg 19472: ln 8: frequencies are typically either low or high rather than "very small". ln 10: "tropopoause" ln 11: 8-1 km?? Must be a typo. ln 15: " make use of a parameterization scheme" should be " use parameterization schemes".

Pg 19473: ln 3: 'tracer transport' can only be accounted for in models which actually

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include tracers therefore remove 'with tracers' ln 9: replace 'determining' with 'influencing'. ln 10: Are capitals necessary for Limited Area ?? ln 14: "... and, as a result, ..". ln 15: " Therefore, these types of models typically ... ". ln 23: " to the vertical and horizontal resolutions applied in any model." ln 25: Insert a comma after "resolute".

Pg 19474: Ln 1: This paragraph should be improved upon. E.g. "This paper is the first part of a two part study focusing on tropical tracer transport as part of the (define acronym) recent SCOUT-O3 EU-integrated project." ln 10: Define what SCOUT-O3 stands for.

Pg 19475: ln 18: "CTM's" ln 26: "model's"

Pg19476: ln 3: why use paranthesis?? Just expand the sentence e.g. "follows, where more comprehensive details are provided in the relevant literature." ln 5 and 18: You already defined CTM as an acronym in the introduction so it should be adopted throughout the entire manuscript rather than flipping between the two as is done in this section. ln 24: Missing paranthesis around (1989).

Pg 19477: ln 3: "the surface moisture".

Pg 19479: Ln 17: Quasi-Global?? Ln 19: Define GOES.

Pg 19480: Lns 1-3: The acronyms should be given in brackets after the full definition of the acronym e.g. Global Precipitation Index (GPI). Ln 9: Move "since July 1983" to the start of the sentence.

Pg 19481: Ln16: The acronym AIRS has already been defined previously so should be used here.

Pg 19482: Ln 1: Is nearly-global the same as quasi-global??

Pg 19484: Ln 1: "... on average more convection ..." should be "stronger convective activity". Ln 16: "Africa" should be "Central Africa". Also correct other instances throughout the text.

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Pg 19486: Ln 7: “sub-equatorial” should be replaced by e.g. “southern” to improve clarity. Ln 14: remove “,” before “suggests”. Ln 28: “Water vapour can be thought of as a tropospherically-abundant tracer”. Please remove hyphen and rephrase. Ln 29: Remove hyphen in “temperature driven”.

Pg 19488: Ln 19: “strong temperature control”?? Do you mean temperature gradient or the limitation of the vertical transport of WV due to condensation/freezing??

Pg 19489 Ln 14: Move “reasonably well” to after “surface precipitation”. Ln 17: The sentence “A few . . . West Pacific” should be removed. Figure 4 shows 50-100% differences which does not correspond to what is said by this sentence. Ln 20: “Model’s” Ln 24: You already say this at the start of the paragraph so remove first sentence of this paragraph.

Pg 19490 Ln 21: remove “similar to that used by most of the models in this study”

Pg 19490-19492 This long paragraph should be broken up a little around ln 8.

Pg 19493 Ln13-16: Avoid repetition. Should be truncated to :“ One possible explanation is that the coarse resolution models . . .”

Pg 19494 Ln 28: Sect 4.1??

Pg 19495 Ln 15: “clod”

Pg19497: Ln 19: New paragraph after (Feng et al, 2010).

Table 1:

Details relating to pTOMCAT_tropical should be included in the table. Figure Legends;

Fig 1: -20ïĆřS?? This is actually 20ïĆřS. E and W should be included for longitude rather than e.g. -80ïĆř. Fig 2: NOOA should be NOAA. Fig 7: Remove “(all months in 2005)”

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