

Response to Reviewer #1 of gmd-2019-104

Dear Reviewer #1,

Thank you very much for taking your time to review our paper. We think that your comments greatly help improve the manuscript. We have revised the manuscript according to your comments as explained below with point-by-point responses to your comments. We hope that the revision is enough to address your comments to make the manuscript now acceptable for publication in *GMD*.

[RC]: Referee comment

[AC]: Author comment

Reviewer #1:

[RC] *The authors describe enhancements to the COSP satellite simulator package intended to aid in model development and evaluation of precipitation processes. New diagnostics on subcolumn fields permit better and easier comparison with satellite datasets, and the paper provides an excellent example of this capability using A-Train data.*

This paper is clearly written, concise, well referenced, and should provide a useful guide for users of these tools in the future. I found it well organised and clear, and have only minor recommendations for ways to improve the manuscript, which are mostly textual in nature. It suits the scope of this journal and should be suitable for publication after some minor modifications.

[AC] We would like to thank the referee #1 for his/her positive and constructive comments. The reply and corrections on individual comments are below. Note here that, page and line numbers denoted in the authors' responses below correspond to the track-changes file, not original manuscript.

Minor points:

[RC1] *Title and throughout – The authors use ‘warm-rain’ consistently with a hyphen, whereas elsewhere in the literature it is usually not hyphenated (e.g. Stephens and Haynes 2007, Chen et al. 2011, Suzuki et al. 2011). I would suggest changing this to match the literature. In the specific case of the title, ‘warm-rain’ could be replaced by ‘precipitation.’*

[AC1] We have removed the hyphen, and unified as “warm rain” throughout the manuscript. The title still uses “warm rain” not “precipitation”, because the present study is limited to liquid-phase cloud microphysics and inline diagnostics for mixed- and ice-phase cloud microphysics are also planned to be introduced as future work, as described in section 4.

[RC2] *P1L1 - ‘has been widely used’ contains a value judgment, and could be simplified to ‘is used’*

[AC2] We have modified, thanks.

[RC3] *P3L2 – This is one example of this manuscript’s slight tendency for overdoing the number of citations in some places. Here and elsewhere it may be better for readers if the authors select one or two of the most important and relevant citations, rather than a half dozen.*

[AC3] We agree with the reviewer to avoid citing too many papers. Accordingly, we have selected the most important citations, throughout the revised manuscript.

Page 2, Line 2: removed Wood (2012).

Page 2, Line 8: removed Michibata et al. (2016); Bai et al. (2018).

Page 2, Line 25: added Maloney et al. (2019)

Page 3, Line 15: removed Tompkins and Di Giuseppe (2015), Norris and da Silva (2016), Griffin and Larson (2016), and Ovchinnikov et al. (2016).

Page 5, Line 1: removed Bennartz (2007).

Page 5, Line 23: added Michibata et al. (2019).

Page 7, Line 28: removed Kubar et al. (2009); Sorooshian et al. (2013).

Page 8, Line 7: removed Michibata et al. (2016).

Page 8, Line 27: added Mülmenstädt et al. (2015); Kikuchi et al. (2017)

[RC4] P3L10 – *I found the beginning of this section to be quite abrupt, immediately discussing CFADs without putting this into context. Given that the section is titled ‘Concept and design’ it might be useful for readers if the overall concept is stated before going straight into the details. Perhaps a short paragraph before Section 2.1 begins, or a sentence to lead into why CFADs are then discussed. This is done in a roundabout way later in this first paragraph, implying that such diagnostics are useful for fingerprinting processes. It would read better if this were rearranged a little.*

[AC4] We agree that this paragraph requires clarification, and have rearranged sections 2 and 2.1 in the revised manuscript.

Section 2: added overall concept in this work as follows:

“The objective of this work is to provide a specific “process-oriented” metrics that is also compatible with “scale-aware” and “definition-aware” diagnostics (Kay et al., 2018) in the manner implemented into COSP for fair comparison of warm clouds among GCMs and satellite retrievals. Here the main concept is using conditional statistics that “fingerprint” the process of interest, by combining multiple satellite observables. One of the transformative advances recently made possible by combining active and passive satellite measurements is the ability to generate observational diagnostics of how the microphysical vertical structure of clouds varies with the surrounding environment (Marchand et al., 2009; Sorooshian et al., 2013), such as aerosol concentration (Ma et al., 2018; Rosenfeld et al., 2019) and dynamical regimes (Nam et al., 2014; Christensen et al., 2016).

As a default diagnostic from the CloudSat radar simulator alone in COSP (Bodas-Salcedo et al., 2011), the so-called contoured frequency by altitude diagram (CFAD) is prepared to provide macrophysical vertical structure including all types of hydrometeors (i.e., liquid droplets, ice crystals, raindrops, and snowflakes). In this regard, more specific statistics are useful when investigating a particular process, including the warm rain microphysical processes that are the focus of this work as described below.”

Section 2.1: In accordance with the response above, some sentences were rearranged.

[RC5] P4L12 – *‘A-Train’ should be defined, either here or at its first mention (P3L6), preferably with a citation to one of the relevant Stephens or L’Ecuyer papers.*

[AC5] We have changed the sentence at Page 2 Line 9 from ‘Global satellite observations, particularly those of satellite constellations’ to ‘The A-Train global observations (Stephens et al., 2002; L’Ecuyer and Jiang, 2010), consisting of the sun-synchronous and polar-orbiting multisatellite constellation, ...’.

[RC6] P4L28 – *This section title might be better without ‘scientific perspectives’ in there, as this is quite vague.*

[AC6] We have changed the section title to “Examples of model–observation intercomparisons”.

[RC7] P4L31 – *Stay consistent, either refer to COSP or COSP2.*

[AC7] The revised manuscript uses “COSP”, except the case that the sentence does not mean the latest version of COSP.

Page 5 Line 29: “COSP2” to “COSP”

Page 8 Line 20: “COSP2” to “COSP”

[RC8] P5L6 – *Please state which specific data products were used including which version or processing level, as this is more important for readers’ interpretation than the original papers cited here.*

[AC8] We have added more detailed description for the A-Train datasets used as follows.

Page 6 Line 3: “As a reference, we also calculated the target metrics (i.e., the occurrence frequency of SLWCs and CFODDs) using CloudSat and MODIS satellite data products (e.g., Stephens et al., 2008)

for the period June 2006–April 2011. The visible cloud optical depth and 2.1 μm cloud droplet effective radius were derived from MODIS level 2B-TAU R04 product (Polonsky, 2008), radar reflectivity profile was obtained from CloudSat-derived level 2B-GEOPROF R04 product (Mace et al., 2007; Marchand et al., 2008), and the pressure and temperature profiles were derived from the ECMWF-AUX R04 product (Partain, 2007). Detailed descriptions of the model configuration and the analysis procedure to detect SLWCs are provided elsewhere (Michibata and Takemura, 2015; Michibata et al., 2016).”.

[RC9] *P5L16 – I disagree with the causative statement here, saying that because the model generated more SLWCs this means that the chosen period is good enough for robust statistics. This could be rewritten to say that it is indeed a long enough period (which I agree with), but that is not proven by the fact that the model had more SLWCs, which is what the current text suggests.*

[AC9] Yes, we agree to the comment. This sentence has been modified as follows: “... present in the A-Train observations. This suggests that one full-year of simulation with 3-hourly diagnosis is long enough, but note that this does not negate the possibility of too frequent generation of SLWCs in the model.”.

[RC10] *P5L19 – This could be clearer, as saying that ‘MIROC6 overestimates ... by 15%’ can be misleading. Please say what the reference is, or it might be best if just stating that MIROC6 finds 48.5% drizzle versus 33.3% in A-Train data. What I mean is, if the A-Train data are taken as truth, then MIROC6 overestimates drizzle by almost 50% relative to the A-Train data, so it’s better to write what is meant explicitly so that it can’t be misinterpreted.*

[AC10] Thank you for suggestion. This sentence has been modified as follows: “The MIROC6 finds 48.5% drizzling regime versus 33.3% in the A-Train retrievals (Figs. 2b and 2e).”.

[RC11] *P5L21 – I disagree with the authors’ interpretation that the model’s ‘geographical pattern’ of precipitation is in good agreement with observations. I would suggest that this statement should be modified or given some caveats at least, since the patterns in the tropical oceans and continental precipitation seem quite different in the figure.*

[AC11] We agree with the reviewer. The geographical pattern simulated in MIROC6 is different from observations mainly over the tropical oceans and continents. We have noted the model bias in the revised manuscript as follows: “For precipitating regime, although the global mean values of occurrence frequency are consistent with each other (15.9% in MIROC6 and 17.4% in A-Train), the geographical pattern is quite different particularly over tropical oceans and continents (Figs. 2c and 2f), implying that the model has biases in the warm rain formation process (e.g., Jing et al., 2019) and/or the representation of cloud types (e.g., Huang et al., 2015).”.

Page 7 Line 3: changed “rain formation processes” to “aerosol–cloud interactions”.

[RC12] *P6L6 – It is implied here that effective radius and the subdivisions of Re used in the analysis are related to whether or not clouds are precipitating. This is surely spelled out in some of the referenced literature, but a sentence or two stating this explicitly would be useful for readers. A reference to Lebsock et al. 2008 might be helpful.*

[AC12] This sentence has been changed as follows: “... regimes as a function of Re , and is consistent with previous observational findings that showed the strong dependence of the onset of precipitation upon Re (Lebsock et al., 2008; Rosenfeld et al., 2012). On the other hand, MIROC6 simulates higher ...”.

[RC13] *P7L15 – Reword ‘by more simple way’*

[AC13] This has been changed to “... in GCMs more effectively and simply.”.

[RC14] *Fig 1 – The use of double quotes to show emphasis (How “often” does it rain) should be replaced by a switch to regular/italic font, or removed.*

[AC14] The double quotes in Fig. 1 have been removed in the revised version. We have also corrected the typo in sampling method in this figure (and also Page 5 Line 9), “fracout = 2” to “fracout = 1”, which means stratiform cloud type. The results analyzed are not changed.

[RC15] *Fig 4 – Please state in the caption whether the colour scale used is identical to those in Fig 3. If not, please provide a colour bar in the figure.*

[AC15] The color scale is the same as in Fig. 3. This has been noted in the caption in Fig. 4.

Thank you very much again for reviewing our paper.

Sincerely yours,

Takuro Michibata