

**Re-review of “Ice injected into the tropopause by deep convection – Part 2:
Over the Maritime Continent” by Dion et al.**

The authors have made extensive revisions in response to the comments of the two referees. In some cases, reviewer comments have been fully addressed and the manuscript has been improved. Unfortunately, however, there are many instances in which that is not the case. As was the original, the revised manuscript is again very sloppily prepared, with numerous errors – not only minor typographical mistakes, but also more serious misstatements and inaccuracies that the authors (including co-authors) should have caught themselves had they carefully proofread their work before submission. Moreover, in at least half a dozen places the authors did not actually enact the changes in the revised text that they assert they have made in their response letter, so several issues pointed out by the referees in the previous draft have not been remedied in the revised manuscript. In addition, new errors have been introduced through the revision process. As a result, the manuscript remains confusing and hard to read and still requires considerable editing and correction before it can be considered ready for publication. The list of detailed comments is therefore longer than is typically the case for a revised manuscript. Finally, although I recommend some minor typographical corrections below, I would also like to reiterate that the final manuscript will need extensive copy-editing to improve the English.

Specific comments and questions (both major and minor comments on each Section are listed together here, rather than separately as in the review of the original manuscript):

Abstract:

- (1) L8: delete the comma after “Version 4.2””; measurement from --> measured by.
- (2) L12: resolutions --> resolution.
- (3) L13: (ΔIWC^{ERA5}) degrading the vertical resolution to that of MLS --> (ΔIWC^{ERA5}), with the vertical resolution degraded to that of MLS.
- (4) L14: lands and --> land but.
- (5) L17-18: Where do the values (4–29%, 55–78%) quoted in these lines come from? They appear nowhere else in the manuscript. In particular, they do not match any of the numbers given in the relevant discussion in Section 7.2 or 7.3.
- (6) L19-21: See point #7 in the comments on Section 7.2 about the phrase “xx% of variability per study zone”, which is used four times in these lines.
- (7) L21: See point #4 in the comments on Section 7.3 about these values (0.6 and 3.9 mg m⁻³).
- (8) L22: found larger than --> found to be larger than.
- (9) L22-25: It is stated that ΔIWC over land is larger than ΔIWC over sea “with a limit at 4.0 mg m⁻³ in the UT”. What does “limit” mean in this context? Where does the value of 4.0 mg m⁻³ come from? In Section 7.1, the minimum ΔIWC over land is given as 4.9 mg m⁻³ and the maximum over sea as 4.4 mg m⁻³, leading to a maximum difference of only 0.5 mg m⁻³.
- (10) L24: strongest amounts --> largest amounts.
- (11) L25: It is stated that Java sees the largest ΔIWC in the UT (“7.7 – 9.5 mg m⁻³ daily mean”). Again, these numbers are not drawn from the main text. Indeed, in Section 7.1, it is stated that “In the UT ... over Java, ΔIWC reaches 7.9–8.7 mg m⁻³”.

Introduction:

- (1) L54: center in the tropics with --> centers in the tropics, with.
- (2) L57: though authors have done a comprehensive work around the study of --> though those authors have done a comprehensive study of.
- (3) L67-68: To avoid unnecessary repetition: improve the methodology of Dion et al. (2019) --> improve their methodology.
- (4) L69: assessing --> comparing.
- (5) L72: Here and in 11 other lines throughout the manuscript, "Bismark" is spelled incorrectly (my apologies for not noticing this error in my original review). Only in L510 in Section 8 is it spelled correctly: "Bismarck".
- (6) L78: recalled --> reviewed.

Section 2.1: The MLS description has been improved but still needs work:

- (1) The sentence in L96-98 ("Although optimal estimation is used to retrieve almost all other MLS products, a cloud-induced radiance technique is used to validate the MLS IWC (Wu et al., 2008; Wu et al., 2009).") should be moved to become the second sentence in the paragraph.
- (2) In the above-mentioned sentence, the word "validate" should be replaced by "derive".
- (3) Neither of the Wu et al. papers cited in that sentence have been added to the reference list.
- (4) L89: Either delete "provides" or delete "are given at".
- (5) L89: Add the units "hPa" after 215.
- (6) L93: tropopshere --> troposphere.
- (7) L94: instrument is crossing twice a day the equator at fixed time --> instrument crosses the equator twice a day at fixed times.
- (8) The sentence in L95-96 ("The horizontal resolution of IWC^{MLS} measurements is ~300 and 7 km along and across the track, respectively.") is fully redundant with information given below and can be deleted.
- (9) L99: allowing to average the IWC^{MLS} measurements within the bins --> allowing the IWC^{MLS} measurements to be averaged within bins.
- (10) L101-103: resolutions ... are --> resolution ... is
- (11) L104-105: The valid ranges quoted for 146 and 100 hPa are switched: 0.02–50.0 mg m⁻³ applies to 100 hPa, while 0.1–50.0 mg m⁻³ applies to 147 hPa.

Section 2.2: The revised TRMM-3B42 description needs clarification:

- (1) L109: Either delete "altitude" or delete "height".
- (2) L111-112: First, "composing" is not the right word in L111. Second, these two sentences seem to be contradictory. The first states that TRMM-3B42 is "a multi-satellite precipitation analysis" but then mentions only GPM. The second states that it is "computed from the various precipitation-relevant satellite passive Microwave (PMW) sensors" and then lists several, not including GPM. These two sentences should be combined and clarified. Third, why is "Microwave" capitalized? It's part of the acronym, but so is "passive".
- (3) Neither Huffman et al. (2010) nor Huffman and Bolvin (2018) have been added to the reference list.

- (4) L121-122: I am still confused by the description of how 1-hour precipitation data are obtained. As I said in my previous review, I am under the impression (based on Huffman et al. (2007) and other sources) that the TRMM-3B42 product contains gridded merged precipitation estimates with a 3-hour temporal resolution. I had thought that only by taking advantage of the precessing orbit of TRMM and the long study period (13 years) are the authors able to bin the data in 1-hour bins. Perhaps that is what they are getting at with these two sentences, but it is not clear. “The granule temporal coverage of TRMM-3B42 data is 3 hours, but the temporal resolution of individual measurements is 1 minute. Thus, it is statistically possible to degrade the resolution to 1 hour.” seems to be saying that the 1-minute resolution of the individual measurements is somehow preserved in the 3-hourly averaged TRMM-3B42 data. In addition, since they are starting from data with a 3-hour granularity, it is confusing to talk about “degrading” the resolution to 1 hour.
- (5) L122: TRMM-3B42 are provided --> TRMM-3B42 data are provided.

Section 2.3: The revised TRMM-LIS description needs considerable clarification:

- (1) L126: relative to --> related to.
- (2) L127: Christian et al. (2000) has not been added to the reference list.
- (3) L127: event --> events.
- (4) L129: delete “a” before “spatial”.
- (5) First it is stated (L130-131) that the “instrument detects lightning with storm-scale resolution of 3–6 km (3 km at nadir, 6 km at limb) over a large region (550–550 km) of the Earth’s surface”. I have no idea what “over a large region (550–550 km) of the Earth’s surface” means. Then in the next sentence (L131) it is stated that “LIS horizontal resolution is provided at 0.25°×0.25°.” Are these two sentences consistent? Finally, it is stated several lines below (L138-139) that “The observation range of the sensor is between 38N and 38S.” The latitudinal coverage and spatial resolution of the LIS data should be described together in sentences that logically flow from one to the next.
- (6) RTEP was already defined in L127, so the typo in L134 should be fixed by replacing “real-tile event processor” with “RTEP”.
- (7) L134: to enable the system and detect --> to enable the system to detect.
- (8) First it is stated (L133-134) that “the weak lightning signals that occur during the day are hard to detect because of background illumination”. Then it is stated (L134-135) that processing to remove the background signal allows the instrument to “detect weak lightning and achieve a 90% detection efficiency during the day”. The next line (L136) states that the “TRMM LIS detection efficiency ranges from 69% near noon to 88% at night”, appearing to contradict the previous sentence. Does the detection efficiency during the day really reach as high as 90%, higher than at night?
- (9) L140-141: is studied at --> is binned to.

Section 2.4:

- (1) L147-148: atmosphere through a large number --> atmosphere through combining a large number.
- (2) L154: into the TTL --> in the TTL.
- (3) L154: Duncan and Eriksson (2018) has not been added to the reference list.

- (4) L160: cloud and precipitations are used as control variable --> clouds and precipitation are used as control variables.
- (5) L165: affects --> affect.
- (6) 169: I'm not sure what information "unitary" is meant to convey here, or why this word is needed. In any case, "an unitary" should be "a unitary".

Section 4.1:

- (1) L200: Somehow the authors are under the impression that I suggested that "the New Guinea" be changed to "NewGuinea", which is not the case. I was merely asking for "the" to be deleted. Here and elsewhere in the manuscript a space should be added: "New Guinea".
- (2) L203: depending of --> depending on.
- (3) L204: could illustrate the impact of the land/sea breeze within the 24 hours --> may be related to the impact of the land/sea breeze over the course of 24 hours.
- (4) L205: land temperature surface is higher than oceanic temperature surface --> land surface temperature is higher than oceanic surface temperature.
- (5) L206-207: sea surface temperature becomes larger than the land surface temperature and the land breeze favours systematically the convection development over coast. These observations are consistent with results presented in Qian (2008), explaining that ... --> sea surface temperature rises above the land surface temperature, and the land breeze systematically favours the development of convection over coasts. These observations are consistent with results presented by Qian (2008), who explained that

Section 4.2: This section requires editing and reorganization to improve the clarity and flow.

- (1) For one thing, it would be easier to follow if the Prec panels were described in detail first and then the similar panels for IWC mentioned; thus, the sentence in L219-221 should be moved. In addition, the description of the calculation of the anomalies in the response letter is actually clearer than that in the revised manuscript. In particular, I find the sentence in L221-222 awkward and difficult to understand. The wording in other places is clumsy as well. Therefore, I suggest rewriting much of this paragraph, such that L217-225 are replaced by: "... each pixel. Figures 3a and b present the anomaly (deviation from the mean) of Prec measured by TRMM-3B42 over the MariCont for the pixels where convection is in the growing phase at 01:30 LT and 13:30 LT, respectively. Anomalies are calculated relative to the average computed over the entire MariCont region. Thus, red colors signify regions that are experiencing the growing phase of convection and whose Prec value is greater than the overall MariCont mean at the respective time (01:30 LT or 13:30 LT), whereas blue colors signify those regions where there is little precipitation compared to the overall MariCont mean during the growing phase of convection. The gray color denotes pixels for which convection is not ongoing. Pixels can be represented in the panels for both local times when: 1) the onset of the convection is before 01:30 LT and the end is after 13:30 LT, or 2) the onset of the convection is before 13:30 LT and the end is after 01:30 LT. Similar anomalies of IWC measured by MLS over the MariCont are shown in Figs. 3c and d, over pixels when the convection is in the growing phase at 01:30 LT and 13:30 LT, respectively. Note ..."
- (2) L225: whithin --> within.

(3) L227 states that the Prec anomaly “varies between -0.15 and $+0.15$ mm h^{-1} ”. The corresponding range for the IWC anomalies is not given until L231-232, but intervening sentences discuss both Prec and IWC anomalies. Therefore, the sentence in L231-232 stating the IWC range should be moved to after the Prec anomaly statement in L227.

(4) L227-228: The growing phase of the convection over land is mainly at 13:30 LT --> At 13:30 LT, the growing phase of the convection is found mainly over land (I suggest this because the parallel structure with the 01:30 LT sentence below helps the reader follow the discussion.)

(5) L229: I was confused when I first read this sentence, because “north of Australia” sounds like it is referring to offshore regions. This sentence is specifically talking about land, so this should be “northern Australia”.

Section 4.3: In some places in this section the revisions have made the writing less clear, and other points also require clarification/correction.

(1) As in the preceding point, the wording in L242 makes the discussion pertaining to land confusing. South of Sumatra, Sulawesi, North of New Guinea and North of Australia --> Southern Sumatra, Sulawesi, northern New Guinea, and northern Australia.

(2) In L242, the amount of IWC injected over seas is stated to be < 10 mg m^{-3} . In L244, the largest amount of IWC injected over seas is stated to be $7-15$ mg m^{-3} , contradicting the first statement.

(3) L245-246: The units on the Prec anomaly of 0.2 should be mm h^{-1} , not mg m^{-3} .

(4) L245-246: It is not necessary to point to Fig. 3a or Fig. 3c here since this sentence is specifically talking about 13:30 LT.

(5) L249: pattern ... come --> pattern ... comes.

(6) L250-251: I suggest rewriting this sentence as “The differences in the magnitudes of the ΔIWC values at 100 and 147 hPa arise from the different amounts of IWC measured by MLS at those two levels. That is, similar ...”

(7) L252: the deep convection --> deep convection.

(8) L260: diurnal cycle of Prec with low value of Prec --> diurnal cycle of Prec, with low values of Prec.

(9) L264: South of Sumatra --> Southern Sumatra.

(10) L267: It is stated that “For pixels with large values of ΔIWC , IWC observed by MLS is between 4.5 and 5.7 mg m^{-3} over North Australia Sea, South Sumatra and New Guinea”. Fig. 5b shows North Australia, not the North Australia Sea.

(11) As I pointed out in the original review, the above statement applies only to New Guinea point #1. It is not true for New Guinea point #2, for which the IWC value is much lower.

(12) For the pixels with low ΔIWC , the IWC^{MLS} ranges from 1.9 to 4.7 mg m^{-3} . Thus the highest IWC for these points is larger than the bottom of the range of the high- ΔIWC points. I note that the revised summary in this section (L269-270) mentions the length of the growing phase of deep convection and the amplitude of the diurnal cycle in Prec as factors associated with large ΔIWC values, but not the magnitude of the IWC values themselves, even though the summary immediately follows discussion of the MLS IWC values. I appreciate that such a statement has been removed in revision. I agree with that deletion and I am NOT suggesting that it be put back in. However, it might strike readers as odd to spend several sentences (L266-269) talking about the MLS IWC values and then end that paragraph with a summary that completely

ignores them. The authors should consider adding a sentence or two noting that the IWC^{MLS} ranges overlap for the high and low Δ IWC pixels, and thus no definitive conclusion about the relationship between IWC and Δ IWC can be drawn.

(13) Furthermore, language linking high- Δ IWC points to high ($> 4.5 \text{ mg m}^{-3}$) IWC values has not been removed from the Conclusions section (L539-540).

(14) In my original review, I suggested that it would be easier to read values off of Fig. 5 if the y-axis for Prec had 4 (not 3) minor tickmarks, as the IWC axis does. I still feel that way.

Section 5: L273: into --> in.

Section 5.1:

(1) L281: I think “similar” would be better than “the same”.

(2) L282: North Australia lands --> northern Australia.

(3) L284: It would be better to use either “~” or “>”, not both.

(4) L285: It is stated that “over NewGuinea where the number of Flash is relatively low ($\sim 10^{-2} - 10^{-3}$ flashes day⁻¹)”, but I think there are several inland areas of New Guinea where Flash exceeds 10^{-2} flashes day⁻¹.

Section 5.2: Again in this section the revisions have made the writing less clear in places, and other points also require clarification/correction.

(1) L296: choice of 5 pixels has been taken applying some sensitivity tests --> choice of 5 pixels was made after consideration of some sensitivity tests.

(2) L298: offshore pixels defined as sea pixels excluding 10 pixels (2000 km off the land) over the sea from the land coasts, thus coastline --> offshore pixels defined as sea pixels excluding 10 pixels (2000 km) over the sea from the land, thus coastline.

(3) L300-302: These sentences (“In that case, we can easily discriminate between land and coastlines or sea and coastlines by applying the land/ocean/coastlines filters. Consequently, this particular pixel will be flagged both as land and coastlines or sea and coastlines”) are awkwardly worded, hard to follow, and appear to contradict previous statements. I understand that pixels can straddle land/coastline and sea/coastline boundaries. I believe that this description is saying that a pixel containing both land and coastline information will be “bookkept” in both the MariCont_L and MariCont_C categories. However, as noted above, in L298 offshore pixels are defined in such a manner as to exclude the 10 pixels nearest land, so I do not see how in this context there could be any sea/coastline confusion. The areas within 5 pixels from land are put in the MariCont_C category, and those more than 10 pixels away are put in the MariCont_O category; presumably the areas stretching between 5 and 10 pixels from land are omitted from this part of the analysis.

(4) The Flash lines in Fig. 7 are described in the caption as being “dashed”, but in actuality they are dotted. This causes confusion when the reader gets to Figs. 8 and 9 and expects the similar Flash curves to also be dotted. However, in those plots Flash lines are dashed and IWC^{ERAS} lines are dotted. For consistency, it would be better to depict Flash data in Fig. 7 with dashed lines.

(5) As I mentioned in my previous review, it would be very helpful to have more minor tickmarks on both the x- and y-axes in Fig. 7 (as well as Figs. 8 and 9).

(6) L307: longer times --> longer.

(7) L310: Fig. 7c --> Fig. 7b.

(8) It is stated (L310-311) that “Prec minimum is around 18:00 LT”. It is then asserted (L312-314) that “These results are consistent with Mori et al. (2004) showing ... a diurnal minimum of precipitation around 11:00 LT”. To me, 18:00 LT is not consistent with 11:00 LT.

(9) L317: Fig. 7b --> Fig. 7c.

(10) L324-326: Consistently, model results from Love et al. (2011) have shown the suppression of the deep convection over offshore area in West of Sumatra from the early afternoon due to downwelling wavefront highlighted by deep warm anomalies around noon. According to the authors ... --> Consistent with that picture, model results from Love et al. (2011) have shown the suppression of deep convection over the offshore area west of Sumatra from the early afternoon due to a downwelling wavefront characterized by deep warm anomalies around noon. According to those authors

Section 5.3:

(1) L344: 6-h consistent with that of Flash and elsewhere, the duration --> 6-h, consistent with that of Flash, whereas elsewhere the duration.

(2) L346: Prec (6 h), that is --> Prec (6 h), which is.

(3) L357-360: In the case of this sentence, the authors did respond to my comments on the previous draft by making a change elsewhere, but they made no alterations to the sentence in question. The statement is made that “The diurnal maxima of Prec found separately over the 5 islands of the MariCont are much higher than the diurnal maxima of Prec found over tropical land (South America, South Africa and MariCont_L) from Dion et al. (2019)”. I am wondering whether these differences in diurnal maximum values are attributable at least in part to the much greater horizontal resolution used in the present study. Thus I suggest that the resolution information along with some other qualifiers be added here: “The diurnal maxima of Prec found separately over the 5 islands of the MariCont (at 0.25°×0.25° resolution) are much higher than the diurnal maxima of Prec found over broad tropical land regions (South America, South Africa and MariCont_L, at 2°×2° resolution) from Dion et al. (2019)”.

(4) L361: a diurnal cycle of Prec and Flash either coastline or offshore areas depending on the area --> a diurnal cycle of Prec and Flash similar to that of either coastline or offshore areas depending on the region considered.

(5) L362: Fig. 7c --> Fig. 7b.

(6) L373: $0.1 \times 0.2 \cdot 10^{-3}$ --> $0.1 - 0.2 \cdot 10^{-3}$.

(7) L376: over island --> over islands.

Section 6:

(1) Both referees questioned whether, given that IWC^{ERA5} is itself unvalidated at this point, ΔIWC^{ERA5} can really be used to validate the observationally derived values, or might this study rather in some sense serve to use the ΔIWC^{Prec} and ΔIWC^{Flash} to validate the new ERA5 values. The authors responded in their reply letter that they are not using ΔIWC^{ERA5} to validate ΔIWC^{Prec} and ΔIWC^{Flash} but to “assess the amounts estimated by our model”, and they have added to the manuscript (L384-386) the sentence “The diurnal cycle of IWC over the MariCont from ERA5 will be used to calculate ΔIWC from ERA5 in order to assess the horizontal distribution and the amount of ice injected in the UT and the TL deduced from our model combining MLS ice and

TRMM Prec or MLS ice and LIS flash.” To me, there is no difference between “assessing the distribution and amount of” the ΔIWC deduced from their model and “validating” it. I still feel that it would be appropriate to acknowledge that ERA5 IWC data cannot be considered “truth”, their quality has not yet been fully evaluated, and the consistency or lack thereof found in the comparisons between ΔIWC^{ERA5} and both ΔIWC^{Prec} and ΔIWC^{Flash} may have implications for both their methodology and ERA5.

(2) L388: consistently --> consistent.

(3) In both L389 and L390, the maximum value reached by IWC^{ERA5} in the UT is stated to be 6.4 mg m^{-3} . But I wonder where the authors are getting that number from. Clearly there are values larger than 6.4 mg m^{-3} in Fig. 10a (as evidenced by the white patches in the middle of the reddish colors).

(4) L393: Java, that is --> Java, which is.

(5) L402: consistently with the diurnal cycle of Prec and ... --> consistent with the diurnal cycle of Prec, and ...

(6) maximum ... are --> maximum ... is.

(7) L408: it is stated that the differences in the timing of the maximum of the diurnal cycle of Prec, Flash and IWC^{ERA5} “do not impact on the calculation of the ΔIWC^{Prec} , ΔIWC^{Flash} or ΔIWC^{ERA5} .” Is the timing unimportant because only the magnitude of the diurnal cycle (max-min) matters for the ΔIWC calculation? There is also a pervasive lack of superscripting in this sentence.

(8) L409: presented Section 7 --> presented in Section 7.

Section 7.1:

(1) It is stated (L418) that over Java, ΔIWC “reaches $7.9\text{--}8.7 \text{ mg m}^{-3}$ ”. This sentence pertains to only two values; thus, I presume that $\Delta IWC^{Prec}=8.7$ and $\Delta IWC^{Flash}=7.9 \text{ mg m}^{-3}$. In that case, $\Delta IWC^{Flash}-\Delta IWC^{Prec}=0.8 \text{ mg m}^{-3}$.

(2) The sentence in L419-420 is not complete: “except for Java where is larger than ΔIWC^{Flash} by 0.7 mg m^{-3} (-8%)”.

(3) Assuming that the above sentence should read “except for Java where ΔIWC^{Prec} is larger than ΔIWC^{Flash} ”, then the difference is 0.8 mg m^{-3} , not 0.7 as stated.

(4) Since no sign is attached to the raw difference in L420, why is the percent difference negative? It doesn’t make sense to say that “ ΔIWC^{Prec} is larger than ΔIWC^{Flash} by -8%”.

(5) Furthermore, the percent difference as defined here should be $0.8/7.9=10\%$, not 8% as stated (L420).

(6) More fundamentally, why did the authors choose to use ΔIWC^{Flash} in the denominator for the percent differences? ΔIWC^{Flash} is no more “correct” than ΔIWC^{Prec} . Thus, a perhaps less arbitrary approach would have been to use the average of the two ΔIWC estimates in the denominator rather than picking one of them.

(7) L422: “ ΔIWC^{Flash} is almost twice as large as than ΔIWC^{Prec} (53%)” – to me it looks like ΔIWC^{Flash} is $\sim 4.3 \text{ mg m}^{-3}$, whereas ΔIWC^{Prec} is $\sim 2 \text{ mg m}^{-3}$ (which does work out to $\sim 53\%$ by the percent difference definition used here), so that ΔIWC^{Flash} is more than twice as large as ΔIWC^{Prec} , not almost twice as large. Also note: large as than --> large as.

(8) The lack of any tickmarks whatsoever on the right-hand y-axes of Fig. 11 makes it quite difficult for readers to judge any of the values quoted in the text for themselves.

- (9) L428-429: island --> islands; sea --> seas.
 (10) L429: contamination of stratiform --> contamination by stratiform.

Section 7.2:

- (1) L433-434: This is an awkward and unclear sentence. I suggest rewriting it as: “We can use the ERA5 IWC to assess the impact of the vertical resolution of the MLS measurements on the observationally derived Δ IWC estimates.”
- (2) L434-435: IWC^{MLS} estimation derived from MLS --> estimates of IWC derived from MLS.
- (3) L435: $300 \times 7 \times 4$ --> $\sim 300 \times 7 \times 4$.
- (4) L436-437: ... we degraded: 1) the horizontal resolution of ERA5 from $0.25^\circ \times 0.25^\circ$ to $2^\circ \times 2^\circ$ (200 km \times 200 km) and 2) ERA5 data by connecting the vertical profiles ... --> ... two steps were taken: 1) the horizontal resolution of ERA5 was degraded from $0.25^\circ \times 0.25^\circ$ to $2^\circ \times 2^\circ$ (200 km \times 200 km), and 2) the vertical resolution of ERA5 was degraded by convolving the vertical profiles
- (5) L439: The first sentence on this line (“Consistently ... respectively.”) is redundant and unnecessary. It should be deleted.
- (6) L439-440: There is no need for the remaining sentence (“The ice injected ... $\langle IWC^{ERA5} \rangle$.”) to be a separate paragraph. It should be joined to the end of the preceding paragraph.
- (7) L444: The phrase “xx% of variability per study zone”, used here and in eight other places in Sections 7.2 and 7.3 as well as four times in the Abstract, makes no sense to me. I believe that the authors wish to quantify the range of the relative differences between ΔIWC^{ERA5} and $\langle \Delta IWC^{ERA5} \rangle$ in the various study zones (that is, the smallest and the largest difference between the convolved and unconvolved Δ IWC estimates from the 5 islands for the island zone and similarly from the 5 seas for the sea zone), but “xx% of variability” means something else and is not the way to convey that information. If my understanding of what the authors are intending is correct, then perhaps something like this would work: “with relative differences between ΔIWC^{ERA5} and $\langle \Delta IWC^{ERA5} \rangle$ of ~ 19 - 22% over the island study zone”.
- (8) L445: same comment as above for “xx% of variability per study zone”.
- (9) L449: Again, without tickmarks on the right-hand axis it is hard to tell, but the value for New Guinea looks higher than 3.7 mg m^{-3} to me, more like 3.9.
- (10) L449: same comment as above for “xx% of variability per study zone”.
- (11) L450: It is stated that $\langle \Delta IWC^{ERA5} \rangle$ is larger than ΔIWC^{ERA5} “by less than 2.1 mg m^{-3} ”. To me the difference between the two estimates looks more like 2.4 mg m^{-3} for Sulawesi and 2.5 mg m^{-3} for Java. Thus the statement “by as much as 2.5 mg m^{-3} ” would be more accurate.
- (12) L451: same comment as above for “xx% of variability per study zone”.
- (13) L451-452: Again, the value of 0.2 mg m^{-3} should be checked here, as it looks as though it might be larger than that for the Java and North Australia seas, and “by as much as” would be better than “by less than”.

Section 7.3:

- (1) L458: delete stray “)” after “respectively”.
- (2) L459: I do not understand what a single percentage value (75%) means in the context of the fairly large Δ IWC range ($1.0 - 2.2 \text{ mg m}^{-3}$) quoted here. Other percentages that originally appeared in this paragraph have been deleted; this one probably should be too.

- (3) L461: same comment as above for “xx% of variability per study zone”.
- (4) L462: I note that the authors’ response letter indicates that the values 0.6 and 3.9 mg m⁻³ in this line have been changed to 0.5 and 3.7 mg m⁻³. However, such changes have not actually been made in the revised text.
- (5) L462: same comment as above for “xx% of variability per study zone” (twice in this line).
- (6) L463: same comment as above for “xx% of variability per study zone”.
- (7) L465: In statements characterizing differences as “within 1.7 to 0.7 mg m⁻³”, “within” would typically be taken to imply small differences, but in this case the differences are referred to as being “significant”. I suggest instead “as much as 0.7 to 1.7 mg m⁻³”.
- (8) I agree that differences of that magnitude (0.7 to 1.7 mg m⁻³) are substantial. This brings up the issue that in the previous paragraph (L456-457), agreement “within 0.1 to 1.0 mg m⁻³” was characterized as highlighting the robustness of the model over land. But 1.0 mg m⁻³ is arguably not a small difference.
- (9) L466: It is stated that significant differences are found over all individual offshore study zones “within 0.7 to 2.1 mg m⁻³”. But differences for the sea zone were given as “1.0 – 2.2 mg m⁻³” on L459. (And again “as much as” would be better than “within”.)
- (10) The previous couple of points lead me to wonder why the information in the sentence in L464-466 is repeated, as these values were already given in the previous paragraph. I think that these two paragraphs could be combined and written more efficiently.
- (11) L470-471: Based on the reanalysis ΔIWC range, it is suggested that Sulawesi and New Guinea may also reach high ΔIWC values comparable to those over Java, even though the observationally derived ΔIWC estimates do not indicate such strong injections over those two islands. But might it not also be the case that the reanalysis might be in error in those regions? Perhaps they pose particularly challenging environments for the models to get right. Again, it seems to me that the authors see the comparisons with ERA5 only as a means of validating (or assessing) their model, and not as a two-way street that possibly highlights regions of potential issues in the reanalysis.

Overall comment on Section 7: Given the fairly large number of places where corrections to values appear to still be needed, I am not convinced that sufficient care was taken in revising the manuscript, and I encourage the authors to double-check all of the values as well as the absolute and relative differences quoted in the text.

Section 8.2:

- (1) L496: “The authors” in this line is unclear – this wording immediately follows a reference to Awaka (1998), but I assume that it is really pointing to Mori et al. (2004). Please clarify.
- (2) L499: Sumatra West Sea --> West Sumatra Sea.

Section 8.3:

- (1) L505-508: There is a pervasive lack of superscripting in these lines.
- (2) L508: Not only is mg m⁻³ not superscripted here, but also in the second instance the unit is wrong: m⁻² --> m⁻³.
- (3) L508: same comment as above for “xx% of variability”.

- (4) L510: as for Java Sea or Bismarck Sea, North Australia Sea has the particularity to be surrounding by several islands --> as for Java Sea or Bismarck Sea, North Australia Sea is surrounded by several islands.
- (5) L514-515: composed by storms with lightning but precipitation are weak or do not reach the surface and evaporating before --> composed of storms with lightning but precipitation is weak or does not reach the surface before evaporating.

Section 9:

- (1) L523-525: It struck me that the summary in the Conclusions does not mention that a key assumption in the model is that deep convection is in the growing phase. Since many readers may only skim the paper and focus mostly on the Conclusions, and many may also be unfamiliar with the earlier Dion et al. (2019) paper, the authors should make clear that they are only applying their model during the increasing phase of the diurnal cycle of deep convection.
- (2) L524: impacting --> injecting.
- (3) L525: into the --> in the.
- (4) L539-540: As noted in point #13 in the comments on Section 4.3, similar language to that in point (ii) here has been deleted from Section 4.3.
- (5) L542: delete “within” in front of both “4–22%” and “7–53%”.
- (6) L544: included into --> included in.
- (7) L546-547: It is stated that “the observational ΔIWC range has been shown to be consistent with the reanalysis ΔIWC range to within 23 % over land and to within 30–50 % over sea in the UT and to within 49% over land and to within 39% over sea in the TL”. It is not clear where these numbers are derived from, as none of them have appeared previously in the manuscript.
- (8) L547: combination between --> combination of.
- (9) L549-551: I think that the wording “to within xx % per study zones” (which appears four times in these lines) will be unclear, especially to readers who skip to the conclusions without going carefully through the entire manuscript.
- (10) L550: As noted in point #4 in the comments on Section 7.3, the values 0.6 and 3.9 mg m⁻³ have been changed to 0.5 and 3.7 mg m⁻³ in the response to referees but not the main text.
- (11) L551-552: Although the differences between ΔIWC^{ERA5} and $\langle \Delta IWC^{ERA5} \rangle$ at the two levels suggest that the vertical resolution of the observations has a stronger impact in the TL than in the UT, the total ΔIWC variation range being discussed in this sentence does not. Shortcomings in the methodology, Prec, or Flash could all contribute to the total ΔIWC variation range.
- (12) L554-556: Are the values quoted in these lines (0–0.6, 1.0, 0.3 mg m⁻³) consistent with the corresponding numbers given in Section 7 (I don’t think so)?
- (13) L557: ice injected with ERA5 than Java in the UT and even larger ranges of values as Java in the TL --> ice injected with ERA5 as Java in the UT and even larger ranges of values than Java in the TL
- (14) Concerning the sentence discussed above, see point #11 in the comments on Section 7.3.

References: The pervasive lack of proper capitalization and bizarre and unnecessary hyphenation that I commented on in my previous review persist in the revised manuscript.