Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-800-RC1, 2019

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Ice injected into the tropopause by deep convection – Part 2: Over the Maritime Continent" *by* Iris-Amata Dion et al.

Michelle Santee (Referee)

michelle.l.santee@jpl.nasa.gov

Received and published: 11 November 2019

This manuscript is a follow-on study from Dion et al. [ACP, 2019], which reported a novel method of correlating the twice-daily measurements of cloud ice water content (IWC) from the Aura Microwave Limb Sounder (MLS) with higher temporal resolution measurements of precipitation (Prec) from the Tropical Rainfall Measurement Mission (TRMM) to reconstruct the diurnal variation of ice in the upper troposphere (UT, 146 hPa) and tropopause level (TL, 100 hPa), thereby estimating the amount of ice injected at those levels by deep convection (△IWC). Since the previous study found the largest convective injection of IWC over the Maritime Continent (MariCont), here that region is divided into separate island, sea, and coastal zones. The approach to deriving △IWC in the UT and TL from MLS IWC and TRMM Prec data is also applied to TRMM lightning

(Flash) data. Results using both TRMM data sets are compared to those based on IWC from ERA5. Java island is found to be the area with the highest △IWC. The roles of small-scale processes in controlling the △IWC over the different areas are assessed.

In general, I think that this is a very interesting and valuable paper that demonstrates the great potential of the authors' innovative technique to "fill in" the climatological diurnal cycle of IWC and the estimates of △IWC in the UT and TL at 2°×2° horizontal resolution that have been derived from it. Thus I would very much like to see this paper in print. Unfortunately, however, the manuscript is riddled with inaccurate, erroneous, or inconsistent statements, many instances of unclear wording, and numerous typos. In my opinion, it requires a substantial amount of "cleaning up" before it can be published. A (fairly long) list of specific issues is detailed below. In most cases these concerns can be allayed simply by correcting and clarifying the discussion, with few if any requiring additional analysis. But, although each point is perhaps minor when considered in isolation, in aggregate they add up to major revisions. Moreover, even after the large number of minor corrections listed below, the manuscript will need copy-editing to improve the English.

Specific substantive comments and questions (in sequential order through the manuscript):

L9, L45-46, L105-106: I believe that the representation of the temporal resolution of the TRMM Prec measurements in the Abstract (L9) and Introduction (L45-46) is somewhat misleading. In both places it is stated that Prec data are available at 1-hr resolution. My understanding, however, is that the TRMM-3B42 data are provided as 3-hr averages. Only by taking advantage of the precessing orbit of TRMM and the long study period (13 years) are the authors able to average the data in 1-hr bins. This binning is obliquely alluded to in L105-106 in the TRMM description subsection, but it should be explained more clearly.

L72: Liu & Zipser [2009] is missing from the reference list, but actually it is not the correct citation here anyway. The 2009 JGR paper did not use TRMM LIS data. A better reference here is Liu & Zipser [JGR, 2005].

Section 2.1: Several aspects of the MLS description require revision. The most significant issue is the implication that the MLS team should have but failed to provide averaging kernels for the IWC measurements (L90-92). This statement and related discussion in Section 2.4 (L130) and Section 7.2 (L383-385) suggest that the authors have misconstrued how the MLS IWC product is derived. In fact, although optimal estimation is used to retrieve almost all other MLS products, that is not the case for IWC, for which a cloud-induced radiance technique is used. Consequently, no averaging kernels are calculated for IWC. It would be appropriate to reference two of the first papers describing and validating the MLS IWC retrievals: Wu et al. [JGR, 2008] and Wu et al. [JGR, 2009]. According to Wu et al. [2008], the IWC measurements represent spatially averaged quantities whose volume can be approximated by a box with dimensions of ∼4 km high by ∼300 km long; a simple box like this could have been used to degrade the vertical resolution of the ERA5 IWC rather than the unitary triangular function the authors devised, likely leading to slightly different results. Other issues are: (1) Information on the quality of the IWC measurements and the screening steps taken to filter out poor-quality data points should be given. (2) MLS provides IWC measurements at 6 levels in the UTLS, not just at 146 and 100 hPa. (3) Although it is essential to specify the version of the MLS data being used in this study, as written the sentence in L84 makes it sound like it is Version 4.2 of the instrument itself and not the data processing algorithms. (4) It would be appropriate to cite the original paper describing the Aura MLS instrument, Waters et al. [2006], in addition to the MLS Data Quality Document. (5) The most up-to-date version of the latter document is Livesey et al. [2018], not 2017. (6) It might be better to say “horizontal” rather than “spatial” in L92.

Section 2.2: It is stated that TRMM provided observations until 2015 and that the Prec product has been extended through 2019, but the source of the data for the most recent

years is not explained (GPM?). No mention is made of Prec data quality (e.g., biases, random errors).

Section 2.3: Not a single reference for the LIS instrument is cited, nor is there any discussion of data quality, detection limits, etc. I do not understand what is meant by “allowing to observe a point within 90 seconds with a temporal resolution of 2 milliseconds” (L110-111). Within 90 seconds of what?

Section 2.4: As noted by Duncan & Eriksson [ACP, 2018], ERA5 differs from other reanalyses in that it differentiates between precipitating ice, classified as snow water, and non-precipitating ice, classified as cloud ice water. In their study, Duncan & Eriksson typically combined the two products. Presumably only cloud ice water was used here, so it would be good for the authors to comment on whether that approach has any impact on their results. In addition, it might be useful to discuss the conclusions of Duncan & Eriksson regarding the ability of ERA5 to capture both seasonal and diurnal variability in cloud ice.

L134: The statement that ERA5 does not provide winds at 100 and 150 hPa is incorrect.

Section 3: The algebra is backwards here: either the correlation should be flipped in Eqn. (1) or Prec(t) should be multiplied by 1/C in Eqn. (2).

Section 4.2: I am confused about exactly what message Fig. 3 is conveying. As I understand it, a pixel is represented in the maps for 1:30 and 13:30 LT only if it is experiencing the growing phase of convection at that time. Thus all pixels in the map for 1:30 LT are undergoing increasing deep convection then, and likewise for the map at 13:30 LT. The description is ambiguous, but when I read it I assumed that the mean was calculated for each individual pixel, as was done in Fig. 2c and 2e, and not over the MariCont as a whole. If so, then the sign of the deviations from the mean value in a particular pixel indicates whether deep convection is in the early stages (negative) or late stages (positive) of the increasing phase at that time, and the magnitude merely identifies whether the convection is just getting started or is just about to reach its peak (large) versus whether it is near the middle of the increasing phase (small). If that is the case, then I do not see how the inferences being drawn from this plot are supported. It is stated (L188-189) that the growing phase of convection is mainly over land at 13:30 LT, but colored (i.e., non-grey, if indeed grey is meant to denote pixels for which convection is not ongoing, which is not at all clear) pixels seem to be present over nearly the entire domain in Fig. 3b and 3d, and IWC and, especially, Prec show fairly large anomalies over most of the sea areas. The strongest Prec anomaly at 13:30 LT is stated (L190) to be over Java Island, but (a) that may only mean that convection is not in the middle of the growing phase there, and (b) the one pixel with the largest deviation from the mean over the island of Java does not stand out above the similarly large anomalies in the surrounding seas. It is stated (L190-191) that the strongest Prec anomaly at 1:30 LT occurs over coastlines and coastal seas, but equally large anomalies are seen in several pixels over Borneo and New Guinea. It is stated (L192) that the strongest IWC anomaly at 13:30 LT is located over Java, but again comparably large values are located over North Australia and the North Australian Sea. Finally, the region over the North Australian Sea is identified as having a negative Prec anomaly and a positive IWC anomaly, but that is really only true at 1:30 LT – at 13:30 LT, both anomalies are largely positive in that area.

Section 4.3: The discussion is muddled in places. (1) It is not true that the anomalies of Prec and IWC during the growing phase are positive over the West Sumatran Sea (L207-208); in fact, this area was identified in Section 4.2 to fall into category #2, with positive Prec anomalies but negative IWC anomalies, and this discrepancy is why it is discussed in detail in Section 8.2. (2) In L207, "< 0.15 mg m-3" should be "> 0.15 mm h-1". (3) The sentence in L208-209 doesn't make sense: the quoted TL ΔIWC max and min values overlap (3 and 2-3 mg m-3, respectively), the min value in the TL is clearly much lower than 2 mg m-3 in Fig. 4, and the difference between the values in the TL and the UT is larger than a factor of 3-4 – indeed, it is stated to be a factor of 6 over land on L210. (4) The TL is mentioned in L213, but Fig. 5 shows only the UT.

(5) In L215, it should be "large enough to observe the diurnal cycle of IWC between 2 and 5 mg m-3", not Prec. (6) It is stated (L225) that pixels with large ΔIWC have IWC values between 4.5 and 5.7 mg m-3, but that is not true for New Guinea point #2, for which the IWC is much lower. Moreover, the range of IWC values (1.9 to 4.7 mg m-3) for low-ΔIWC points overlaps that of high-ΔIWC points. Thus, large ΔIWC values are not always associated with large IWC values at 13:30 LT over land, as asserted in L227-228. Nor is it possible on the basis of Fig. 5 to make a similar assertion for 1:30 LT over the seas, since no such cases were actually examined in that figure. (7) L228-229 states "This shows that ΔIWC is strongly correlated with the shape of the diurnal cycle of Prec". But isn't that true by definition, since ΔIWC is simply scaled from the min and max in the diurnal cycle of Prec (Eqn. 3)?

Section 5.2: A number of points need clarification. (1) The discussion throughout this section is inconsistent with Fig. 7, which shows the coastlines of the MariCont in the middle panel, not the bottom one. The figure caption is also incorrect. (2) I think the description of how coastlines are defined is unclear; it would help to say "extending into" rather than "over" the sea in L255. It is clear from previous figures that a number of pixels straddle coastlines – are they categorized into the land or the coastal bins? (3) Liu and Zipser [2008] is not included in the reference list, but it is unlikely to be the correct citation in any case. Perhaps the authors meant Liu et al. [JAMC, 2008], but I am not sure that that paper made the specific points about the diurnal cycles of Prec and Flash being made in L259 and L267. (4) The max in the diurnal cycle of Flash over MariCont_O is stated (L266) to be reached between 4 and 9 LT, but the peak is more like 6-7 LT and values are fairly low by 9 LT. (5) Petersen & Rutledge [2001] is also missing from the reference list. (6) I think that another sentence or two of discussion to put the results of the Love et al. [2011] paper into the context of this study would be helpful.

Section 5.3: (1) Sulawesi is singled out (L301-302) for exhibiting the same onset of the growing phase of convection as Java, but it seems to me that all of the islands in

Fig. 8 show fairly similar timing for the increase in Prec and Flash as Java; rather, it is the declining phase when Sulawesi more closely resembles the steeper decrease over Java than the other islands do. (2) It is stated (L287) that Prec and Flash are studied at 0.25°×0.25° resolution in this subsection. Therefore, couldn't the fact that the diurnal max in Prec over the 5 small islands in Fig. 8 is much higher than that reported by Dion et al. [2019] over the broad tropical regions of South America, South Africa, and MariCont_L – based on 2° bins – merely be a consequence of the much greater horizontal resolution used here? (3) In L323-324, it is stated that Flash reaches a max of only 0.1 flashes h-1 over the North Australian and Bismark Seas, but (a) the value should be 0.1 x 10-3 and (b) it is not true for NAuSea, for which the max is about 0.6 x 10-3 flashes h-1. (4) While the diurnal min in Prec is around 18:00 LT over the Bismark Sea, there are several local min in Flash (8, 14, 18 LT).

Section 6: (1) The duration of the increasing phase of the diurnal cycles of Prec, Flash, and ERA5 IWC is stated (L349) to be 4-5 h over islands, but in L296 this interval for Prec was given as 8-10 h over all land areas besides Java (6 h). (2) Over sea areas, the max of the diurnal cycle of ERA5 IWC is stated (L350) to occur mainly between 7 and 10 LT, but this is not true for the Bismark Sea (∼3 LT), WSumSea (there is another essentially equal peak at 17 UT, as noted in L354-355), or China Sea (16 UT), nor is it true in those cases that the timing is consistent with the max in Prec. The statement that the max in the diurnal cycle comes 2-3 h after that in Flash is inconsistent with what was said in L330-331 (4-7 h). (3) The sentence in L353-355 appears to contradict itself ("consistent with the one of Prec . . . but not with the one of Prec") – perhaps "Flash" was meant in the latter case. (4) Although the comparisons with ERA5 IWC are interesting, I am wondering what the main goal in including them is. Is the intention to use ERA5 IWC, and the ∆IWC estimated from it, to confirm the observationally derived values? Or, conversely, is the idea to use the Prec and Flash to "validate" the new ERA5 values?

Section 7.1: (1) It is very difficult for the reader to judge any of the ∆IWC values stated here in the absence of any y-axis minor tick marks in Fig. 11. (2) It is not clear how

the quoted percentages are being calculated (i.e., relative to what). For example, a range of values of 4.87–6.86 mg m-3 is given for ∆IWC over a subset of islands in the UT. It is then stated (L368) that ∆IWC from Flash is greater than that from Prec by "less than 1.0 mg m-3 (41%)". I have no idea how a value of 41% could possibly have been calculated. (3) I am not convinced that the methodology and measurements employed in this study truly allow ∆IWC to be estimated to three significant digits. (4) The fact that ∆IWC from Flash is almost twice as large as that from Prec over the North Australia Sea is attributed to the lagged diurnal cycle of Flash compared to Prec (L371-372), but (a) this is backwards: it is Prec that is lagged compared to Flash, as noted in L325-326, and (b) I did not follow why a lag in the diurnal cycle would cause larger ∆IWC values. (5) The third paragraph is confusing. It starts with a sentence about Java, but then the rest of the paragraph is about the differences between Prec and Flash ∆IWC estimates in general, making the Java sentence seem out of place. The final sentence is badly written and difficult to parse, but it appears to say that Flash (unlike Prec) is not contaminated by stratiform precipitation and thus should serve as a better proxy than Prec over the sea, but because it is negligible there it cannot be used to calculate ∆IWC in those regions – but of course Flash has been used to do exactly that, with the results shown in Fig. 11. And the statement that Flash is a better proxy for deep convection than Prec because it is not contaminated by stratiform rainfall is repeated in Section 8.2 for the West Sumatra Sea specifically. So the discussion here needs to be clarified.

Section 7.2: (1) the definitions of the UT and TL in L381 are switched. (2) The reference on L384, which was likely supposed to be Rodgers [2000], is missing, but as discussed above it is not relevant here anyway, as optimal estimation is not used to retrieve MLS IWC data. Thus the discussion related to that point needs to be rewritten. (3) There is no Livesey et al. [2019] document – the latest version of the MLS Data Quality Document is Livesey et al. [2018]. (4) It is not clear what is meant by the statement "xx% of variability per study zones", which appears in numerous places throughout this subsection and also in Sections 7.3 and 8.3, nor how those values are calculated.

Please clarify. (5) The convolved ERA5 ΔIWC values are greater than the unconvolved values in the TL, not lower as stated in L398.

Section 7.3: (1) I assume that the statement "observation and reanalysis ΔIWC ranges agree to within 0–0.64 mg m-3" (L404-405) is meant to indicate that the ranges generally overlap, not that the estimates precisely agree. I think it might be clearer to say "observation and reanalysis ΔIWC ranges overlap, except over New Guinea and Sulawesi, where the differences between the extrema of the two ranges are 0.64 mg m-3 and 1.63 mg m-3, respectively". (2) Does the fact that the observational ΔIWC range is more or less consistent with the reanalysis range over most islands but is systematically greater than the reanalysis range over all sea regions imply anything about either the validity of the methodology used here or the reliability of the ERA5 IWC values over offshore areas? (3) The combined ΔIWC range over land in the TL is stated (L408) to be 0.63–3.65 mg m-3, but the lowest value (which occurs over Sumatra) looks smaller than that (below 0.5) to me. Again I question whether the degree of precision in all of the ΔIWC values quoted throughout the manuscript is really supportable. (4) The consistency between ΔIWC estimates is discussed in L410-412. It is not clear to me why Sumatra was left off the list of specific land areas where agreement is good. On the other hand, although MariCont_O is identified as showing large differences, it seems to me that it should be noted that agreement is poor for all individual offshore areas.

Section 8.2: It is stated (L449-450) that Flash is a better proxy for deep convection over the West Sumatra Sea than Prec. I note that Flash shows higher ΔIWC than Prec over the WWS (as in almost all offshore areas) in Fig. 11. But I am puzzled about how the discussion of ΔIWC estimates in this section relates to the negative IWC anomaly in this region in Fig. 3, which is based directly on MLS IWC data, not estimates of ΔIWC derived from either Prec or Flash. More discussion tying the IWC / Prec anomalies of Fig. 3 (and how they differ over the WSS from other regions) to the ΔIWC estimates in Fig. 11 would be helpful here.

Section 8.3: I'm not sure that I follow the discussion in this section. The authors note

that daily mean Flash rates are higher than daily mean Prec values over the North Australian Sea, and that that difference is why ΔIWC estimates from the two sources differ most strongly in that region. They then go on to suggest that IWC injected during the day over North Australia land areas is transported to the coastlines and sea areas overnight. But the bottom-line point of this argument is not clear – what observations presented in this paper is it intended to explain? Are the authors contending that this transport of IWC somehow affects their ΔIWC estimates? That appears to be the case based on the final sentence (L517-519) of the Conclusions section. If so, then I find that very confusing, because the underlying basis for their approach in estimating ΔIWC is the assumption that deep convection is the dominant process driving the diurnal increase in IWC in the TTL and that other processes, such as horizontal advection, can be neglected. If indeed horizontal advection of IWC is a factor here, then wouldn't that mechanism operate in other regions as well? (Even just in the North Australia Sea, it seems that similar contributions from New Guinea might also play a role.) Fundamentally, it seems to me that this has potentially serious implications for the validity of their technique for deriving ΔIWC over any offshore areas that should be discussed in more detail here and stated more explicitly in the Conclusions.

References: (1) There is a pervasive lack of proper capitalization throughout the references listed, as well as several instances of bizarre (and unnecessary) hyphenation. (2) The correct reference for the MLS Data Quality Document is: Livesey, N.J., Read, W.G., Wagner, P.A., Froidevaux, L., Lambert, A., Manney, G.L., Millan, L.F., Pumphrey, H.C., Santee, M.L., Schwartz, M.J., Wang, S., Fuller, R.A., Jarnot, R.F., Knosp, B.W., Martinez, E., and Lay, R.R., Version 4.2x Level 2 data quality and description document, Tech. Rep. JPL D-33509 Rev. D, Jet Propulsion Laboratory, available at: http://mls.jpl.nasa.gov (last access: dd MMM yyyy), 2018.

Minor points of clarification, wording suggestions, and grammar / typo corrections:

L11: lightnings –> lightning events

L14 (also L38, 166, 167,169, 189, 205, 210, 211, 216, 219, 241, 252, 255, 289, 309, 311, 407, 421): lands –> land

L16: I think it would be clearer to add “they agree” in front of “to within 4-20%”

L28: dimentional –> dimensional

L29 and L31: add “e.g.” at the beginning of the lists of references on these lines

L35: add a comma after “respectively”

L38: add a comma after “areas”

L41: The first sentence of this paragraph seems out of place, as it has nothing to do with the rest of the paragraph. It would be better to move it somewhere else or delete it.

L53: center –> centers

L56: Is the statement “a comprehensive work has been done around the study of the diurnal cycle of precipitations and convection over the MariCont” referring to previous studies other than Yang & Slingo (cited in the previous sentence)? If so, references are needed. In any case, the sentence needs to be clarified.

L56 (also L104, 430): precipitations –> precipitations

L65: It is not clear what is meant by “the authors were expected”

L73: that will be compared –> and compare

L84: the NASA’s –> NASA’s

L91: add a comma after “respectively”

L92-93: delete “study” after “resolution”; datasets –> data

L96: has been launched in 1997 and has been able to provide –> was launched in 1997 and provided

L97: composed by –> composed of

L102-103: depend –> depends; add “and does” in front of “not differentiate”

L108: lightnings –> lightning

L109: was using –> used

L123: number –> number of

L129 (also L292, 304, 337, 348, 350, 435): consistently –> consistent

L136: Est –> East

L167 (also L194, 241, 245, 337, 339, 416, 431, 464, 516): the New Guinea –> New Guinea

L175: Fig. 2c –> Fig. 2e

L176 (P7): it would be helpful if the Timor Sea and the Arafura Sea were also indicated on the map in Fig. 2a

P8, Fig. 2 caption: It would be helpful if the information about the horizontal resolution of the TRMM and MLS data were added to the caption in addition to being stated in the main text

L179: that –> as

L180-181: each duration –> the duration; can be defined –> can then be defined

L185: present both in Figs. 3a and b (Figs. 3c and d, respectively) –> present in both sets of Prec and IWC panels in Fig. 3

L187: this doesn’t make sense – I think 13:30 LT –> 1:30 LT

L193: is –> are

P10, Fig. 3 caption: It would be good to state in the caption that the IWC plots are for

146 hPa. Also, add a comma after “respectively”

L210: more important –> larger

L232: lightnings are created into –> lightning is created in

L233: lightnings –> lightning

L244: the pervasive lack of superscripts in units (e.g., “month -1”, “mg m-3”, “m s-1”, “mm h-1”) is puzzling, given that superscripts are used for other purposes, but it is only a trivial annoyance in most places in the manuscript. In the case of Flash, however, it is a bigger issue, since it is hard to read “10-2- 10-3” in this line. Sometimes the units on Flash are given as per day and sometimes as day-1. Also, I don’t think it is true that Flash values are lower than 10-2 per day over New Guinea, at least not in the interior of the island.

P12, Fig. 5: It would be more convenient if the y-axis for Prec had 4 (not 3) minor tick marks, as is the case for the IWC y-axis. The solid and dashed lines should also be described in the caption.

L246: Fig. 2c –> Fig. 2d

L252 (also L264, 269, 314): as noted above, the panels in Fig. 7 are mislabeled

P14, Figure 7: It would be very helpful to have more minor tick marks on the x-axis. In the caption: full line –> solid line; dash –> dashed

L289: delete “areas”

L289-290: add commas after “(2008)” and “1 mm h-1”

L294: add commas after ”6h” and “Flash”

L299: it might be good to remind readers that the elevation is shown in Fig. 2b

L302: than Java –> as Java

L304: maintaining –> maintain

L305: rainfalls –> rainfall

L306: convections –> convection

P16-17, Figs. 8 and 9: again, it would be very helpful to have more minor tick marks on the x-axis. Also for Fig. 9, the label for the North Australia Sea is given in panel (b) and the figure caption as “NAusSea”, but in the main text it is “NAuSea”. The labels should be consistent.

L312-313: it would be clearer to say “either coastline or offshore areas depending on the area”

L320: most of –> most

L332: In the next section –> In Section 7

P19, Fig. 10: It would aid the comparisons with Fig. 2e discussed in the text if the same color bar were used, particularly since the ERA5 IWC values reach higher values than those of MLS, yet the color bar in Fig. 2e extends to larger values. This might also alleviate the issue that the highest values of ERA5 IWC over New Guinea and North Australia appear to saturate the color bar in Fig. 10 (that is, white colors appear in the map in those regions). Also, since panels (c) and (d) have been labeled “TL”, it would be good to add “UT” to panels (a) and (b).

L340 (also L342, 347, 366, 373, 377, 391): island –> islands

L344: is –> are

L353: cycles –> cycle; zone –> zones

L363: calculated –> calculate

L366: Fig. 10a –> Fig. 11a

L368: excepted –> except

L370: from –> by

L372: twice greater than –> twice as large as; also delete “values”

L373: Fig. 10b –> Fig. 11b

P21, Fig. 11 caption: West Sumatra Coast (WSS) –> West Sumatra Se (WSS)

L390: why are there parentheses around the convolved ERA5 ∆IWC term in this line?

L406: to within –> by

L410: observational –> observations

L412: negligeable –> negligible

L414: are –> is; twice larger than –> twice as large as

L433: merged –> merging

L433 (also L516): tiny –> small (not only does “tiny” not sound very scientific, but also it could come across as dismissive)

L436: transport –> transports

L437: Fig. 10 shows IWC, not ∆IWC, so it should not be listed with Fig. 4 here; perhaps Fig. 11 was meant instead

L439: section 4.3 –> section 4.2

L442: Awaka (1998) –> (Awaka, 1998); to –> from

L446: “PR” has already been defined in L441

L453: Fig. 10 –> Fig. 11

L462: would be –> is

L479: from by –> from

L481: to impact –> injecting

L482: into –> in

L495: amount –> amounts

L500 (also L504, 517): combination between –> combination of

L510: delete commas after “that” and “Flash”

L513: delete commas after “Guinea” and “Sulawesi”; range –> ranges

L514: as Java –> than Java

L516: cumulus merged –> merged cumulus

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2019-800, 2019.