Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2019-800-RC2, 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.

Anonymous Referee #2

Received and published: 19 November 2019

The paper by Dion et al. is the second part of a work aiming at quantifying the diurnal cycle of ice particles in the tropical tropopause layer (TTL), and more precisely, the amount of ice injected by deep convection up the tropospheric part of the TTL, and up to the tropopause. It is mostly based on the analysis of 13 years of ice water content (IWC) data from MLS onboard the AURA satellite, as well as precipitations data from TRMM, and lightning flashes from the LIS instrument onboard TRMM. While the first part of the study, already published in ACP, is dedicated to the study of all tropical regions over the globe, this companion paper only focuses on the Maritime Continent (MariCont) during the austral convective season of December January and February, because the MariCont has been shown to be one of the most efficient tropical regions to transport ice up to the TTL in Dion et al. (2019). Here the study focuses on each

sub-region of the Maritime Continent, that is the different islands and seas composing it. The main contribution from this study is to present the Maritime Continent not as a whole continent but as the sum of very different contributions. It was already shown in the first paper that the land parts had a different impact and cycle than the oceanic part. Here the authors are going further in estimating the climatological contribution of each main islands and seas of the MariCont. For example, Java and New Guinea are shown to be the main contributors in the transport of ice up to the TTL. In that sense, this innovative work and point of view deserve a publication. Before it can be done, I have major comments and minor comments that should be addressed before the paper can be accepted in ACP. Some of them can be easily addressed by adding explanations and references, some others may require additional calculations.

Major comments: Instrumental part: Information is missing in the description of the satellite products that are used. Most of all, I would expect information on the accuracy, precision, or biases for MLS IWC, TRMM prec, and TRMM LIS (detection limit, false detection of fhashes, etc). See "minor comments" for details.

The use of ERA 5 to estimate DIWC. I have a significant problem with this part. ERA5 is a relatively new reanalysis from ECMWF. The Authors are using ERA5 ice products to compare DIWC from satellite observations and from ERA5. But here, no reference is made on any estimation of the quality of ice from ERA5, nor how the ice is provided or calculated in ERA5. Is ice assimilated in the ECMWF model? If yes, from which instrument? If not, how it is calculated, is there a correlation between the ice product and any reported bias in ECMWF model? As a consequence, is DIWC_ERA5 used to validate DIWC_prec, and DIWC_flash, or should it be understood the other way round? To make a meaningful comparison between both types of estimation, all of this question should be addressed in the manuscript. Furthermore, to my knowledge, the ice in ERA5 is composed of 2 different variables. Are you using the total ice, or only one parameter (which should be non-precipitating ice)? The authors should be more explicit on this point and justify the choice of the variable they have used.

Winds from NCEP. In Section 8, winds from NCEP are used instead of winds of ERA5. It is stated that ERA5 winds are not available. For sure they are. What would be the results shown in Fig 12 if ERA5 winds were used? Does it impact the duration of transport of ice from North Australia land to seas westward?

Minor comments: - Abstract: one of the key highlights of the study is to present the MariCont as a jigsaw puzzle of different contributions with respect to the effect of deep convection on ice in the TTL. For example, Java and New Guinea are presented as very efficient locations for the injection of ice into the TTL. Thus to me, some key findings on the effect of subregions of the MariCont should appear in the abstract. - L11 Lightning is always singular. See also p4 and p11.

- Introduction L31. Jensen et al. (2007) are providing important inputs on the effect of deep convection on the hydration or dehydration in the TTL. It seems appropriate to cite this study here.

- Section 2.1: though the reference for the MLS ice product is given, no information is given on the accuracy and the uncertainties on the IWC. Please add it.

- Section 2.1 L90. This sentence is not clear to me. Please rephrase. At least it should be explained why you need the averaging Kernel at 100 hPa and 146 hPa.

- Section 4.1 L170: there is a very strong contrast in the maximum time of Prec between land and coastal region. If convection and Prec maxima are due to a sea breeze effect and orography over land, as stated before, why coastal region maximum of Prec is clearly not influenced by the sea breeze (otherwise the time of maximum of Prec should not be so different)? On the other hand, the coastal behavior seams relatively independent from the oceanic behavior since the oceanic behavior is very dependent on the sea considered, whereas the Prec maximum for coastal region is relatively well identified. A longer comment or hypothesis should be presented here to explain this behaviour

- Section 4.2 about Fig. 3. From what I understand, the number of occurrences (= cases per pixel that are during the growing phase at 13:30 or 01:30) on which the average/the anomaly is calculated depends on the pixel. What is the amplitude of the number of occurrences to get this figure?

- Section 4 Fig. 4. This is one of the key finding of the publication. However, I am surprised that qualitatively, the same patterns are found at 146 hPa and at 100 hPa, the only difference being the scale. I would expect a slightly different behavior at 100 hPa because the ice amount might be also driven by other processes than just deep convection (e.g. in situ formation of cirrus or ice particles close to the tropopause). Does the result mean that other processes than deep convection are negligible, or cannot be detected by this method or the instrument? A discussion should be given at the end of the comment of Fig. 4.

- Section 4.3 : 228 "this shows that…" Ok but what can we learn about the diurnal cycle or the intensity of deep convection from this correlation?

- Section 5 l231: potential energy –> electric potential energy

- Section 5.2, l254. The choice of 5 pixels over the sea from the land limits: why this choice? Was a sentivity test made on the number of pixel to infer the behavior of coastal regions?

- L255: 10 pixels offshore for oceanic behavior. Please justify.

- P14 fig 7 and results from it. There is a mismatch between the titles of the middle and bottom panels and the corresponding figure captions. Middle panel is entitled MariCon_C and It is captioned MariCont_O. The other way round for the bottom panel. In a general manner, the results for the coasts are relatively close to the one offshore. At least the time shift is weaker between MariCont_O Vs MariCont_C than for MariCont_C Vs. MariCont_L. Is the choice of 5 pixels from the land to define the MariCon_C has something to do with it? What if you had chosen 3 or 2 pixels only? Would the coastal diurnal cycle of Prec and FLash be closer to the Land cycle?

- P15: References Liu and Zipser (2008) and (2009) appear in the text but not in the reference list.

- Fig 8 and 9. The ERA5 IWC is also presented in the figures and is not commented in section 5. This does not make any problem since it is commented later on in section 6 but at least, a sentence should inform that the ERA5 results would be commented later. About the same figs in section 6, L346: it would be interesting to overplot an equivalent value of the MLS IWC and comment it. As written in my major comment, there no real estimation of the ice product in ERA5. Adding the MLS IWC here could give an idea of a potential bias in the reanalysis. In section 6 the authors comment on the consistency or the inconsistency of the ERA5 IWC diurnal cycle with the Prec one. But no reason or hypothesis are given to explain this disagreement. I wish a discussion appeared at the end of section 6 on that point.

- Section 7. Before reading the whole paper, I did not understand why results from DIWC offshore could be given for flash since it was shown previously that IWC_flash are not synchronous over seas. Thus, one could deduce that the method to estimate IWC (and DIWC) cannot be applied offshore. We understand later that some regions are better described by the IWC_flash approach than by the IWC_Prec, due for example, to the higher contribution of stratiform Prec. So in section 7.1, the part where Fig 11 is presented and commented, it should be justified more clearly why DIWC_prec and DIWC_flash can be presented as a range of observational DIWC. In section 7.2 p 20 and 22. It would be interesting to recall here the number of model levels from 150 to 100 hPa to have an idea of the vertical resolution of the undegraded ERA5 data.

- 7.3 Obviously, a comparison section is needed here, but without describing how ERA5 ice is produced/calculated, the results presented here are meaningless. I do not say it is out of interest to do so. There is probably something to learn in this comparison (for example from the fact that over seas, ERA5 DIWC is systematically lower than the

observational DIWC), but here one must be aware the meaning of the product used.

- L418. Considering the large range of ERA5 to <ERA5>, the numbers given may not be representative.

- Section 8.3 L459. I would have added Corti et al. (2008) for the references concerning Hector.

- Section 8.3 from L465. See my major comment about the NCEP winds.

- L471 and 472. Ice in the UT and at the tropopause is not a passive tracer. So, to state that, an estimation of the lifetime of ice particles for both altitudes should be given.

- L503 “consistent to within 75 % over seas”. This corresponds to a relatively fair agreement, and the use of “consistent” seams exaggerated.

- L517. “DIWC is a combination. . .” Again, you can write this statement only if you show that the lifetime of ice particle is long enough for such a transport.

Suggested references: - Jensen, E.J., A.S. Ackerman, and J.A. Smith, 2007: Can overshooting convection dehydrate the tropical tropopause layer? J. Geophys. Res., 112, D11209, doi:10.1029/2006JD007943. - Corti, T., et al. (2008), Unprecedented evidence for deep convection hydrating the tropical stratosphere, Geophys. Res. Lett., 35, L10810, doi:10.1029/2008GL033641.

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