Review of 'Assessing the impact of the Kuroshio Current on vertical cloud structure using CloudSat data' by A. Yamauchi et al.

This manuscript investigates the response of clouds to the Kuroshio current in the East China Sea region. CloudSat products are used to provide cloud properties and precipitation profiles, combined with regional re-analyses in order to provide a meteorological context to the study. Analyses are here limited to summer months (15 May - 15 June) from 2007 to 2010. The authors' main conclusions are that the Kuroshio current leads to stronger precipitation in thick (convective) clouds, which also become more frequent.

An important motivation for this study is the need to provide regional observational constrains to climate or weather prediction models. The authors point in particular to limitations in auto-conversion rate parameterizations in GCMs or the need to thoroughly evaluate the representation of cloud and precipitation structure in new high-resolution cloud resolving models (CRMs) such as NICAM. I fully agree with the importance of these questions, but find that using 3 years of satellite data makes the results presented here fall in between of these two objectives. A longer time period is needed to establish more rigorous climatological conclusions useful to GCMs, whereas more precise case study analyses would be needed to evaluate CRMs. I would advise for the former solution, as discussed below.

I also fail to be fully convinced by some of the conclusions made here, as the presented cloud properties observed in ON and OFF Kuroshio regions (terms used in the manuscript to identify areas under the influence of the current or not) do not always clearly seem significantly different. The conclusions are not illogical with expectations, but I would advise the authors to perhaps change their definitions of the ON and OFF regions in order to make the differences more obvious (see comments below). More in-depth analyses and a stronger story line that links the figures together could also help making some of the arguments more convincing. This could be done by introducing a short section that discusses the overall conclusions based on all figures.

Nevertheless, I think that this study is of interest and can significantly be improved by adding more data and clarifying a few points. It is generally well written and, providing more in-depth analyses, could fit within the scope of ACP. I therefore advise for publication after major revision. Further details are provided below.

Major comments:

1. p2, l1-2: The authors cite two papers by Koike et al. (2012, 2016) that investigate the impact of aerosol on liquid clouds in relation to Kuroshio current. It would be nice to mention this effect and clarify how the present study fits in the context of this previous literature. Could some of the cloud and precipitation changes observed in this manuscript result from fast cloud adjustments to aerosol perturbations induced by the current? I realize that this is not the main topic here, and disentangling aerosol-cloud-meteorology effects is extremely difficult, but this issue could be briefly addressed.

2. p3, l17-18: I strongly encourage the authors to expand their analyses to the entire A-Train period (2006-2016). This would greatly improve the statistical significance of the results presented here, and I think could even help the authors to make their conclusions stronger by reducing weather noise. The data is freely available and I do not see any reason to only use 3 years of data. Perhaps because the night-time data is not available later on? Also, have you have merged day and night overpasses in the analyses and, if so, are there any consequences on the results by comparison to using day-only and night-only statistics?

3. p3, l20-23: Using a strict SST threshold is perhaps not the best option to determine the ON and OFF regions. This method implies that transition areas are included and could blur the expected changes in statistics of cloud properties and precipitation between the two regions. Another option could be to select ON and OFF regions based on separate ranges of temperatures, for instance corresponding to the first and last quartiles (or other percentiles, depending on the desired sensibility) of region-mean SSTs. This way, both regimes are better defined and can be distinguished.

4. Section 3: As previously mentioned, and in relation to the previous point, the differences between properties observed in ON and OFF regions aren't always obvious. For example, the authors have mixed interpretations of Fig. 2, sometimes stating that no clear differences are observed (e.g. p5 l2-3, l6-7, and I'd agree with that) and later that the Kuroshio impacts heating at high altitude. It is not clear to me from Fig. 2k where this impact is, could you clarify? Also, based on Figure 5 (analyses in section 3.4) I do not see any clear signal of perturbation by the Kuroshio current, except perhaps in 4e. More statistics (more A-Train data) could help clarify if this is within weather noise or not. In general, all the 3.x subsections appear a bit as lists of figure descriptions without a consistent in-depth analysis, until the very end where interesting arguments based on cloud processes are provided (beginning of p.8). It would be good to improve how each figure and their respective results fit all together, to make the final conclusions more convincing. I had some troubles understanding towards the end how all the presented results connect, maybe an extra section would help.

Minor comments:

1. p2, l.11: This is a detail, but isn't the across-track resolution of 1.4 km? This information, and the following technical details, could go in section 2.

2. p4, l.18: Any reason to subset the previous region (yellow box)? Especially that this thick dotted rectangle region only is used in section 3.1.

3. p2, l.12: Analysis of Fig. 2i,j: What are the vertical black bands?

4. p2, l.17: net radiative heating here means SW + LW?

5. p5, l.24: Could you clarify what are "contoured frequencies by altitude diagrams" and their advantage by comparison to the previous PDF?

6. p6, l.8: "for higher altitudes": Do you mean above 6 km? The statistical significance doesn't seem high in regions where the difference if positive at higher altitude. It seems more clear from this figure that heavy precipitation below 6-km is reduced but there is more drizzle.

7. p6, l.15: Can you precise how the geometrical thickness is computed? Please keep in mind that CloudSat is sensitive to the surface echo (so the cloud base of low clouds is difficult to get) and not sensitive to thin layers (so could miss cloud-top). I am not sure how this impacts the conclusions made here.

8. p7, l2-3: "the frequency of occurrence of the precipitating clouds with a geometric thickness of 7 to 10 km significantly increased ON Kuroshio." - True, but does it mean that thick clouds precipitate more or that there are more thick clouds in ON Kuroshio regions (consequence of stronger updrafts). This is an example where coupled analyses with results from previous figures would be helpful.

9. Figure 4: How did you bin the geometric thickness? How is the PDF normalized?

Technical corrections:

1. p2, l.4: "General" instead of "Generation" or "Generation of general"? I assume that the authors refer to the "too bright too few" problem, but the sentence is a bit confusing.

2. p2, l.12: "cloud particle size, which ranges from drizzle to precipitation" can be misleading. Replace by "large cloud particles and hydrometeors"?

3. p4, l.10: why "(divergence)"?

4. p4, l.19: Are the sections not meridional instead of zonal?

5. p6, l.4: "for both" instead of "both"?