

Interactive comment on “Analysis of geostationary satellite derived cloud parameters associated with high ice water content environments” by Adrianus de Laat et al.

Anonymous Referee #2

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Formal Review Of: Analysis of geostationary satellite derived cloud parameters associated with high ice water content environments

Lead Author: A. de Laat

This paper describes methods for discriminating high ice water content (HIWC) producing convection using satellite imager observations and satellite cloud microphysical retrieval products. The methods are threshold-based with the first set of thresholds being derived from in-service aircraft encounters of ice water content (IWC) $> 1 \text{ g/m}^3$ and the second being an adjustment of these thresholds to maximize Critical Success Index based on IWC derived from CloudSat and CALIPSO observations (their so-called DARDAR product). Satellite parameters used for HIWC discrimination include cloud

C1

mask, effective radius, cloud phase, cloud temperature/height, cloud optical thickness, and ice water path.

As noted in my initial review, I found this paper to be well written and it is clear that the set of authors have extensive knowledge of the HIWC engine icing phenomenon and have a clear understanding of the challenges associated with HIWC detection/nowcasting. The authors make it clear that no passive satellite imager based algorithm will be capable of 90+% POD and 10% FAR (for example) due to the complex physics of the problem and that passive sensors cannot “see into the cloud” to altitudes where HIWC is located. Thus we are left to rely on inferences of in-cloud dynamics from cloud top observations. Nevertheless, even after their minor revisions to the paper and responses to my initial comments, I still do not feel that this work is ready for publication.

I have several significant concerns regarding this study that were not satisfactorily addressed from my initial review: 1) Your Equation 1 shows that ice water path is a parameterization based on optical depth and effective radius but if effective radius is not useful, then you are basically including optical depth twice. So your “optimized” end product includes almost any pixel that could possibly be ice (cloud temp < 270), optical depth > 20 , and ice water path > 100 . The water path would almost never be less than 100 with optical depth of > 20 , even with very small ice radii. Please explain your rationale for essentially double use of optical depth. I could envision other parameters that would be of value to substitute for one of your optical depth based parameters.

I appreciate the challenges you state regarding isolating HIWC in clouds and lack of understanding of how HIWC is generated. But this mask does not seem to be a product that the aviation community could use. No forecaster I know would alter flight routes based on this type of product. The product seems more like a “ice cloud mask for moderate to high optical thickness cloud” than anything that could be useful for discriminating localized HIWC conditions. You’re basically showing locations where HIWC would not be rather than where exactly it is. I have analyzed how your product would perform

C2

using every flight from the HAIC-HIWC project. I find that with your mask thresholds and use of TWC > 1 to define HIWC, your product would provide a 0.97 POD and 0.78 FAR. In addition, if you're limited to solar zenith angle of < 60 then, you'd only have a product for a handful of hours a day, and only a few of these hours would actually have robust convection (i.e. intense storm activity typically begins no earlier than 2 PM). Given what I explain here and the thresholds you've selected as "optimal", please help me to understand how this product could be useful to the operational forecast community. Would you honestly expect aviators to fly around all the moderate optical thickness clouds you identify to "avoid HIWC"?

2) I still do not find the reasoning behind including RDT to be well explained nor are the findings worthwhile. Please explain the POD and FAR for RDT identification of HIWC. We know that based upon your optical depth > 20 and cloud temp < 270 optimized HIWC mask thresholds, almost every convective anvil will be identified. Based upon the name of the product, RDT is designed to identify rapidly growing thunderstorms. This rapid development often occurs during the initial development phase where, as you indicate, the cells are small. So of course you should see some mismatch in the areal coverage of the product. You state that you are "comparing" the two products but the one scene you show isn't especially interesting aside from showing the areal coverage differences. In examples I personally have seen in talks and papers, RDT seems quite capable of detecting large anvils and embedded developing regions within them, not just newly developing small cells. I would think that, given how this product behaves and that it can extract cloud cooling and areal expansion, RDT may actually be better suited for HIWC detection than the mask you show here. Perhaps merging the two products (i.e. cloud temp < 270, optical depth < 20, within a cooling/expanding RDT object) would create a product more accurate and useful to forecasters.

In summary, given the relatively weak analysis shown in the RDT section and tangential relationship to the primary focus of the paper (as it is currently written), I am not seeing how this section is relevant and request again that the RDT section be removed to

C3

improve the focus of the paper.

Given what I describe above and below, I do not feel that what seems to be the core goal of the paper, namely the development, validation, and characterization of an HIWC mask was satisfactorily accomplished here. The authors raise many interesting points and are clearly excellent scientists. Yet the extraneous material throughout such as RDT and DARDAR-CPP comparisons, combined with double usage of optical depth in the mask and an end "optimized" mask product that basically classifies any moderately thick ice cloud as HIWC are major roadblocks for me that I don't think could be addressed in the limited time allotted for revision. Nevertheless, I will allow the authors one final opportunity to address the issues I raise. As I am already on the fence between major revision and reject, if the core issues I mention throughout are not addressed, I will reject the next round.

Additional comments Line 37, replace IEC with IWC

Line 58, recommend citing Smith et al. (JAMC, 2012) here

Lines 65-66, you note that 80% of in service events were related to ice crystals in anvils and 20% related to strong convection. Do you think that this is caused by the fact that aircraft are directed to avoid classic convection by air traffic control and via signals they see on their onboard radar. I'm guessing that if there were more convective core penetrations, your perception of where in service events are located would change. Please comment on this in the paper text.

Line 80, please reference the partnership with North Americans via the HAIC-HIWC campaigns.

Line 85, if it is understood that 80% of events are outside of classical convection, then why was it a focus for HAIC to study core and near core regions?

Line 93, it seems as though Grzych and Mason (2010) and Grzych et al and Brevin et al (SAE, 2015) have characterized in service icing pretty well. This contradicts your

C4

statement here that indicates that icing is not well understood or characterized. I recommend you revise your wording here.

Line 120, what are the “second and third HAIC flight campaigns?” I wouldn’t expect many to know about this so I recommend you explain further or omit

Line 124, why is daytime only the focus here? Many flights occur at night especially over oceanic waters. Convection peaks in intensity at night over ocean and also over land associated with mesoscale convective systems that develop overnight. I understand that you want to utilize optical information to aid detection, and night would have IR only. Given what you say about the 60 degree solar zenith angle limitation, you’re quite limited in the hours of day when your product can operate and intense convection is present. I request that you explain these additional limitations and prospects for night-time HIWC identification.

Lines 176-180, so are you saying here that the phenomenon of “small ice crystals” in storms commonly attributed to HIWC cannot be retrieved using satellite measurements? I see inclusion of Reff in your Mask Version 1, what is the purpose of including this field? I also recommend that these sentences be separated into another paragraph given that it is in an already long paragraph.

Line 184, please explain how these products differ from the “official” merged CALIPSO/CloudSat cloud microphysical products produced by the CloudSat Science Team (<http://www.cloudsat.cira.colostate.edu/data-products/level-2c/2c-ice>) and why you opted to use DARDAR rather than these? What are the advantages of DARDAR?

With regards to DARDAR, I read in Austin et al (JGR, 2009) that the uncertainty of the CloudSat IWC retrieval can be up to 40%. Given this uncertainty, what you perceive to be an HIWC event may not be, and vice versa. This casts doubts on the data used as foundation for HIWC truth. I strongly request you address this in the paper text.

C5

Also with your DARDAR discussions, you’re mentioning height of maximum IWC quite a bit, i.e. line 438, Figure 9. If you look at Figures 1-5 of Setvak et al. (Atmos Res, 2013), you’ll see that in and near deep convective core regions, the CloudSat signal attenuates in the layer nearest cloud top (their Fig 5 is the best example) which is thought to be due to the presence of graupel near cloud top in strong updrafts. So the apparent height of max IWC would be very high, but in actuality, this height may be further down into the cloud that cannot be seen by CloudSat. I strongly recommend you consider this in how you state your results and mention this observational characteristic in the paper text.

Line 244, please provide a point of contact or a mechanism for one to acquire a list of these AIRBUS events for scientific research. I see an AIRBUS representative on the author list so I’d imagine that he could provide this information. The results from published journal articles are supposed to be reproducible and, without this list, one cannot verify the reproducibility of your results

Line 255, you spend some time in the Supplemental section doing a pretty good job of presenting an interesting HIWC mask (I’d say a more interesting one than v2 mask), but then in lines 254-257, you almost immediately cast off both the training dataset and mask as being not useful. This seems very odd. . .why bother with describing the mask if you don’t think the in service events are useful?

Line 258, “was agreed upon”, by whom? Please reword for clarity

Line 266, you say that a test dataset from 2008 was provided, but then only 31 orbits were analyzed? This is confusing; many more than 31 orbits were recorded in the entire year of 2008. Please explain in the paper text

Line 270, Figure 1, you use a combination of red and green to show where ice was observed in the profile. Red and green are an awful color combination for those that are color blind so I request that you consider this and recreate this and other figures that use red/green combos to highlight meaningful results.

C6

Lines 270-328, I find that the material presented in these lines is extraneous and does not necessarily advance your goal of developing a HIWC mask. 278-288 talks about cloud top height assignment relative to DARDAR. Cloud top height was irrelevant to your optimized mask. Also, you spend a good bit of text comparing CPP to DARDAR IWC/IWP and effective radius considerations. Your goal in this paper was to develop a mask to discriminate HIWC using satellite observations. It really doesn't matter what CPP produces for CWP; all that matters is that you pick a threshold that gets the job of discriminating done well.

As I'm reading the text prior to line 270, I'm engaged with the paper and its message, then line 270 comes along and I lose focus, waiting for the main point that never seems to come. Please explain the relevance for including this material and perhaps make more concise to stay on the primary message of the paper.

Lines 348-353, I recommend rewording this paragraph as I'm not currently following what you're getting at here. You say "were then analyzed", what are you referring to? I don't see anything about steepness in the paper. Line 351 says "mask in the maximum IWC interval in the interval. . ." which reads a bit awkwardly.

Lines 376-382, Figure 8a, I see that you have a ROC curve here, but your axes are backwards. FAR is usually along the Y-Axis so that the "area under the curve" can be analyzed to assess the quality of a product. I recommend you reverse the axes here to make this plot consistent with how others in the validation community present their results.

Line 401, as noted above, if one uses your thresholds to analyze product performance relative to 45 second averaged TWC > 1 from the IKP2 probe during HAIC-HIWC Darwin, you get a product with an estimated POD of 0.97 and FAR of 0.78. Obviously we're not using CPP results here but other cloud retrieval algorithms have been proven to provide comparable retrievals to CPP as evidenced in the Hamann GEWEX assessment paper. This is quite inconsistent with the results you get here which is a really

C7

big issue to contend with. How do you reconcile these differences between DARDAR based truth and IKP based truth?

Line 443-448. You mention that maximum IWC of 0.1 is "still quite high". My assessment of in-situ TWC data during HAIC-HIWC and column max IWC from RASTA retrievals is that 0.1 is about the 25th percentile of TWC observed during the HAIC/HIWC campaigns. My personal opinion is that 25th percentile is not particularly high.

Line 452, you may be seeing attenuation of CloudSat signal leading to higher IWC altitudes, which would indicate a more vigorous storm that could be easier to detect via satellite.

Line 470, I recommend a different term than "air masses" here

Line 563, I wouldn't exactly characterize the analysis here as "fine tuning" given the very liberal thresholds and double counting of optical depth used within the end product

Line 577, after thunderstorms have ceased developing and they are decaying, wouldn't the risk of HIWC greatly decrease? So the fact that the HIWC mask identifies these may not be a good thing, correct?

Line 603, where was the HAIC field campaign in 2016?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-246, 2016.

C8