Review of AMT(D) paper amt-2017-367.

Yost et al., [2017], A prototype method for diagnosing high ice water content probability using satellite imager data.

This paper describes a satellite observation based method for identification of atmospheric HIWC conditions (now-casting). Identification of HIWC has become a relevant research topic related to aviation, as aircraft have been reported to occasionally suffer from what is known as “ice particle icing” or “in Service icing”, whereby engine damage, engine failure, and loss of control has been reported by aircraft. Such events are thought to be predominantly associated with large concentrations of high altitude small ice crystals, but remains enshrouded in considerable uncertainty about what exactly happens when and where.

The PHIWC product presented in the paper is based on optimizing a set of satellite-based cloud parameters such as cloud top temperatures, optical depths, overshooting tops, and derived quantities.

The product is optimized towards three (aircraft) field campaigns that specifically focused on better characterization of clouds and cloud microphysics for HIWC conditions, thought to be an important condition for the occurrence of “ice particle icing”.

The paper is long but well written, and logically structured. The is a clear buildup of arguments for the parameter choices, detailed analysis of a number of case studies, and a statistical underpinning of the verification statistics of the PHIWC.

Overall, I recommend publication.

Below is listed a number of questions and minor issues.

Jos de Laat

General comment

The paper uses both "Total Water Content" and "in situ Total Water Content". For clarity choose either one, preferably "in situ Total Water Content" in order to avoid confusion as the use of "Total" in other atmospheric research communities often is interpreted to refer to "Total column" . In addition, the abbreviation "TWCi" or "iTWC" could be used, but I leave it up to the authors to decide.

Specific comments

- Page 1, line 17. Is that true? High IWC does not necessarily imply high mass concentrations of ice crystals (the reverse obviously does). Maybe rephrase to avoid confusion?

- Page 1, line 20-22. Not directly clear what the "weak reflectivity" refers to. Suggest to change the sentence to:

  "… have been document during flight in regions near convective updraft regions that do not appear threatening in onboard weather radar data (weak reflectivity)"

- Page 3, line 5. Suggest to add "… difficult to identify and avoid based on currently available cloud information in the cockpit (mostly weather radar)" or something similar. Reason is that this may change in the future, as evidenced by the satellite data product introduced in the paper.

- Page 4, lines 6-11. There is actually a significant difference between the target of this paper and that of de Laat et al. [2016]. This paper is trying to optimize a set of satellite observed parameters for detection of (local) in-situ TWC exceeding the threshold value of 1 g/m3. De Laat et al. [2016] tries to optimize a set of satellite observed parameters for detection of clouds where anywhere in the vertical in-situ TWC exceeds the threshold value of 1 g/m3. Effectively de Laat et al. [2016] looks first considers the maximum in-situ TWC in a cloud profile, selects those cloud profiles where the maximum in-situ TWC exceeds the threshold, and then optimizes the cloud parameters for detection of this subset. This is fundamentally different from the approach in this paper. For example, in this paper whether in-situ TWC exceeds the threshold elsewhere in the cloud does not matter, whereas in de Laat et al. [2016[it does.

  Reason for de Laat et al. [2016] to focus on the maximum in-situ TWC is that weather satellites – which could be useful for an operational service due to their continuous spatio-temporal coverage – only observe clouds from the top down, and only provide either parameters representative for the cloud top, or representative for a (partial/vertically weighted) integrated vertical cloud profile.

  This paper and de Laat et al. [2016] thus have fundamentally different goals, which affects identification and characterization of clouds and cloud systems where such conditions occur.

This difference in parameter for which the respective algorithms are designed should be clarified (local in situ TWC vs maximum in situ TWC in vertical cloud profile).

- Page 6, lines 6-10. Discussion of the three field campaigns and whose data is used in the construction of the PHIWC product.

  These are three campaigns focusing on particular types of convection, mostly probing active mesoscale convection while also avoiding particular clouds and cloud conditions (see also page 7, lines 15-16; page 8, lines 3-4; lines 16-18; lines 20-22). Although it is accepted that these "ice particle icing" events frequently occur in (mesoscale) convection, they are not exclusively confined to convection alone. This means that that the PHIWC product is tuned towards the particular type of convection probed during the field campaigns, while other types of convection or cloud systems are left out. This might result in particular types of convection and cloud systems to be under-sampled and for the PHIWC product to be less accurate in detecting "ice particle icing" conditions associated with types of convection and cloud systems.

  Obviously no one knows whether this is really the case, but I think it would be good for the paper to briefly discuss this in the discussion section 4, also because this notion can be translated into some recommendations:

  > - data sharing by the aviation industry. It would be extremely useful if the aviation industry would be willing to share more data and information about "ice particle icing" events. This is currently not standard practice, which hampers research progress.
  >
  > - field campaigns focusing on clouds and convective systems not probed during the various HIWC/HAIC campaigns.

- Page 13, lines 10-13. If, as the authors contend, the shadowing effect leads to underestimation of the COD, then I would be tempted to argue that the COD is underestimated (biased low). Smoothing reduces noise, but does not reduce a bias. This is also what is seen in figure 2e-f, where the smoothing reduces the noise. But does that mean that the smoothed field is better?

  Shouldn't the goal be to remove the bias caused by shadowing? If so, that would translate into removing outliers (reduced COD by shadowing) rather than smoothing. For removing outliers other statistical methods should be used.

  I don't believe it is necessary here to change the method for dealing with the shadowing effect (I cannot envision how this could have a significant effect on the verification statistics of the PHIWC product), but if the authors agree, I would recommend mentioning that the smoothing does not remove a bias associated with shadowing, and possibly a statement that this is a topic for future research.

Unless the authors have the possibility to do a quick check on this, in which case results could be included and briefly discussed.

- Page 21, lines 10-11 (section 3.2). I can imagine that the statistics of the occurrence of low/mod/high TWC in relation to their vicinity to OT depends on the type of cloud systems that are sampled during the field campaigns (mesoscale convective systems). Is there any indication that this may be the case?

- Page 37, lines 5-10. Simple question: would the use of motion vectors based on cloud displacement provide a viable possibility for short term prediction? For other applications motion vectors have been shown to allow up to a few hours prediction ahead in time. If the authors think this might be a viable option here as well, they could mention it as something for future considerations.

Typos

Page 39, line 1. thermondynamic → thermodynamic

Page 40, line24. thick anvil cloud → thick anvil clouds