Atmospheric Measurement Techniques Discussions

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A good methodological work, which needs some generalization and improvement

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Referee comment on "A CO_2-independent cloud mask from Infrared Atmospheric Sounding Interferometer (IASI) radiances for climate applications" by Simon Whitburn et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-127-RC2, 2022

The reviewed manuscript presents a cloud detection algorithm for the hyperspectral spaceborne IASI instrument(s). The method uses a neural network approach and uses only IASI radiances as an input. The authors paid specific attention to avoid the channels affected by trace gases with the concentration variable over time. When applied to a series of IASI measurements, the method shows physical results and its robustness is demonstrated over the whole period of all MetOp/IASI instruments' observations up to now.

The research is topical, the methodology presented in the article is sound, and the paper is well structured and written (except for some minor issues listed in "Technical corrections" section). Still, there are several issues I'd like to clarify/fix before recommending the manuscript for publication.

I have chosen "major revision", but the changes I suggest are easy to implement. I believe, if the authors add the suggested information to the manuscript, it will become irreproachable from the methodological point of view and it will have a broader impact.

General comments:

- Even though the average cloud amount shown in Fig. 5 looks physical and reasonable, I see a general methodological issue in using IASI L2 cloud product for training the neural network. I do not question the quality of this product – as follows from lines 64–79 of the manuscript, the methodology is mature and the results are generally good, but there is one caveat. I would not hesitate if the training were based on some "ground truth" dataset coming from in situ measurements, ground- or space lidar, or some other instrument, but I see an inconsistency in using IASI itself as a reference, given that its time series (Fig. 6) shows certain artefacts in cloud cover. I do not ask

the authors to redo the whole work, but it would be good to supplement it with some kind of validation of the training dataset using, e.g. CALIPSO clouds as a reference. To avoid the diurnal effects related to different overpass times, one can focus only on the clouds over the ocean. Perhaps, it would be sufficient to show several representative profiles of the training dataset and to compare them to overlapping CALIPSO cloud profiles (e.g. Chepfer et al., 2013). Or, better yet, show the 3-month average of the IASI L2 and CALIPSO (3 months are required to get a full spatial coverage from CALIPSO). One can also use GEWEX Cloud Assessment files (https://climserv.ipsl.polytechnique.fr/gewexca/index-2.html) for the comparison, but in this case IASI L2 cloud product should be processed in accordance with GEWEX CA rules (Stubenrauch et al., 2013). Either way, this cross-validation of the training dataset seems necessary to wrap up the methodological part.

- As far as I understand, the present requirements to articles published in EGU open access journals include the distribution of the codes and/or the data used in the article. To my knowledge, the trained neural network of a kind applied in the manuscript can be represented by a couple of pages of ASCII-text in pseudocode (variables of the first layer = linear combination of the variables of zero-layer, second layer =…, …, result= linear combination of the variables of N-1th layer). It can be added to the manuscript itself or be provided as a supplement, but it should be certainly doable and it will be useful for the community.
- The contribution functions for the channels centered at the same or close wavelengths for IASI and AIRS should be close to each other, see e.g. (Feofilov and Stubenrauch, 2017) mentioned in the manuscript. Correspondingly, I'm almost sure that the neural network trained for IASI will be applicable to AIRS and maybe even to other instruments like HIRS. It would be good to apply the NN to AIRS L1 data to show the potential and versatility of the method. Just a single map in the appendix would generalize the approach and the NN file explained in comment #2 will enable the other researchers to calculate their own cloud mask on the fly.

Specific comments:

Lines 105−120: this text could be significantly simplified if Fig. 1 were supplemented with the vertical contribution functions (averaging kernels) for each channel. One can put these curves side by side, on top or at the bottom of Fig. 1. The actual scale is not that crucial, but the center and the halfwidth of the averaging kernel should be clearly visible. This is an important methodological point, which would explain the information content of the signals used in the approach. The NN itself has enough "black box" features, so anything that could be clarified should be clarified.

Lines 130−132: I'm not sure I've got the idea here. Normally, the thresholds of this kind should be selected basing on the minimization of an error (or maximizing of the correlation coefficient) for two datasets. That's what is written in lines 134−136 below, and I agree with this approach. I'd suggest to leave only this part in this paragraph since the beginning is misleading. It would be also interesting to have a look at the difference curve mentioned in line 135 to estimate the uncertainty of the threshold, but the authors can just do it themselves and provide a ±value along with the threshold.

Fig. 2: In the left-hand side panel, I do not see the cloud structures of the right-hand side

one. There are a lot of yellow circles in a cloud-free (?) area in the lower left section of the image. Perhaps, the light "haze" which one can see in the right-hand side corresponds to a real cloud, but it is not clear from the image. What is the correlation coefficient for these two panels and what is the r.m.s. of their difference?

Lines 175−180 : I wonder if the NN training with CALIPSO would improve the agreement.

Lines 280−289 : as in general comment #1, I stumble here because there is a certain issue in the source dataset used for training, and at the same time we have a NN based on this dataset, which doesn't have this issue. I understand that this is possible, but one has to discuss this inconsistency because methodologically the neural network is not supposed to be different from the training dataset.

Fig. 5: It is not clear whether the average cloud cover here was calculated considering "shrinking" of the lat/lon box when moving towards the poles. I made this exercise for Fig. 5d (see below) and I've got close values (67.0% and 66.9% for area-weighted and simple averages, respectively), but the differences might be larger for the instruments with better coverage of the polar areas. In any case, it is recommended to use the area-weighted values. Please, check.

Fig. 5: this is more a comment rather than an issue and I do not require the authors to squeeze another panel to Fig. 5, which is already busy, but I think that this plot is worth providing here. In Fig. 1 below, I show the mean CIRS-LMD IASI and mean CIRS-LMD AIRS for 2015. As one can see, they are quite similar because the channels of (almost) the same wavelength were processed with the same methodology. The remaining difference is due to diurnal variation (Feofilov and Stuberauch, 2019). What is important, the similarity of these plots indirectly proves the point made in general comment #3.

Line 305 : please, mark these areas on the maps in Fig. 7.

Lines 345−346 and 125-129: please, provide the information on training time and number of samples.

Figure 1. Mean annual cloud amount retrieved by CIRS-LMD algorithm for (a) IASI-B (to be compared to Fig. 5d of the manuscript) and (b) AIRS (to be compared to Fig. 5f of the manuscript). The source data is the same as in (Feofilov and Stubenrauch, 2019).

Technical corrections:

Line 18: please, change "on the weather" to "for the weather"

Line 21: either "in detection … and in derivation" or "to detect … and to derive"

Line 66: "performance" would be better here

Line 75: "retrieves"

Line 88: comma is missing after "In the next section"

Lines 247, 255, 270, and elsewhere: the terms "more conservative" and "less conservative" require too much thinking, especially if clear sky attribution is mixed up with cloud fraction in one sentence. I would just write "assigns clear sky flags more (or less) often than …" to avoid any ambiguity.

Lines 341−342: please, reformulate to make a distinction between AVRR products and CIRS-LMD IASI product. In the current version, the sentence reads as if they are in the same category. As far as I understand from the manuscript, this is not the case.

References used:

- Chepfer H., G. Cesana, D. Winker, B. Getzewich, and M. Vaughan, 2013: Comparison of two different cloud climatologies derived from CALIOP Level 1 observations: the CALIPSO-ST and the CALIPSO-GOCCP, J. Atmos. Ocean. Tech., doi.10.1175/JTECH-D-12-00057.1
- Feofilov, A. G. and Stubenrauch, C. J.: Diurnal variation of high-level clouds from the synergy of AIRS and IASI space-borne infrared sounders, Atmos. Chem. Phys., 19,

13957–13972,
- Stubenrauch, C., and 22 co-authors, Assessment of Global Cloud Datasets from Satellites: Project and Database Initiated by the GEWEX Radiation Panel, Bull. Amer. Meteorol. Soc., 94(7), 1031-1049, doi:10.1175/BAMS-D-12-00117.1, 2013