

Interactive comment on “A high-level cloud detection method utilizing the GOSAT TANSO–FTS water vapor saturated band” by Nawo Eguchi and Yukio Yoshida

Anonymous Referee #1

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The paper by Eguchi and Yoshida refines and evaluates a detection algorithm for thin cirrus clouds using saturated water vapor bands in the shortwave infrared range measured by the Greenhouse Gases Observing Satellite (GOSAT).

Variants of the method have been studied previously for other satellites (and other spectral ranges) and, variants have been used for several years within the GOSAT standard retrievals [Yoshida et al., 2011, their section 3.1] and within alternate retrievals by the science community [e.g. Guerlet et al., 2013, their section 3.2]. In particular, Guerlet et al., 2013 (their figures 4, 5, 6) did cover quite some discussion of the method. Neither Yoshida et al., 2011, nor Guerlet et al., 2013, are referenced by the manuscript.

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While that might be negligence (Y. Yoshida is co-author here!), the question is whether the paper actually delivers some new insight. This could be argued twofold. First the algorithm is refined compared to the GOSAT standard retrievals. Second, the paper includes a comparison to CALIPSO data which has not been covered before.

So, topic-wise, the paper is suitable for Atmospheric Measurement Techniques (AMT). But scientific mass is limited. I would rate the progress compared to Yoshida et al., 2011, and Guerlet et al., 2013, as minor. Most importantly, the study (aiming at a methodological refinement) lacks a thorough quantitative comparison to the standard algorithms ie. there is no clear proof that the progress is actually positive except for some numbers in the introduction with a "not shown" attribute. There is the possibility that I just missed this key point because I could not follow the text.

The paper requires a major amount of rewording to become useful for the average AMT reader. There is a severe lack of clarity and completeness and, there is also severe language issues throughout the text; some paragraphs I simply did not understand (although I tried repeatedly).

Some detailed comments:

P2, L8+: This would be the place to refer to Yoshida et al. 2011 and to discuss the results by Guerlet et al., 2013. I do not understand what the quoted performance values against CALIOP for TANSO and CAI refer to. Is this part of the present work or is it previous work? If it is present, it appears strange to discuss it in the introduction. If it is other work, the respective references are required.

P2, L21: How can the method be applied to GOSAT-2, not even launched?

P2, L22+: This is a quite unfocused discussion of cirrus occurrence and properties which is far from complete; the number of references is small. I recommend focusing on thin cirrus and their importance because of their ubiquity in general and their impact on gas retrievals from satellites in particular.

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P3, L1+: It should be made clear that the paper improves and evaluates existing methods; it does not invent the principle.

P4, L31+: If I understand correctly, the method builds on the existence of a thermal infrared channel to group the shortwave infrared radiances according to the thermal brightness temperature of the scene. This must be made clearer, since it implies that it would not be applicable to most of the upcoming greenhouse gas satellites (except GOSAT-2) that lack the thermal infrared. Thermal infrared brightness temperatures are representative for the layer where the remaining optical depth toward top-of-the-atmosphere is about unity. So, grouping according to thermal infrared brightness temperatures should miss thin cirrus clouds. Is this a basic flaw of the method?

P5, L2+: What does it mean: "Using these typical spectral shapes as supervised data"? Add a more detailed description of the procedure. What is "supervised data"? Why are the entire band 3P spectra used and not just the saturated micro-windows identified in Fig. 2?

P5, end of section 2: What are the fractional contributions of the tests A,B,C? Is test C actually required?

P6, L16; Fig. 6: Why do the clear cases dominate? At a footprint of 10 km of the TANSO, the expected clear-sky fraction should be much less than 10

P6-7: The whole section 3.1 is hard to follow. I do not really understand sentences such as "The poor matching ratio over the ocean, especially during the boreal summer, seems to be due to the total water vapor amount during summer was approximately three times that in boreal winter (not shown) in addition to the lower clouds could be not detected." There is several grammar mistakes and in the end, I got lost. It continues "It is suggested that the reflection by lower clouds below 5 km and the water vapor amount may have an influence on the high-level cloud detection by the water vapor saturated method." Isn't that an obvious statement given figure 2?

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P6-7: The reason why this paper might be worth publishing is that the refined cirrus detection method is superior to the GOSAT standard algorithm. There appears to be an attempt to show that in appendix A but doesn't it rather show that the old algorithm has similar (or even better) performance as (than) the new one?

P7, L32: "It is clear that the TANSO-FTS water vapor saturated band method is superior for studying cirrus cloud features over a short period." A case study for 7 days without any independent validation does not support such far-reaching statements.

References

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Guerlet, S., et al., Impact of aerosol and thin cirrus on retrieving and validating XCO₂ from GOSAT shortwave infrared measurements, J. Geophys. Res. Atmos., 118, 4887–4905, doi:10.1002/jgrd.50332, 2013.

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