

Atmos. Meas. Tech. Discuss., referee comment RC1  
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## Comment on amt-2022-102

Anonymous Referee #1

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Referee comment on "On the influence of underlying elevation data on Sentinel-5  
Precursor TROPOMI satellite methane retrievals over Greenland" by Jonas Hachmeister et  
al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2022-102-RC1>, 2022

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### General comments

Hachmeister et al. present an analysis of TROPOMI methane column data to identify the causes of retrieval artifacts over Greenland. They investigate both the SRON operational methane retrieval and the University of Bremen WFMD retrieval. They find that the systematic errors over Greenland are largely explained by errors in the surface altitude datasets underpinning the two methane column retrieval algorithms, particularly for the WFMD retrieval. Applying a simple linear correction to the methane columns on the basis of surface height is sufficient to address the problem. The paper is a useful contribution and a good match for AMT. I recommend publication subject to minor revisions addressing the points below.

### Specific comments

- L. 52-54: Why give the old spatial resolution upfront and current resolution parenthetically? Consider reversing to give current specs first.
- L. 61: Recommend defining the GMTED2010 acronym here.
- L. 76: You use WFM-DOAS here but WFMD elsewhere.
- L. 93-96: Bias of a few percent could be significant for inference of regional emissions, no?
- L. 135: Recommend mentioning/defining the TROPOMI quality filter somewhere in Section 2.
- L. 141-142: What do you mean by "this effect merely shifts the reference point of the anomaly". As written it's not clear what the "effect" is. I think you mean that the selection of reference area defines the reference point for the anomaly, is that right?
- L. 147: Wouldn't removing observations with quality flag > 0.1 remove most data? Should it not be < 0.1? (I'm not familiar with ICESat-2 data conventions.)
- L. 149: Are these weights inversely proportional to the errors? Using the errors as weights directly seems like it would more strongly weight higher-uncertainty data.

- L. 150: Consider adding an equation here to unambiguously describe the approach.
- L. 168-169: This phrasing seems to imply that the differences between ICESat-2 and GMTED2010 are due to ice sheet dynamics, but isn't 100-200 m too extreme for that to be the case? I don't know much about ice sheets, but from Section 2 the error seems mostly related to the radar altimetry.
- L. 182: Recommend using "r" or "rho" for the Pearson correlation coefficient, not "p", because "p" can easily be mistaken for the p-value of the regression. This confused me on my initial review of the figures.
- Section 4.5: Is there a reason not to show maps of height-corrected WFMD v1.5  $\Delta XCH_4$  (or XCH<sub>4</sub>) and height-corrected operational  $\Delta XCH_4$  (or XCH<sub>4</sub>)? Perhaps these could be added to Fig. 10 or Fig. 12, or made into a separate new Figure. I understand the paper already has quite a few figures and you show how the scatter plots improve from the linear height correction – but I was surprised not to see how the final methane maps improve post-correction.
- L. 210 & 222-229: Going back to my question about ice sheet changes over time – can you say more about what causes the GMTED2010 data to be outdated? It would be useful to know what fraction of the surface altitude errors are due to ice sheet dynamics vs. altimetry errors.

### Technical corrections

- L. 14: "difference of GMTED2010... in the retrievals \*compared\* to a more recent..."
- L. 35: not clear what "and the surface extent" means here.
- L. 57: duplicate "a".
- L. 92: should "led" be "lead" (present tense)?
- L. 155: \*affect
- L. 161: Ice shield or ice sheet? Same question elsewhere in the manuscript where you use the term "ice shield".
- L. 206: should be \*were instead of "where".
- L. 212: you say "region 2" here but elsewhere "region two" – should use consistent terminology.