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Interactive comment on "Global height-resolved methane retrievals from the Infrared Atmospheric Sounding Interferometer (IASI) on MetOp" *by* Richard Siddans et al.

Anonymous Referee #1

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The authors describe a new methane retrieval product from IASI and show extensive evaluation with model, satellite, ground-based and aircraft retrieval. They highlight the novelty of their approach and make significant effort to compare their data with others, despite different weighting functions. The paper is well written and I would recommend publication, provided the following points are properly addressed:

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Major comments

- Averaging kernels have been commonly used in the modelling community for more than 10 years, in particular since the first MOPITT products. In this context, there is no point in comparing the satellite data to simple pressure weighted partial columns from a model. The corresponding figures and discussion should be removed, which would allow better focusing the whole discussion.

- In Section 6, it is not clear whether what the authors call "Estimated Standard Deviation" (ESD) is compared to misfits using individual IASI soundings (as suggested p. 1, l. 18 and p. 9, l. 21) or to misfits using averages of IASI soundings (as suggested p. 18, l. 3). The latter would be wrong in the absence of any information about retrieval-to-retrieval error correlations, but the former would require additional information (how is the IASI sounding selected for a given HIPPO profile? – it would be wrong to re-use a given HIPPO profile several times in the statistics).

- The impact of temperature uncertainty is **very** large (p. 10 and Figure 2) if we compare it to the natural variability of the partial column. In contrast to the ESD (as it is computed), it is very likely highly correlated in space and time. This is obviously a major limitation of the IASI information content and it should be much better highlighted and discussed. In p. 10, l. 18, the authors hope that future use of temperature retrievals from IASI will improve the situation, but (i) errors may be correlated between those retrievals and the assimilated spectral samples (at least through RTTOV) and (ii) ERA-Interim already assimilates HIRS and AMSU radiances from Metop (ok, not from IASI, but at least collocated temperature information is used).

- Section 4 lacks explanation. It suggests that total and partial columns are compared together, which would be wrong, in contrast to what is done later with Eq.

(13). The quality of the retrieved cloud parameters is also not discussed, even though these play a key role in the retrievals. The use of cloud observations from AVHRR instead of these ones suggests, maybe erroneously, that it is quite poor.

- Section 5.2: the authors have chosen to use a GOSAT "proxy" product even though it only provides the methane total column. However, "full-physics" products (e.g., available from the same data providers and co-authors) provide methane profiles: they would be much more appropriate and would (in conjunction with proper use of the averaging kernels – Rodgers and Connor 2003) avoid the not-so-clean trick of Eq. (13).

Minor comments

- There are a few typos that should be removed (e.g., p. 4 l. 13, p. 18 l.13 and l. 22).
- The format of the references is not unified.
- Methane units in all figures should be ppb rather than ppm.
- P. 1, l. 18: it should be said that the column is partial.
- p. 1, l. 25-28: the list includes both processes (e.g., fossil fuel use) and explicit sources (e.g., wetland emissions). The format should be harmonized.
- P. 2, l. 14: to be fair, the 2009 paper from LMD should be mentioned (Crevoisier et al. 2009). Actually, since that IASI methane product line has been well established, a few words to explain how the design of this product differs from the new one presented here would be good.
- P. 2, l. 18: Sentinel 5P is about to be launched and could usefully be mentioned.

- P. 3, l. 24: “error” is missing before “covariance”.
- P. 4, l. 1: km^2 rather than km.
- P. 4, l. 29: because of the large number of spectral samples, this way of doing does not make sense without accounting for the correlated errors (see also Stewart et al., 2014).
- P. 5, l. 28: it is not clear what the FM uses in input.
- P. 6, l. 6-7: the empiricism of this 4% (70 ppb !!!!) correction in the methane amount is quite perturbing.
- P. 7, l. 11: “statistics” is missing before “are estimated”.
- P. 20, l. 5-8: the last sentences of the paper are nearly the same as those of Crevoisier et al. (2013) that were actually a simple reformulation of the last lines of their 2009 paper. Either this idea is trivial, and it should be removed, or it would be fair to quote the previous papers.

References

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