

## ***Interactive comment on “In-flight calibration results of the TROPOMI payload on-board the Sentinel-5 Precursor satellite” by Antje Ludewig et al.***

### **Anonymous Referee #3**

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Section 5: The text states that in-flight linearity deviates from on-ground by no more than 1%. This seems rather large. Is this a statistically significant deviation? This deserves more discussion.

Section 7: This is an important topic, but the authors choose to devote only a short qualitative discussion to it. It would be helpful to the reader to provide some idea of the errors involved. At what error level does the flagging occur?

Section 8: The authors state they have only addressed geolocation in Bands 4-7. Geolocation in the shorter bands, esp. Band 3, are also important and validation should be possible except for Band 1. The authors should at least discuss what their plans are

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to validate these bands.

Section 9: A similar comment about wavelength registration. The authors imply there is no source of wavelength information other than from L2 products and there are no products providing this information for Bands 1 & 2. Yet the spectral registration in these bands is no less important than at longer wavelengths. The authors can at least acknowledge the problem and discuss their plans to deal with it.

Section 11: The discussion in this section (esp. the paragraph starting at line 260) was somewhat confusing. The authors should consider two alternatives to remedy this: provide a bit more explanation to the reader, or eliminate some of the details that are the source of the confusion. I recommend the latter because it's not clear what is to be learned from these details.

Section 12: In Line 285 the authors seem to throw cold water on any technique, other than on-board calibrations, to derive or validate radiometric change. It is quite reasonable that the authors have not had a chance to implement any of the well-documented techniques for validating the calibration, but they should refrain from suggesting these were omitted because they lack useful information.

I think I follow the 'competing change' argument described in Lines 330-335, but I doubt most readers will. The authors need to describe explicitly what about Figures 12 & 13 indicates increasing detector response competing with diffuser degradation.

Table 3: These numbers appear to be in percent. The authors should say so explicitly.

Section 13: The authors imply at the start of Section 12 that the reflectance calibration of TropOMI is an important quantity, but they fail to address its accuracy. If that is outside the purview of this paper, the authors should say so. The authors also fail to discuss in this section the effect that adjusting the irradiance calibration has on measured Earth TOA reflectances. Since the radiance calibration wasn't mentioned, the reader is left to assume that all the adjustments described in Section 13 are being

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applied in inverse to the instrument's reflectance calibration. What is the justification for doing so? The authors provide no insight as to why the pre-launch irradiance calibration might be so much in error. How do they know that the radiance calibrations are not in error by an equal or nearly equal amount?

Grammar comment: Use of the word "for" in connection with "corrected" should be accompanied by an object rather than a subject. "We correct for something" rather than "Something is corrected for."

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Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-488, 2020.