

Interactive comment on “Atmospheric ammonia retrieval from the TANSO-FTS/GOSAT thermal infrared sounder” by Yu Someya et al.

Anonymous Referee #2

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The manuscript “Atmospheric ammonia retrieval from the TANSOFTS/GOSAT thermal infrared sounder” introduces a recent product of ammonia from GOSAT. This is an important topic and should be published. I do have a few questions that I hope the authors can address before the manuscript is accepted.

Major Comments: 1. The “Discussion” section is relatively weak. I believe the largest difference between GOSAT and IASI is the retrieval/measurement sensitivity due to the thermal contrast differences between the two sensors that take measurements ~3-hours apart. This would explain why the differences (GOSAT-IASI) are smaller or even in opposite sign in the summer when the surface temperatures are the highest. Another evidence for the thermal contrast influence is that the differences between the two sensors are less at low latitudes. Diurnal cycles do contribute to the differences, but

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I would think the differences should increase in the summer, not decrease. The other possible causes (1-5) for the ammonia differences are less relevant. For example, “The GOSAT retrieval has a sensitivity in the middle troposphere as mentioned in Sect. 2, and therefore the scaled profile from the GOSAT retrieval likely underestimates the concentrations near the surface in these situations.” This is incorrect. Even though the spectral sensitivity is in the mid-troposphere, most of the ammonia concentration is near the surface. Also, if the AFGL profile amount is biased, it would affect the GOSAT retrievals similarly globally, but the large positive differences are at higher latitudes and in colder seasons. Additionally, these differences do not seem to be limited to the agricultural source regions, as discussed. Biomass burning signals are stronger, so all sensors should capture the signals well. 2. There is not enough evidence in its current form to support that the ammonia differences in the central African is due to aerosol contaminations. It could be due to surface emissivity, temperature, etc. Should state instead that the aerosol contamination is a likely cause of the difference in CAF but thorough studies are needed, at a later time. 3. The latter half of the paper is difficult to read. I have the following suggestions: a. In the “Results” section, figures were introduced first, then they are described in the paragraphs. Readers have to go back and force to find the relevant figures to understand the discussion. I suggest adding (see Figure xx) after the main sentences. b. The acronyms, ECH, CUS, EUR, CAF, SAM, WRU, and SEA etc. do not save a lot of space but makes reading much more difficult. I suggest eliminating them, at least in the text. The DJF, MAM, JJA, and SON are fine since they are commonly used. c. Figure 11 and 12 are not very helpful, neither were they discussed thoroughly. I would eliminate them, but this is up to the authors.

Minor Comments: 1. Page 4 Line 29, “The standard deviations of the a priori and the measured spectra for ammonia retrievals were assumed to be 20 and 0.3 K, respectively.” What is the unit of 20? 2. Page 9 Line 3, “. . . which iteratively decreases the difference. . .” should be which iteratively minimize the difference. . .

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