Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-261-RC1, 2019

Atmospheric Measurement Techniques Discussions

Interactive comment on "Validation analysis of deriving acetonitrile (CH_3CN) profiles by observations of SMILES from the International Space Station, in the stratosphere and lower mesosphere" *by* T. Fujinawa et al.

Anonymous Referee #1

Received and published: 4 October 2019

This article describes retrievals of CH3CN from the SMILES instrument on board the international space station and performs a validation through comparison with results from the MLS instrument.

The level of English in the manuscript could use some work, starting with the title. It is readable, but the phrasing is occasionally not quite correct. There are too many instances for me to provide line-by-line corrections.

I have some issues with the background discussion on CH3CN. While it is all technically

correct that the molecule is associated with biomass burning and pollution and can be used as a tracer for biomass burning events, most biomass burning activity occurs in the troposphere but the measurements reported here are in the stratosphere. Some of the discussion is not entirely relevant to what they are measuring.

They report the observation of a seasonal maximum of CH3CN in the upper stratosphere in February supposedly resulting from a peak in biomass burning during the time period from December to March. Age of air in the upper stratosphere is the order of a few years, but they appear to be suggesting they are measuring enhanced CH3CN in the upper stratosphere almost immediately. It should take a few years for young tropospheric air containing enhanced CH3CN to make its way to that atmospheric region. Intense fires can inject biomass burning products directly into the stratosphere (pyroconvection), but I don't think that is what they propose is happening here. CH3CN levels would be anomalously high in that case, and I expect the effect would not extend into the upper stratosphere. It would be confined to the lower stratosphere. The authors may want to rethink their interpretation taking transport times to the upper stratosphere into consideration.

In Figure 7, for altitudes below 40 km, the systematic high bias of results from AOS1 relative to results from AOS2 are quite evident. The authors make the statement that the differences between the AOSs is due to "sensitivity differences." I do not know what that means. The phrasing would suggest something like signal-to-noise ratio, but I would expect that to yield increased variability and not a systematic offset, unless perhaps the retrieval is being "pulled" more toward the a priori in the optimal estimation analysis because of the reduced signal-to-noise ratio. Is it possible to provide a few extra words to explain what is meant by sensitivity differences? It might be instructive to see the signal from the two different AOSs under similar measurement conditions, to see why one is yielding a larger VMR than the other.

In Figure 9, the top VMR two points from MLS are negative, which contributes to the strong magnification of the discrepancies between the two instruments in this altitude region. Negative VMRs have no physical meaning. You should probably point this out when discussing the discrepancies. This is an item in your favor.

How was the a priori state constructed? If MLS results went into generating the a priori used in the optimal estimation analysis, extra care might be required in using comparisons to MLS as part of a validation effort.

I feel that the description of the CH3CN retrievals was fine, and it looks like a good data product, but I was not convinced that their interpretation of the results (biomass burning on the surface yielding an almost immediate enhancement of CH3CN in the upper stratosphere) was correct.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-261, 2019.