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Comment on amt-2021-163

Anonymous Referee #2

Referee comment on "New sampling strategy removes imaging spectroscopy solar-smearing bias in sub-km vapour scaling statistics" by Mark T. Richardson et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-163-RC2, 2021

This manuscript documents an OSSE-type (Observation System Simulation Experiment) study of how the high-spatial resolution spectroscopy observation of total column water vapor from satellite observations should be sampled to understand the horizontal variability and structure of water vapor in the planetary boundary layer (PBL).

Topic of this study is important and suitable for the AMT. However, the manuscript suffers from several major issues and significant flaws as pointed out below. Its methodology (i.e., using a simple emulator instead of full simulator) is not justified and has serious potential problems. No causes and underlying physics are provided for the "solar-smearing bias", which is a key finding of this study. Even though the methodology is problematic, and the results are not explained, the authors still try to propose a universal "new sampling strategy" to the current and future high-resolution spectroscopy sensors. This is overreaching the say the least and could be misleading.

Based on these considerations, I strongly recommend rejection of this manuscript. It this study were published, the "emulator method", the "solar-smearing bias", and "new sampling strategy" could be cited again and again as if they were correct. But they are not, at least not justified by this study.

Major problems:

- The first major problem of the manuscript is the lack of important details on the methodology and the discussions are often too short and unsatisfying.
  - Although the concept of "total column water vapour" (TCWV) appears to be simple, the retrieval process can be quite complicated and involves many technical details, especially at high-spatial resolution. For example, when water vapor has both strong horizontal variation and vertical gradient, the solar-viewing geometry will become

important because the path-integrated water vapor can be significantly different from the TCWV, depending on how instrument geolocation/collocation is done. In such situation, observations from different angles need to be de-convoluted to re-construct the horizontal and vertical structure of water vapor. The manuscript briefly mentioned this issue in section 2.2 and 3.1 but the discussion is far from clear or satisfying. For example, it is mentioned "TCWVret from input TCWV, which is in fact the integrated water path along the solar path". But how is the "path-integrated water path" converted back to the TCMV (only times a cosine factor?)? Is the definition of TCWV dependent on solar and/or viewing angle? Although Figure 2 provides some information on the vertical variation of water vapor of the cases used in this study, the corresponding discussion in Section 3.1 is so brief (only one sentence) and obscured that it only raises more questions than answers. In particular, it is hard to tell how the author could "confirm that our derived values are indeed representative of bulk PBL statistics" from the figure, when there seems to be significant vertical variation of epsilon in the PBL.

  - Some other technical details are also missing. For example, how cloud mask is applied? Is it dependent on the sun-viewing geometry? If cloud mask is independent of sun-viewing geometry then there is apparently an inconsistency between the use of path-integrated TCWV and use of path independent cloud mask. Is the 3-D radiative transfer considered in the simulation or emulation? Previous studies have noted the "halo effects" of cloud in the so-called twilight zone. How are these 3-D effects of cloud treated in the study? Are they simply ignored (i.e., using 1-D RT model), or removed by cloud masking (then how?) or considered in the simulation?
- The use of a very simple retrieval emulator is not justified and raises many questions.
  - OSSE type of studies often use a "retrieval simulator" consisting of a "forward" RT simulator and an "inverse" retrieval simulator. The simulator should be as "realistic" as possible in comparison with the real retrieval to faithfully capture the influences of various factors on the retrieval. In contrast, this study only uses a seemingly naïve retrieval "emulator" (i.e., equation 4) and the only reason to justify this is "due to computational constraints". This "emulator" skips both the RT simulation process and the retrieval simulation step, and directly connects the retrieval to the input fields in a very simple way (linear). There is no discussion on the accuracy of this emulator in comparison with the "full OSSE simulator" if there is one. As a result, it is unclear if the artifacts in the "retrieval" is meaningful or simply due to the inadequacy of the emulator. It is also hard to imagine what kind of "computational constraints" the authors are referring to. This is a case study based on a handful of LES scenes. Many previous studies have performed full RT simulations, even 3-D RT simulations, based on LES scenes. How and why is the RT or retrieval simulation in this study so computationally expensive?
- The solar-geometry dependent retrieval bias in section 3.3 is interesting. However, I tried hard to find some explanation of the causes and underlying physics but didn't find any. There is neither any reference to previous studies or discussion on whether this phenomenon had been discovered before or completely new. **The authors didn't even bother explaining why this bias is called "solar-smearing" effect.** The word "smear" only occurred twice in the manuscript, one in the title and the other in the conclusion.
- Event though the "solar-smearing" effect is completely unexplained (and is based on highly questionable methodology), the authors still recommended the "new sampling strategy" to many current and future sensors. This totally unacceptable to me.