

# Review of Revised Manuscript “Calibration of a Water Vapour Raman Lidar using GRUAN-certified Radiosondes and a New Trajectory Method”

David N. Whiteman, Howard University

## General Comments

I believe this paper is nearly ready for publication and with a few small changes will be so. It will make a nice contribution to the literature concerning calibration of Raman water vapor lidars. My two main concerns are listed below. The first will require a mandatory (but very easily done) modification of the text. The second is not a required change but I do believe that the authors should address why they chose a column versus a cone in their text. With the other tests that are used to guarantee similarity of profiles, the use of a column seems unnecessarily restrictive.

1. as contained in the detailed comments, the authors still are using text that cannot at the current time be supported by available references when they discuss the ability of Raman water vapor lidar to detect trends in the UTLS. They state that such instruments are “well-suited for trend measurements in the upper troposphere and lower stratosphere” and that “lidar measurements are particularly useful for creating statistically significant water vapour trends of the Upper Troposphere and Lower Stratospheric region”. Such language seems to imply that studies have been performed using Raman lidar data that have revealed significant trends in water vapor. It is my sincere hope that such studies will be successful (and I know the authors are working on that currently) but to my knowledge there is presently no such published account. Therefore I think it is necessary to modify the language they use in this context. Perhaps the authors could mention that both NDAAC and GRUAN have designated Raman lidars as potential candidates for trend studies and that for such studies improved calibration techniques are needed. That then can be the context for this paper as they also look toward performing such studies themselves.
2. The authors find that the trajectory technique works better than the traditional below 4 km but not so above 4 km. They state that the reason perhaps for the result above 4 km is the reduced number of comparison points. What is happening, it seems, is that the radiosonde is floating farther away and thus leaving the 3 km radius column around the lidar. One wonders, therefore, if an acceptance cone instead of a column might make more sense. The other tests of similarity of profiles would still be in effect insuring that the profiles come from the same atmosphere. Having a cone could increase the number of comparisons and improve the statistics above 4 km. I would like the authors to add some small discussion for why a column was chosen versus a cone.

## Detailed Comments

1. First line of abstract states “Carefully calibrated and quality-controlled Raman lidars are well-suited for trend measurements in the upper troposphere and lower stratosphere, particularly for species such as water vapour.”
  1. This is a modified version of the original as shown by the blue text but does not address the primary concern. To my knowledge Raman water vapor lidar measurements have never been demonstrated to produce a dataset from which a statistically significant trend in water vapor can be discerned. I know that the authors are pursuing this goal and I sincerely hope they are successful. But, for now, no such demonstration has been made. Furthermore, it has been demonstrated that revealing a trend in lower stratospheric water vapor concentration using Raman lidar measurements would be significantly more difficult than for the upper troposphere. So, since trend detection has not been accomplished with Raman water vapor lidar in either the UT or the LS and it will clearly be more difficult to accomplish in the LS this opening sentence really must be revised or removed.
2. Lines 11-13 state “Lidar measurements are particularly useful for creating statistically significant water vapour trends of the Upper Tropospheric and Lower Stratospheric (UTLS) region, as they are able to take make long term and frequent measurements (Weatherhead et al., 1998; Whiteman et al., 2011b) (Whiteman et al., 2011b).”
  1. Again, this statement is not justified by any work that I am aware of. Note that the 2011b reference cited states that Raman water vapor lidar “could contribute usefully to monitoring trends in upper tropospheric water vapor although it will be necessary to have stringent quality control procedures in place to guard against errors [Whiteman et al., 2006; Leblanc et al., 2008; Boers and van Meijgaard, 2009] in

- upper tropospheric Raman lidar water vapor measurements. It should be noted in this context, though, that a large random error could mask the presence of a small systematic error making its detection more difficult.” So I do not believe that the cited reference justifies the sentence that it refers to.
2. For both items 1 and 2 in these comments, I suggest that the authors adopt a different perspective on trend detection using Raman lidar. They can instead talk about the potential for doing so and that their future work is aimed in exactly that direction. The current work, therefore, is helping to lay the ground work for future efforts in trend detection using Raman water vapor lidar. If the text in the manuscript adopted this kind of tone, I think there would be no issues with reviewers.
  3. I suggest that upper troposphere and lower stratosphere not be capitalized although with the abbreviation UTLS it is customary to do so.
  3. P 6, line 6 - “...backgrounds above 0.01 photon counts.” Unit: Per bin? Per second? Those units are used earlier and should be repeated here.
  4. Section 3, Traditional method. Line 12: calibration → calibrating
  5. Section 3, line 20. Presumably the saturation correction comes before the background subtraction so I would reverse the order of those processes in this sentence.
  6. P. 7, line 23. “...exponent is small.” Can you quantify this? It is important to convey that fact that making such approximations yields a useful correction to the data without greatly increasing the uncertainty budget.
  7. With respect to Eq 4, the 2003 reference is more suitable than the 1992 reference due to the presence of the temperature dependent terms.
  8. P 9, L 25 “... each of the scans is ...”
  9. P10, Line 8-10. “First, we use the latitude and longitude of the radiosonde, as calculated by the on-board GPS system, as the initial position for air parcel tracking. The air parcel is then assumed to have traveled in a straight line until the time it was measured by the radiosonde.” I’m confused by this sentence, should the last word here be “lidar”? Or is the lat/lon referred to that of the sonde upon launch and not at altitude?
  10. P12, line 10. “same procedure as in the traditional method is ...”
  11. P19 line 11. “a subset ... is shown...”
  12. P22 line 1-2, “ The contour ... shows ...”
  13. P22, line 3. “These rapid changes produce...”
  14. P22, line 8. “There” is capitalized and should not be ...
  15. P22, line 15 “This is again caused ...”
  16. Fig 8 Suggest binning at lower vertical resolution, perhaps 100m or more, to reduce some of high frequency noise.
  17. P23 line 9. You could add something like “ which is to be expected since the traditional technique tends to sample the same volume of air as the trajectory in the lower altitudes as shown in Fig 5.”
  18. P 27, line 14. Instead of “...maximum possible uncertainty”. How about “...maximum possible contribution to the total uncertainty”.
  19. P 32, line 19. GRAUN → GRUAN. Occurs twice.
  20. P 33, line 9. “...for the detection of water ...”