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Atmospheric Measurement Techniques Discussions

Interactive comment on "Quantifying TOLNet Ozone Lidar Accuracy during the 2014 DISCOVER-AQ and FRAPPÉ Campaigns" *by* Lihua Wang et al.

Anonymous Referee #1

Received and published: 16 June 2017

The manuscript reports on the intercomparison of three tropospheric ozone lidars, ECC ozone sondes and an aircraft-based chemoluminescence ozone instrument (P3B) during two field campaigns in Colorado in summer 2014. The goal is to investigate the accuracy of the lidars, that is to discover potential systematic biases, and to estimate and check their precision. This topic is well suited for Atmospheric Measurement Techniques. A thorough published characterization of system performance and accuracy certainly increases the value of these systems for tropospheric ozone research and monitoring.

While the manuscript presents substantial information about this intercomparison, I

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feel that the necessary subsequent scientific analysis and evaluation is still lacking. Such analysis would be needed to draw firmer conclusions about system precision and potential biases. As it stands now, the results are rather vague, more like a report. What is missing, to me, is a thorough scientific analysis of the presented material. Also missing are clearer messages on the resulting biases and uncertainties. The current $\pm$15% given in the abstract is rather wide and generic, hardly meriting a new paper. I feel that with the information inherent in the manuscript much tighter and more specific uncertainties could be given, especially when aerosol interference does not seem to play a large role. I recommend to address the following major points, before the manuscript can be accepted for publication:

1 General Comments and Questions

1. Figs. 1d, 2d, and 3c,d indicate that the TOPAZ system generally reports higher ozone. Where is this bias coming from? Is it significant? Does it have something to do with the signal recording / background subtraction? Why do these error sources not appear in Table 2?

2. Fig. 4c-e, indicates a significant high bias of the P3B measurements. Given that TOPAZ (and possibly also LMOL, see Fig. 3e-f) seems to have a high bias against the sondes, the high bias of the P3B would be quite substantial. I think this possible bias needs to be investigated in more detail. It also needs to be reported in the abstract.

3. If significant, the potential biases in 1.) and 2.) need to be reported in the abstract. Or the authors have to clearly explain why they think these biases are not significant, and how they are covered by the different systems uncertainty budgets (e.g. in Table 1).

4. Apart from potential biases, the authors also need to verify the precision estimates, e.g. those in Table 1. Since the statistical uncertainty (e.g. from photon counting statistics) is generally the largest uncertainty source for lidar ozone profile measurements, it is very important to calculate and report that properly. These estimated uncertainties then need to be checked using the statistics arising from multiple individual intercomparisons like the ones in this manuscript. This important check, to me, is missing completely here.

5. For example, the scatter / standard deviations from Figs. 3b,d , and 4b,d,f need to be compared to the estimated statistical uncertainty estimates available from the lidars. This probably requires additional plots. The information can then be used, on the one hand, to check the estimated lidar uncertainties, on the other hand to check the estimated sonde and P3B precisions. To me, such checks are a key component of an accuracy assessment. They are missing here.

6. Plots of average profile differences and their standard deviations should also be generated for the comparisons in Figs. 1 and 2. They also need to be included in the precision checks under 5., probably with additional plots and discussion.

7. In Figs. 4b,d,f, for example, it looks like the profile difference standard deviation is of the order of $\pm$5 % (1σ). This would indicate that the precision (repeatability) of the lidar profiles is about 5%, assuming that the precision of the P3B profiles is 1% as stated in 2.3 ($\sqrt{5^2 + 1^2} \approx \sqrt{25} = 5$). The precision of the 30 minute lidar profiles would then be better than the 8% reported in Table 1 (and much better than the 13% reported in Table 1). Similar considerations apply to the standard deviations in Figs. 3b,d. Especially in Fig. 3b, the (expected) decrease of lidar precision with altitude seems quite apparent to me, and this should be checked against the lidar uncertainty estimates (e.g. from photon counting statistics).

2 Detailed Remarks

Line 1: Since the authors are only examining three of the many TOLNet lidars and certainly not TOLNet as a whole, I would suggest to move TOLNet after “2014” or after “FRAPPE”.

Line 25: Replace “cross-instrument calibration” by “the network”. The authors are not calibrating the lidars (I hope), they should be self-calibrating. Network uniformity is what the authors are really looking for.

Line 32: Drop “In terms of range resolving capability”. I found this confusing, because there is really no investigation of consequences of the different and altitude dependent vertical resolutions of the lidars in the manuscript. This would be a whole separate issue, and therefore I would just drop this here.

Line 37: Replace “very good measurement accuracy for” by “that” and replace “making them” by “are”. I am not sure that $\pm$15% are “very good”.

Line 44: Drop “high fidelity”? Is $\pm$15% high fidelity? I don’t think so. P3B claims 1% if biases are corrected.

Line 47: Swap “scientists” and “managers”? Or do the authors mean modeling and satellite managers?

Lines 56, 57: Replace “that . . . their” by “of”.

Line 59: Move “range resolution” after “operating ranges” in line 60. Range resolution is not really a hardware thing, and is much more determined by software.

Line 64: Add “can” before “form”?

Lines 67, 68: Drop “This particular study . . . United States”? Is this relevant? Are the authors sure it is true? Was there no comparison, e.g. between TROPOZ and the Table Mountain tropospheric ozone DIAL?

Line 82: “selected” instead of “selective”. Don’t see how some sites would be more selective than others.

Line 82: Replace “profiles of ozone measurements” by “ozone profiles”.

Line 93: Replace “lasers” by “pulses”. Otherwise this would be a very expensive system indeed.

Line 102: Remove “zenith looking”. As is now, this is confusing and contradictory.

Line 117: Add “s” after “measurement”. Lines 117-127: This is a lot of text to say that, in the end, the system was just pointed to zenith. Shorten.

Lines 144,145: Drop “database” and “to calculate differential”.

Line 152, 153: I do not understand what is done here. 5 points at 6 m hardware resolution would be 30 meters. 450 meters at 6 m hardware resolution would be 75 points. Explain / correct, also in Table 1.

Line 155: Please cite Leblanc et al. 2016 here. The authors should also include the other Leblanc et al. 2016 paper(s) on ozone profile uncertainties in the references. Also, the results here, i.e. range resolutions, ozone uncertainties and accuracies need to be properly put into the context of these papers, here and in other places in the text.

Line 168: Remove “non-standardized”. Because it is so system specific, standardization is not really a criterion/ issue.

Line 174: Remove “maximum”. What would that mean?

Line 179: Leblanc et al. 2016 on ozone profile uncertainties should be cited here, and should be put in context. Somewhere the authors should also mention that lidar uncertainty increases a lot with increasing altitude/ range.

Lines 181 to 191: To me, this is a bit backward. First the authors give the principle, then end results, and then the authors go back to the principle again. Rewrite / reorder.

Lines 108/109: These two citations should probably come before, on line 106 right after "observations".

Line 115: Replace "cloud interferences" by "clouds".

Line 223: Similar standard deviation could also be due to similar noise / precision / uncertainty. In fact, I think this seems to be the case from 13 to 17 UT, whereas similar variations seem to captured from 18 to 22 UT. Please reword.

Line 227: As mentioned above, please also show the mean and standard deviation profiles of these ozone differences. Same for Fig. 2.

Lines 265 to 271: I think this needs to be thought through much better. Are the sondes too low? Why would averaging time affect a bias? It should only affect the noise / significance. Same goes for SNR. Biases that are not resolvable/ not significant / within the uncertainty margins should not be discussed at all.

Lines 286 to 287: Why not the P3B? Figs. 4d and 4f look very similar. Many things point towards P3B being high. Same as the discussion of lidar sonde differences this discussion is to short. A lot more needs to be done / said here. See my major comments.

Table 2: Are these uncertainties 1σ or 2σ?

Figs. 1d and 2d. Please plot (some/typical) error bars for these time series.

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