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Atmospheric Measurement Techniques Discussions

Interactive comment on "Upgrade and automation of the JPL Table Mountain Facility tropospheric ozone lidar (TMTOL) for near-ground ozone profiling and satellite validation" *by* Fernando Chouza et al.

Anonymous Referee #1

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The manuscript by Chouza et al. describes autonomous technique for an ozone lidar system, introduces a recently added near-surface channel, and presents intercomparison results of validation using other in situ measurements. This manuscript is well written in general, is technically sound, and has some original aspects which could be valuable for other lidar groups. I have two slightly major concerns/comments: 1) clarify the 5% accuracy in the abstract to avoid confusion and 2) suggest adding description of the auto alignment feature. I recommend to publish after addressing these comments.

Specific comments:

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Discussion paper

P1, L8-9, “An agreement of 5 % or better with the ozonesonde down to an altitude range of 100-m above ground was demonstrated.” It looks very ambitious to conclude the accuracy “5%” just based on one intercomparison experiment between the lidar and tethered ozonesonde (Figure 8). There are two coincident ozonesonde profiles in the May 25 case study (page 14-18 and Figure 11). The discrepancies between the sondes and lidar are not quantified well in the context although the explanations about these discrepancies are provided. But it can be seen that these discrepancies have exceeded 5% at many altitudes even excluding the STE layers in Figure 11 by a visual check. So, it looks like the authors made favorable conclusions by using selective results. Please clarify.

P1, L 13-14, “These comparisons revealed localized differences between sonde and lidar, possibly owing to the differing vertical resolutions (about 52m for lidar and at least 120m for sonde).” What is the corresponding altitude range for the 52-m resolution for the lidar retrievals? 52m is probably the highest resolution of the lidar while ozonesonde has a constant resolution about 100m from surface to $\sim$35km.

P2, L9, have you given what TEMPO stands for somewhere?

P3, L16-17, pure H2 or D2 without buffer gas? Can you give the conversion efficiencies (or the UV pulse energy sent into the sky) for both wavelengths?

P3, L26, what are the sizes of the receivers for the low altitude channel?

P4, L2, what does APC stand for? There are a lot of acronyms in this paper. They are well known in some particular fields (e.g., computer science, satellite), but probably not so for everyone and need to spell out at the 1st time in use.

P5, L13, “the alignment module was. . . the acquisition.” Alignment of laser beam with receiver is critical for correct lidar retrievals. Since autonomous alignment is an important feature of the whole autonomous system, can you provide additional description about how your auto alignment works?

P6, L1, Table 1 showing detailed numbers of lidar profiles in each month is not closely related to this paper.

P7, L10-13, 1) the location of the citation is not appropriate. Suggest to put (Kovalev and Bristow, 1996) and Wang et al., 1997) after "dual DIAL measurements". 2) The statement is a little confusing, or incomplete. Reduction of aerosol interference will naturally reduce the uncertainty in DIAL retrieval. But you say reduction of aerosol interference will increase the ozone retrieval uncertainty potentially by inducing other larger errors. The readers will wonder what are the uncertainty sources with larger errors. Please clarify.

P7-8, the authors try to justify the ozone DIAL retrieval with a wavelength pair of 266-289nm without an aerosol correction with two pages of words. This might be ok by considering that the focus of this paper is the description of the autonomous features of lidar system and this particular lidar is located at a high-elevation site where the sky is relatively clean. But I have following comments: 1) the ozone DIAL can't measure accurately in the presence of fire smoke where the aerosol gradients could be even larger than the assumed profile in Figure 4. Especially forest fire occurs frequently in CA recently. 2) The aerosol layer is assumed at a low altitude, 500m AGL, in Figure 4. The percentage uncertainty due to aerosol interference could be much larger at a higher altitude because of ozone number density typically decreases with altitude in troposphere. In addition, the 266-nm laser is not able to cover the altitude higher than 3000m AGL as you mentioned in the paper. 3) There exist some aerosol correction methods for two-wavelength ozone DIALs such as Alvarez II et al. 2011 (cited earlier for another purpose); Immler 2003, Kuang et al., 2011, Sullivan et al., 2014. I believe you still need to mention them although you don't apply aerosol correction because of some reasons.

P10, in terms of the error induced by the timing offset between the two wavelengths, similar issue has been investigated in previous literature (e.g., Figure 4 in Kuang et al., 2013). How does your result compare to previous ones?

P12, L24, “there is a very good agreement (less than 5% difference)...”. This statement is not accurate since you mention one sentence later “some deviations between 70 m AGL and 100 AGL” are “less than 10%”. Maybe should change to say “mostly less than 5%” or constrain the altitudes above 100m.

P12, L29, “a 17-sample Savitsky-Golay derivative filter was used”. Why is the filtering window length different with “9 samples for the analog detection mode and 31 samples for the photon-counting mode” which was mentioned earlier?

P18, L21, “a good correlation between these two quantities is observed”, what is the correlation coefficient between the Lidar and i49? Is 10% (a mean value) an exact value or an approximation? Looks like Fig. 12 has sufficient samples so that an exact value of the mean difference can be statistically meaningful.

P18, L25, “The result of this comparison show a very similar difference of about 5-10% between ozone sonde measurements at 100m AGL and surface ozone measurements.” If “the result” is an unpublished result and is not given in the paper either, I suggest adding “not shown” to avoid confusion.

P18, L 31, “As a result, the system is now capable of performing measurements autonomously from about 70m AGL up to about 15km.” Also P19, L9, “...the lowermost 70m AGL...”. There are large discrepancies between the lidar and tethered sonde below 100 m (Figure 8), which actually has not been explained very in the context. Moreover, the first lidar altitude grid is also 100m for the intercomparison with the surface observations (Figure 12). These facts clearly suggest the lidar retrievals below 100m are relatively unreliable. I suggest changing “70m” to “100m” to avoid confusion.

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