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Atmospheric Measurement Techniques Discussions

Interactive comment on "Aircraft based four-channel thermal dissociation laser induced fluorescence instrument for simultaneous measurements of NO_2, total peroxy nitrate, total alkyl nitrate, and HNO_3" *by* P. Di Carlo et al.

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We found the review of this Referee to be constructive when she/he suggests to include some more references, but, from our point of view, very inconceivable in his/her quick conclusion about the novelty of the work reported here. In the following points we try to explain in detail why we found this criticism not appropriate:
a) The method of single frequency LIF was described previously only for ground-based systems and only for NO2 measurements (Matsumoto et al., 2001, Dari-Salisburgo

et al., 2009) never for aircraft LIF and never for ΣPNs, ΣANs and HNO3 measurements.

b) As reported in the abstract, here we have used for the first time the single frequency LIF technique on aircraft and we have used 4 cells to measure, for the first time on aircraft, simultaneously NO2, ΣPNs, ΣANs and HNO3, that means that in our case we have continuous observations (usually averaged at 1 sec., but we acquire at 10 hz) of all of these species without gaps. In fact, to date, the only system that measures these species on aircraft platforms is the TD-LIF developed by Prof. Cohen group at University of Berkeley (Day et al., 2002; Perring et al., 2010) but that system uses only 2 cells therefore they have to switch between them so that: "for every 2 min duty cycle they have three 20 s average measurements of NO2, two 20 s average direct measurements of ΣPNs, one 20 s average direct measurement of HNO3 and one 20 s average measurement of ΣANs using interpolated ΣPNs values" (Perring et al., 2010). The fact that in our system we do not have to interpolate observations of one species to retrieve the other is, from our point of view, a novelty and an improvement that reduces the uncertainty in data retrieval, and gives continuous observations (without gaps) really important especially in case of aircraft measurements.

c) This work reports the first aircraft TD-LIF that weighs less, requires less power and eliminates hazardous liquid materials required in dye lasers (for safety on board this is a good choice in the development of aircraft instruments), compared to the only similar one used so far worldwide on aircraft (Day et al., 2002; Perring et al., 2010), and this could be a good reference for the development of future aircraft instruments.

d) To our knowledge this paper reports the first in-flight intercomaprison of a single frequency LIF measurements of NO2 with chemiluminescence observations, therefore we think that a report of inflight intercomparison is a valuable tool for new instrument development and airborne observations interpretations.

e) To our knowledge this paper reports the first in-flight intercomaprison of TD-LIF observations of ΣPNs with N2O5 observations made by a BBCEAS system. We think that this is another novelty of this manuscript really helpful for future observations of species like ΣPNs and N2O5, not so frequently measured on aircraft.

Regarding the 4 detailed points raised by the Referee these are our responses:
Point 1: We disagree with the reviewer because the NO2 detection by LIF reported by our group (Dari-Salisburgo et al., 2009) was a single cell system to measure only NO2 with a flash lamp pumped laser for ground-based measurements, here we have reported a new system that includes a new diode pumped laser and 4 cells to measure for the first time on aircraft NO2, ΣPNs, ΣANs and HNO3 simultaneously. Moreover here the thermal dissociation system is implemented and described, all the improvements to use the system on aircraft are described. Finally Fuchs et al. 2010 reported a nice chamber intercomparison of several NO2 systems, but a single frequency LIF was not included in the instruments list of that intercomparison, as erroneously suggested by the referee.

Point 2: We appreciate the suggestion of the reviewer and we will add the very recent references that she/he suggested to update the introduction.

Point 3: We agree with the Referee that this is a good point and really important in case of observations in forest environments where isoprene nitrates can be abundant. In fact this kind of check was made, for example, by Perring, et al. (2009), in case of observations in areas impacted by high isoprene emissions and they report no interferences in other heated channels and no loss in the ΣANs channel where it is expected to be observed isoprene nitrates. We did this test in a previous version of

our system with longer quartz tube and we did not observe interferences nor loss; to be honest we did not repeat this test in the aircraft configuration, but it is a good suggestion and it is a further check that we are planning to do quite soon. In any case for observations of urban plumes as we did during the RONOCO campaign we are quite sure that isoprene nitrates is of small contribution to the observed ΣANs.

Point 4: We thank the Referee for suggesting this very recent and helpful paper.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 8759, 2012.