Editor Comments (Andreas Hofzumahaus)

I am pleased that the authors have addressed the referees' comments in detail. With respect to the main comment by referee #2, the treatment of error propagation has been adequately corrected. Nevertheless, I am not fully satisfied with the error estimates.

- The CLD instrument measures NO and NOc by chemiluminescence. The NO detection involves photon counting and the statistical noise of the signals should follow Poisson statistics. The absolute 1σ precision should scale with the square root of the NO signal for a given integration time, whereas the relative precision (in percent) is expected to decrease with the square root of the NO concentration. In line 156 of their revised manuscript, the authors specify a general constant value for the relative precision (5%). How can that be? Same arguments apply to the measurement of NOc.
- In line 157, the authors calculate a total relative uncertainty of NO from the signal precision (5%) and the error of the calibration gas mixture (3%). The same is done for NOc. In the calculation of the total uncertainty for NO2, the total uncertainties for NO and NOc are treated as statistically independent. This is not correct, because the calibrations of NO and NOc rely on the same calibration standard. The correct procedure to calculate the uncertainty of NO2 would be to calculate first the precision of NO2 from the precisions of NO and NOc. In a second step, the precision of NO2 can be combined with the uncertainty from the calibration mixture.
- In line 166, the authors assume that the error of the NO measurement is zero when no NO is present. However, when NO approaches zero, the signal noise will be dominated by the instrumental background (e.g., dark signal) which is responsible for the limit of detection. The role of background noise should be clarified.

Further technical comments.

- In the paper, HO2 data are called "preliminary" because it has not been corrected for interferences from organic peroxy radicals. Since the discovery of possible RO2 interferences in the detection of HO2 by LIF, uncorrected HO2 is generally called HO2* (e.g., Lu et al., Atmos. Chem. Phys., 12, 1541–1569, 2012). I recommend to adopt this nomenclature.
- In line 199, the possible RO2 interference is treated as a statistical error when calculating the uncertainty of HO2. This is not appropriate. The interference is an additive positive bias. If the interference is 7%, then all HO2* values will be too high by this amount. Please clarify: where does the value of 7% come from? Why don't you correct the HO2* data by this amount?
- In Table 1, I have the impression that TMU is used either for precision, or accuracy, or a combination of both. I suggest to specify instead the limit of detection and accuracy (usually related to calibration) for each substance in separate columns.
- Equation (4) in line 271 looks weird. I find the re-definition of RO2 as the total sum of peroxy radicals (including HO2) confusing and inconsistent with other parts of the paper (e.g., line 196, line 239, reactions R4-R6). According to the general nomenclature, "RO2" should be used for the sum of organic peroxy radicals without HO2. I suggest to modify the equation accordingly:
  $P(O3) = k_{NO+RO2}\,[NO]\,[RO2] + k_{NO+HO2}\,[NO]\,[HO2]) \cong k_{NO+HO2}\,[NO]\,([RO2] + [HO2])$