

Interactive comment on “Six years of high-precision quasi-continuous atmospheric greenhouse gas measurements at Trainou Tower (Orléans Forest, France)” by M. Schmidt et al.

Anonymous Referee #1

Received and published: 26 February 2014

Reliable monitoring of greenhouse gas concentrations is one of the most important goals of atmospheric chemistry and all the technical solutions and inventions should be published as soon as possible. From the other hand, a field evaluation of the analytical instruments requires a lot of time and usually applicability of the technique is proofed after few years. In this light I find this paper very much expected especially in scope of AMT. I hope that the further remarks will not be treated as an overall criticism of the paper but rather critical comments to which referee is obligated.

General remarks: 1) It is not easy to understand the aim of the article. - The paper is rather balanced to present technical aspects of instruments measuring trace gases at

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different level of the tower. For this purpose one would expect definitely more detailed description of the instruments and validation of the sampling techniques itself (side instruments, valves, cause of their malfunctions and repair routines, validated opinions about used gc, generators, gases, deep statistical analyses commonly used in technical sciences etc...) rather than simple descriptions of records. By this I don't mean that the record itself is not valuable, in opposite, its detailed discussion would be very interesting but not in AMT. I would suggest to remove the part of the article describing trend analysis but remain the record for overview of the instrument work. Much more interesting are results of target gases and working standard measurements presented in paper. Authors may refine it with more statistical analysis and a bit deeper discussions (e.g. noise analysis or proper detailed nonlinearity).

- CARIBOU is a kind of modifications of commercial instrument, in this case comparison of original and modified instrument would be reliable for evaluating of proposed solutions. Actually the most common technique for high accuracy CO₂ measurement is CRDS, authors should compare both instruments aiming at possible applicability of presented technique, also comparison of Loflo would increase value of the paper. Any new instrument shell be validated in some way. If the CARIBOU instrument is not recommend for this measurement in future please indicate possible field of application.

2)Some serious statistical mistakes have to be corrected, what is especially important in case of technical articles. It regards both: notation and calculation. Authors of technical papers dealing with metrology should apply the commonly accepted way of uncertainty expression: e.g. JCGM (former GUM : <http://www.iso.org/sites/JCGM/GUM-introduction.htm>) to obtain the consistency of applied methodology.

Detailed comments: -Do the authors posses the footprint calculation or any other model validation of the impact of the surrounding (or regional) area on the results obtained at different levels of the tower, can it be included? Without that the site description is not validated.

- From the abstract, but later at the instrument description there are constantly mistakes

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in rounding of the value and standard deviation also notation may be a bit misleading as it is not expanded uncertainty. Notation 1080 ± 0.03 is incorrect.

-Pump is not a process variable itself (however pressure, voltage and angular momentum might be).

-Page 8 line 11: add concentration or mixing ratio (if applicable) after CO₂ in parenthesis.

-Standard gases are very short description - please explain what cylinders (volume, alloy, producer) and pressure regulators etc. are applied.

-Chapter 3.2.2: application of averaging values of reference gases measured 5 years from each other seems to be unjustified especially if the changes are small and might be due to the drift of the real air composition inside a cylinder.

-Chapter 3.2.2: What is an order of the drift correction, can it be presented on the graph?

- Why ambient air is not measured at the level 5m by the CARIBOU, what are the purposes to include only one 100m analyses in each pyramid while 50 and 180 are measured twice.

-Chapter 3.3: What method was used for test of catalyst efficiency, how variable is it? Why temperature of catalyst was not higher (Agilent gc can control temperature zone up to 400)?

- Indicate the letter Q of Haysep columns (or make the reference to the table) in ECD branch. It is also important to put the supplier as there are few different companies producing this columns (unequal grade).

-Page 10 line 16,28 - not analysed but detected (analysis require reference, integration etc.)

-Page 10 line 27- Backflush system are commonly used for purpose presented by

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authors but in this special case both columns are of the same kind. Only one aim of backflush application in this case is to shorten the analyse time.

-Linearity of ECD: When authors apply the linear correction function also intercept is important what is its value, what are uncertainties of both parameters.

-Chapter 3.4. Why meteorological sensors are placed only at 2 heights while authors are suggesting importance of temperature gradient influence on trace gas measurements.

- There are no 14C results presented as well no ABL heights obtained from Lidar.

-Wrong rounding of flask-gc differences.

-Chapter 4. There are no uncertainties of: gradients, diurnal variability and trends.

-can authors comment if the CARIBOU was exchanged in 2013, what are the plans for CARIBOU?

-Table 1. What is the value of drift before correction.

-Table 1. Sentence "The reproducibility of the standard gas is 0.02 ppb." is perhaps in wrong place. Which cylinder, why this information is placed here. values of parameter (not only standard deviation) are interesting.

-Table 2. What does "regulation" refers to?

-Table 2. Molecular Sieve 5A instead of Å or indicate the problem to the editor?

-Table 2. What "30" means?

-Table 3. Hydrogen most actual scale is MPI 2009.

-Table 4. Maybe it is coincidence but for 100m level overall average 2007-2012 is equal to average of yearly mean values - please check the calculation as for other levels results are different due to the different representation of each mean.

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-Table 4. Is overall stdev (2007-2012) really so low while for 2007 it was 1.75, only 2011 is lower than 1.00 but not substantially.

-Fig.1 Why model is used while wind rose and distribution might be obtained from direct measurements for the years when measurements were performed.

-Fig.1 Is any important information coming from the wind speed cumulative distribution while the distribution is presented?

-Fig 2. Ethanol instead of "ethalon"

-Fig 3. A lot of text with no apparent reference in the text of the article. complexity of the figure is OK but this require more detailed description inside the paper.

-Fig 4. Two valves marked #4

-Fig 4. It would be good to mark the separately heated zones.

-Fig 5. Scale for CO₂ is to big

-Fig 5. Target value of H₂ is constantly drifting (aprox 15ppb/year)- there's no comment to this in the paper.

-Fig 6. "(Electron capture detector)." not necessary.

-Fig 6. Why the fig5 and fig6 have the different x-scale labelling?

-Fig 7. The diurnal cycles from neighbouring months should not be connected with lines (no meaning).

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