

# ***Interactive comment on* “Greenhouse gas measurements from a UK network of tall towers: technical description and first results” by Kieran M. Stanley et al.**

## **Anonymous Referee #1**

Received and published: 18 November 2017

review of amt-2017-349

Title: Greenhouse gas measurements from a UK network of tall towers: technical description and first results

Authors: Stanley et al.

General comments:

The manuscript presents a comprehensive overview of the recently set-up network of greenhouse gas measurements at tall towers in the UK. The paper is clearly structured and well written. It provides much useful information for readers that also aim

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at establishing monitoring capabilities at tall towers. Therefore, the manuscript merits publication in Atmospheric Measurement Techniques. However, the paper summarizes several things already published in the literature. Thus, I feel that some paragraphs of the manuscript could be shortened by minimizing repetition of already published technical description.

At the same time, the paper somehow lacks novelty or the author missed to emphasize the new approaches applied here. I suggest to highlight the new approaches and, in addition, to provide more information likely of interest for the reader. Issues to be addressed in this respect are (a) the required maintenance of the measurements (e.g. how did the regular maintenance look like, how many maintenance (regular as well as “emergency”) visits were needed over the years); (b) how did the troubleshooting look like; (c) which major technical problems were the authors facing; and (d) add a paragraph on lessons learnt in the conclusions.

How does this exercise refer to European efforts like ICOS, in terms of instrumentation, quality control and data processing?

Moreover, there are also a few minor comments that should be considered prior to publication.

Specific comments:

Page 1, line 17: I don’t agree with the wording “automated custom-built instrumentation” here as a large part of the instruments is commercially available.

Page 2, lines 11-13: “. . .independent emission estimates for comparison with the UK national inventory . . .”: if this is mentioned in the abstract, I expected to see some related results in the manuscript but I couldn’t find it.

Page 4, line 6: “. . .except MHD which samples every 20 minutes . . .”: this statement is in contradiction to Table 1 where a Picarro G2301 is listed for MHD.

Page 5, line 19 – 20: Here for RGL and also further below for TAC: what is the rationale

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for measuring at several heights?

Page 7, lines 3 – 10: why Synflex tubing is used at RGL and TAC while stainless steel tubing is used at MHD. What are the pros and cons for using one of them? What is the difference between Synflex 1300 and Synflex 3000?

Page 7, line 17: I suppose that only the bowl is made out of Perspex. What is the material used for the other wetted parts like the body, the seals and the gaskets?

Page 9, line 22 to page 10, line 1: If the temperature is maintained at  $318 \pm 0.004$  K, line 1 must read a few / thousandth of a K . . . , correct?

Page 10, line 11: 0.25 % volume ratio of water: which dew point is that?

Page 10, lines 12 – 14: did you test for potential CO<sub>2</sub> losses in the Nafion?

Page 10, lines 17 – 21: Correction for water vapour interferences: are the correction coefficients listed in Rella (2010) the ones that are implemented in the Picarro software? If so, the direct Picarro output internally corrected for H<sub>2</sub>O cross-talk can be used right away, correct? Did you also use the Rella (2010) factors for TTA when running the measurements w/o dryer? If so, why didn't you use individually determined correction factors as suggested by Rella et al. (2013) (<https://www.atmos-meas-tech.net/6/837/2013/>).

Page 12, lines 11 – 12: It reads like it was a new idea of the authors to use 5% CH<sub>4</sub> in Ar (i.e. P5) instead of CO<sub>2</sub>-doped N<sub>2</sub> as carrier gas. However, this is already done by many groups for many years. You may refer to Schmidt et al. (2001) (JGR; <http://onlinelibrary.wiley.com/doi/10.1029/2000JD900701/epdf>).

Page 16, line 13: what is a “significant change”? Was the change detected automatically or manually (visually)? Which criterion was applied?

Page 16, lines 14 – 15: “A second order non-linear curve is fit to the data . . .”: Does that mean that the Picarro has a non-linear response? If so, please state it clearly

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and elaborate on it and quantify the effect when erroneously neglecting the non-linear response.

Page 17, lines 9 – 11: If I understand correctly, only a one point calibration approach is applied. This only works when assuming no detector signal at zero concentration, right. Was that tested and how was the one point calibration approach applied when a GC-MS signal  $> 0$  was detected for species-free air?

Page 17, lines 25 – 26: how did you make sure that there were no traces of N<sub>2</sub>O and SF<sub>6</sub> in the zero air? The zero air, was it real air having N<sub>2</sub>O and SF<sub>6</sub> trapped or was it a N<sub>2</sub>/O<sub>2</sub> mixture? How about the Argon content in the zero air?

Page 18, 8: troubleshooting is mentioned here but it remains too vague if and how and how often troubleshooting was required. See one of my general comments above.

Page 18, line 18: I suggest skipping the explanation of the naming convention of the stripchart files. This is irrelevant.

Page 18, line 23: remove “with time stamps corresponding to the beginning of the measurement, and stored“ It isn’t of importance here.

Page 19, line 5: add a table with the thresholds for the maximum allowed standard deviations?

Page 19, line 24: remove explanation of the naming convention.

Page 20, line 1: is it important that there was a 4:1 compression ratio?

Page 21, lines 3 – 5: how often did it happen that the processing routines filtered false negatives? Is that a time consuming (and important) task to review the automatically filtered data?

Page 21, lines 9 – 10: Is the flagging of spurious data a manual process? If so, I suggest clarifying it by saying “Spurious data are manually flagged and a justification ... can be added and logged.”

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Page 21, line 11: what is “GCcompare”?

Page 22, lines 18 – 19: where are the 21 nmol mol<sup>-1</sup> coming from? Add a reference.

Page 23, lines 2 -3: Add a statement that the trend of 0.8 nmol mol<sup>-1</sup> per year is lower than the global trend. You may refer to the latest WMO GHG bulletin #12 ([https://library.wmo.int/opac/doc\\_num.php?explnum\\_id=3084](https://library.wmo.int/opac/doc_num.php?explnum_id=3084)). What do you mean by seasonal trend, isn't it the annual growth rate?

Page 23, chapter 6.2: this is all largely text-book knowledge and can be considerably shortened.

Page 25, Summary and conclusions: It only summarizes what was said before. I would like to read some kind of outlook and some recommendations that go beyond a simple description of the setup as given above. Topics to be potentially addressed could be: are there any modifications planned (based on some lessons –learnt); are there any major flaws in the setup which cannot be easily changed anymore; with the experience gained during the few years of operation, would the setup again look the same when you may be able to once more start from scratch?

Page 26, lines 11 – 13: The reference given here to underline the benefit for such measurements for GHG inventory verification was published in 2011, i.e. before the presented measurements were implemented. Either remove the reference to the emission verification on page 26 (and in the abstract) or elaborate on the benefit of additional observations for the GHG inventory assessment based on tall-tower measurements and inverse modelling.

Page 31, footnote a to Table 1: I cannot find the data on the ICOS carbon portal as indicated.

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