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Atmospheric Measurement Techniques Discussions
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Interactive comment on "Recent six-year atmospheric CO_2 concentration at the summit of Mt. Fuji observed by a battery-powered CO_2 measurement system" *by* Shohei Nomura et al.

Anonymous Referee #1

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GENERAL COMMENTS

The paper present a new proof of concept and interesting application of a system for the execution of long-term continuous CO2 measurements to be used in a remote, unmanned measurement site characterized by the total absence (for several months) of electric power. The authors provided enough technical details on the proposed set-up to allow reproduction at other measurement sites. Moreover, new data were presented about CO2 variability at Mt. Fuji (Japan).

My first general comment is that I'm not completely sure that this paper by Nomura et al., is completely fitting to AMT. As specified by AMT web site, AMT is dedicated to the" publication and discussion of advances in remote sensing, as well as in situ and

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laboratory measurement techniques for the constituents and properties of the Earth's atmosphere. The main subject areas comprise the development, intercomparison, and validation of measurement instruments and techniques of data processing and information retrieval for gases, aerosols, and clouds." The paper by Nomura et al. presented an automatic, low-consumption system (even if based on already well-known and used commercial system), for the execution of CO2 measurements at Mt. Fuji or at other remote, unmanned, adverse-weather measurement sites. Thus, for this aspect, it fits without any doubt to AMT. However, my feeling is that the paper is too unbalanced towards the analysis and exploitation of the CO2 data series provided by this system or in demonstrating the geographical representativeness of the measurement site (which is more fitting to ACP in my view), rather than (as should be done for AMT, I think) in presenting and commenting the experimental set-up or in demonstrating/assessing the quality and the strong/weak points of the presented system.

Another major point is that the authors did not provide enough information to allow a robust assessment of the system performance and, thus, possible application. I think that the scientific purposes of this set-up should be better addressed. As an instance, I'm wondering if this set-up (based on the NDIR LI-COR Li-840) can be able to meet the data compatibility goal (0.10 ppm) requested by WMO for long-term global CO2 measurements. . .(I'm skeptical about this).

Also the abstract should provide more details/information about the technical set-up and about the results of possible QA/QC test used to assess the quality of the data provided by the proposed set-up. Now, the abstract mostly provides a series of conventional atmospheric data analysis results, which (even if interesting) are not, I guess, the main topic for AMT.

I suggest that the authors can make more use of supplementary material (SM). Too many figures are presented in the manuscript up to now (17!). Some of them (e.g. Figure 2 and others – see my specific comments) can be moved to SM or skipped. The title clearly reflect the contents of the paper, but I would suggest recommend to modify keywords. I would suggest: carbon dioxide, unattended system, low-consumption system, Japan, long-term observations

By the way, several points should be addressed before publication (several of them are also related to assess the "performance" of this new set-up). Moreover, along the paper some inaccurate terms are used (e.g. "precision") and some analyses are too "qualitative" (i.e. lacking of robust statistic).

At some place in the paper, the authors should mention the exact period of operation of the measurements, i.e. "month 2009 – month 2015" (this info is only present in the abstract).

Thus, for summarizing, I think that the paper can be considered for AMT, but only after major revisions. Basically, I suggest to the authors to strength the analysis of the performance of their system, to clearly assess the typical application for which this system is suitable for (i.e. long-term monitoring – if yes which data quality objective can be reached- vs air quality vs event studies) and to consider the comparison with Mauna Loa time series as a "case study" or a "typical application" for the proposed system.

SPECIFIC COMMENTS

Concerning the experimental set-up, I strongly recommend to change the PTEFE with stainless steel or Synflex 1300 tubing (PTFE is well known to be not optimal for CO2 sampling). By which frequency did you change the PTFE tubing? I would like to see more information about the working standards for routinely calibration of the system (i.e. flask material, mixture matrix, pressure regulators) and more discussion about the choice of your calibration strategy (are 4 min sufficient enough to obtain stable measurements?).

Pag 3, line 16. You cannot use this comparison of data for evaluating the precision (i.e. the repeatability) of your measurements! I also have serious concern that you can

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provide an assessment of spatial representativeness by using this data-set...

I think that the authors should spent more work in presenting an assessment of the quality of the system performance. Along the manuscript, the authors provided a statistical analysis (Fig. 5 of the manuscript) of the standard deviation of the measured value of standard gases. This information can be useful to assess the "long-term repeatability" of the measurements (see e.g., Lebegue et al., AMT, 2016). However, to better exploit this point, I would like to see this analysis also split as a function of the different standard gases used and/or as a function of the different years. It should be interesting to see the absolute difference between the CO2 measured values of the standard gases and the reference standard values. Moreover I would like to see information about the uncertainty or expanded uncertainty of the measurements achievable by this system, which is an important parameter missed in the paper.

Concerning the difference with MLO, which may be the role of using two calibration scales (NOAA vs NIES09)? Please quantify... How many often did you re-calibrate the working standard against your National reference (I suppose once per year, but this should be clearly stated in the paper)?

Another important information to provide is the data coverage (%) over the measurement period: I feel this is rather high, but this represent a parameter (you must mention it along the abstract!) to evaluate the success of the presented system.

Pag 6, line 2: " The precision....is calculated to be about 0.3ppm". As recommended by the WMO/GAW glossary, "precision" is a term to be used only in relative terms. How did you calculate this "precision"? If this is a standard deviation, it is the "repeatability" or the "reproducibility". Any information about the "uncertainty" of the measurement?

Pag 7, line 24. For year 2012 it seems that the CO2 variability (error bars) experienced a diurnal behavior (higher variability during night-time): please comments. Does a diurnal cycle exist for internal temperature or other system parameters that can affect the measurements? Figure 8: you did not provide explanation in the Figure caption for the error bars (please, do it). To investigate if statistical significant diurnal variability exist, you should plot for each hour an average mean value with the 95% confidence interval (not simply the standard deviation).

Pag 8, line 6: “. . .a strong wind always blow”. Too generic. Please provide data or references.

Pag 8, line 11: “it was evident that. . .often see in July”. It is difficult from this plot to obtain information about monthly values. I would suggest to replace this plot with monthly mean values and related error bars (representing the associated 95% confidence interval). This would also allow to skip Table 1.

Pag 8, line 24: Since the CO2 is characterized by a long atmospheric lifetime, 72h back-trajectories can explain just a fraction of its variability. Please comment in the paper. It is possible to plot the back-trajectories (as “number concentration field” or as centroids of the detected clusters) over the spatial domain in Figure 11? In any case, consider to move Figure 11 to SM. You should also provide the percentage of occurrence for the different air mass transport class. The discussion about results reported by Figure 12 is too qualitative. For the summer, it seems that a large amount of variability affect the data set. For each class of air mass transport please provide average values and 95% confidence interval. How did you define background air (Pag 8,line 27)?

Pag 9, line 1: In my opinion, a too low number of Pacific Ocean air-masses were observed during winter (at least basing on the visual inspection of Figure 12 since no information about their seasonal occurrences were provided) for provide any kind of comment. “air from China SOMETIMES. . .”. Too qualitative, please be more specific! How many times (frequency) did you observed these spikes with air-mass from China? The sentence from line 9 to 12 is not clear. I think some words were missed somewhere. . .However, I do not think that this paragraph and figure 13 add really important information to the paper, thus they can be skipped.

The comparison with CONTRAILS observations must be better commented (or even skipped). The authors claim for a good agreement but I do not completely agree: a rather large "average" bias (2 ppm) can be deduced by the linear correlation result. In general I do not think that you can use this comparison for "verify" the precision of the measurement system (the term "precision" is not correct at page 10, line 16)! Also concerning the assessment of the representativeness of the measurement site, I think that this analysis can provide only a "qualitative" information (also considering that CONTRAILS observations are not uniformly distributed on the geographical domain). As an instance, the most part of the CONTRAILS observations are recorded not far from Tokyo. Would this introduce a bias on the contrails data-set due to the urban emissions? Can this (at least) partially explain the 2 ppm bias? Moreover, it is not clear what do you mean for "daily data". For CONTRAILS what kind of daily average did you consider? 24h mean values? Or did you select the hourly data on a time window in agreement with Fuji measurements (i.e. 14:100-17:28 and 21:00-00:28)?

Fig. 14 is almost unreadable. Mt. Fuji points completely overlap the "reference" time series of MLO. I'm wondering if it's really necessary to show the whole long term time series of MLO: for the most part of it, you do not have any data to overlap!!! I would suggest to start the x-axis since 2010 and make the MLO point more visible.

Fig. 9, line 17: "The RANGE of the annual rates of increase during 2009-2015 was 1.5-2.7 ppm/yr". Actually, the variability in the growth rate can be also related to other factors, as an instance ENSO (as mentioned in the following by authors) or to change in fluxes between atmosphere and terrestrial biosphere.

Pag 10, discussion of Fig. 15. Some statistical analysis should be provided for the linear correlation (i.e. statistical significance). In general, I think that is dangerous to compare and comment (small) long-term CO2 differences between two measurement sites without providing an assessment of the measurement compatibility...I'm wondering if these tendency in long-term differences are evident also at other measurement sites. In this case, this would reinforce your conclusions. I recommend to consider open-access data-sets available at the World Data Center for Greenhouse Gases by WMO/GAW and to repeat a similar analysis for other sites in the East Asia/Pacific regions vs MLO.

TECHNICALS: Pag 2 line 10: "(CDIAC, Boden et al., 2015)" line 15: "air mass transport" line 23: "Mt Fuji is positioned. . ..of the Asian continent". Please provide references or show back-trajectory analysis. Line 22: "Mt. Fuji is located. . ." Line 27: What do you mean for "irregular"? Please use a more specific terminology.

Pag3, Line 28, point (v): it is difficult to obtain information on the annual rate of increase basing on so "sparse" measurement.

Pag 4 Line 2: "shows a picture. . ." Line 3: "The system consists. . ." Line 16: "In addition, WHEN the temperature. . ."

Pag 8 Line 27:" It was found,. . .IN SUMMER"

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