Response to reviewer #2 Recent six-year atmospheric CO_2 concentration at the summit of Mt. Fuji observed by a battery-powered CO_2 measurement system

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General comments:

1. The manuscript presents a technical description of a CO2 monitoring system operated for 6 years at the high altitude site Mt. Fuji, Japan, and associated data analysis. The description of the measurement setup is rather short for a publication in Atmospheric Measurement Techniques and should be extended (see specific comments below). On the other hand, the data discussion could be shortened as it is not the main scope of a paper in AMT. This would be easily possible as the current manuscript extensively compares the Mt. Fuji data with data from Mauna Loa, Hawaii (differences of daily averages (Fig. 10), differences of daily averages for summer and winter (Fig. 12), trends and growth rates for Mt. Fuji and Mauna Loa (Fig. 13), Mt. Fuji and Mauna Loa time series (Fig. 14), trends of monthly Mt. Fuji-Mauna Loa differences (Fig. 15)). Alternatively, the authors can also envisage to elaborate the data interpretation section, e.g. by also incorporating observations from other (high altitude and/or background) monitoring stations, and can consider submitting the manuscript to Atmospheric Chemistry and Physics. Thus, I recommend the publication of the manuscript in AMT after strengthening the experimental section and concisely abridging the data interpretation. A revision of the manuscript with a stronger focus on the data analysis and a submission to ACP seems to be a suitable alternative, too.

   >We focused on submitting to AMT. So, we added our analytical explanation (calibration information, data handling, standard gases handling and stability, our scale and NOAA scale) to strengthen the experimental section. We also deleted some figures and explanation in the section of results to shorten the text, as suggested.

2. The manuscript contains quite a lot of tables and figures, some of them are to my mind not really needed or can be merged. The authors may consider reducing the number of tables and figures, see also my comments below.

   > We decreased number of figures to six.
   Deleted: Table 2, Figure 2, 6, 14, 15 and 16,
   Merged: Figure 1 and 11, and Figure 3 and 4, and Figure 9 and 13,
   Created: one table and two figure, Move to supplementary material; Table 1).

3. I strongly suggest revising the conclusions which are currently only a summary of what was said before. Please add the lessons-learnt (especially on the instrumental side) – e.g. what would you do differently when you could start from scratch again – and provide an outlook, e.g. are the measurements ongoing? If so, are there any changes/improvements on the measurement setup planned? Did the authors modify the measurement setup during the 6 years of operation, i.e. did they improve their system based on the experience gained in the earlier years?

   >We rewrote the conclusion, according to your suggestion.
   First, we met the difficulty in sending data by ORBCOM satellite. It was so important that we changed the system to Iridium satellite, quickly. We wrote some improvements for measurement and operations such as method for charging 100 batteries. After we installed the system, in 2010 (next year), we developed and installed auto battery charger (Switch for power mode and charge mode). In 2012, we also added the switch box for winter mode and summer mode. For preventing electrical shock from lightning which often happened at the summit we strengthen the earth connection. In 2016, we replaced 50 batteries.

4. Even if it is common to use expressions such as "the CO2 concentration was 400 ppm" colloquially, it is an incorrect statement and not suitable to be used in the scientific literature. Quantities given in ppm refer to mole fractions or mixing ratios and cannot be called concentrations. Please use the correct terminology throughout the manuscript.

   > We tried to change the word "concentration" to mole fraction.

Specific comments:
Title:

5. I suggest changing the title to "Six-years of atmospheric CO2 observations at Mt. Fuji recorded with a battery-powered measurement system"

   > We modified this sentence as you indicated.

Abstract:

6. Lines 15-18: "The difference in monthly average CO2 concentration between Mt. Fuji and MLO appeared to increase from 2009 to 2015. Interannual variability and growth rate of CO2 concentration were similar both at Mt. Fuji and MLO, 13 ppm increase from 2009 to 2015, but the annual average concentration at Mt. Fuji was about 1 ppm higher than at MLO." It sounds like a contradiction (the difference increases but the average was 1 ppm higher), or is at least not understandable without having seen Fig. 15 and the related discussion, respectively. I suggest to delete the first sentence and to add another sentence after the second sentence, like "However, differences in Mt. Fuji and MLO observations show divergent trends depending on seasons."

>We agreed with your advice. But, we decided to shorten the discussion about the trend of CO2 concentration, because our data is still relatively short and it is difficult to clarify the seasonal differences in the trend, which could be influenced by the climatic variation. So, we rewrote only seasonal variation and difference of CO2 concentration by air mass origin about the results of measuring atmospheric CO2 concentration at the summit of Mt. Fuji.

7. Line 18-19: delete "Monthly averaged . . . in April 2013." Not relevant here.

> We deleted this sentence as you indicated.

8. Lines 21 – 22: change ". . . indicating that Mt. Fuji was a representative site . . . in the mid-latitude Asian region" to ". . . indicating that Mt. Fuji is a representative site to monitor CO2 concentrations in the mid-latitude region."

>Thank you for the advice. We rewrote abstract. So unfortunately, this part was deleted.

Introduction:

9. Page 3, lines 8-9: ". . . without electricity supply . . .", better say ". . . without gridded electricity supply . . ."

> We changed this sentence as you indicated.

10. Page 3, lines 8-9: reword sentence: ". . . even under the harsh conditions . . ."

> We changed this sentence. The explanation was added.

11. Page 3, lines 8-9: " . . . harsh conditions found at the summit of Mt. Fuji, where the atmospheric pressure is low . . ." Why is low pressure a harsh condition?

> Low pressure is related to the sensitivity of NDIR. Also it is related to our maintenance work at the summit. We added some explanation about harsh condition in introduction.

12. Page 3, line 16: add latitude, longitude and altitude for Mauna Loa

> We added latitude, longitude and altitude for Mauna Loa.

13. Page 3, line 17: "To evaluate the regional representativeness and precision of the measurements obtained by our system, the data are compared with aircraft observations." Don't you evaluate the accuracy rather than the precision when comparing with other data?

>We agree with your advice. We added another result using bottle sampling data to evaluate the accuracy of our measurement system. Instead, we used comparison with CONTRAIL data to evaluate the regional representativeness of Mt Fuji data. This detail was written in 3.4 comparison with the bottle sampling data.

Methods:

14. This should be the main part of the paper and thus, needs elaboration. Page 3, lines 20-25: add Mount Fuji altitude. How can you access the station? Is access only possible in July and August, or also during the rest of the year (in exceptional cases, e.g. for trouble-shooting).

>We added altitude of Mt. Fuji. It is 3776 m. Also we added explanation about transportation and how to access the station. Actually, we can access only July and August by using the bulldozer with our goods for exchange. The bulldozer is specialized only for the transportation for public maintenance around Mt Fuji. The operation of the bulldozer was done in only July and August because the operation in September to the following June was interrupted due to snowfall. If CO2 observation is interrupted in September to the following June, we have to wait coming summer to fix the system. We have two set of the main measurement system. So, if we have some trouble, we can exchange them during summer time.

15. Page 4, line 6: add number of pumps (4, according to Fig. 3). Why do you need four pumps? Can you redesign the setup using less pumps reducing the power consumption?

>We use four pumps for sucking outside air, sending room air to NDIR line, sending outside air to NDIR line and flowing dry air, respectively. The air pump for sending room air to NDIR and the air pump for sending outside air to NDIR work alternatively. So usually 3 pumps are working.

16. Page 4, line 7: write ". . . using a Nafion membrane . . .".What is the dew point that you achieve with the setup? Does the drying efficiency change (decrease) with time? Is a 2 liter cartridge of Silica gel sufficient for the Nafion counter flow for 10 months.

>We changed the cartridge of Silica gel every year, although over 90% of Silica gel was still blue after one year.

17. Page 4, first paragraph: did you apply any modifications to the measurement setup, in particular to the CO2 analyzer? E.g. disconnecting the display or reducing the flow through the NDIR to reduce power consumption.

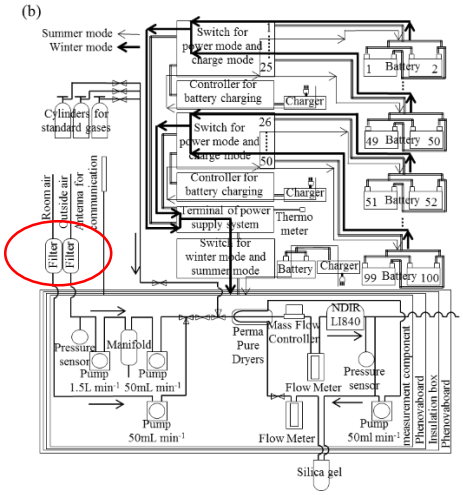
>We did not change the measurement setup in CO_2 analyzer. We connect the PC to CO2 measurement only summer. We did not connect PC from September to the following June. The system is controlled by the control board and operated automatically.

18. Page 4, line 17: "... a small internal heater was planned to activate . . ." was it only planned or was it also in place? If the latter is true, write "... a small internal heater was implemented to activate . . ."

> We changed this sentence as you indicated.

19. Page 4, lines 20-24: did you use any inlet filter? If so, how often was it changed?

> We forgot to add a filter in the figure. Our line has the filter (Swagelok SS-4F-7) which is changed once a year.



20. Paragraph 2.3 Electrical power system: It remains unclear why gridded power is only available in summer. What does exactly change in early July and late August? Is the station permanently staffed in summer? Did you ever try off-grid generated power with wind turbines or solar panels?

> To avoid an ignition accident by an electric short circuit, the commercial power is not supplied to the station from September to the following June because the worker is not permanently stationed there in September to the following June. Installation of equipment like wind turbines or solar panels outside the station was tightly restricted by law because Mt. Fuji including the station is categorized the National park.

21. Paragraph 2.4 Measurement sequence: How is the measurement system controlled? Which software is used? Elaborate on the instrument maintenance. Can you remotely access the measurement setup, i.e. can you access the computer when the satellite communication is running? E.g. to modify the measurement sequence. Did you never face any serious instrumental failures? There seems to be a longer data gap in 2012 (according to Fig. 10)? What happened? What is the overall data coverage based on your daily averages? What is the yearly consumption of reference gas? How long do the reference cylinders last?

> Measurement is controlled by the control board (MC-mini, Kimoto Electric CO., Ltd.). We can remotely access basic setup parameters when the satellite communication is running (e.g. changing start time). However, we did not use these command because satellite communication was not so smooth. We added some explanation about satellite communication in the text.

Our measurement system got the lightning influence in 2012 and 2014. One board was damaged and exchanged. We got CO2 daily data of 2219 days from July 2009 to December 2015 (for 2354 days), which was covered 94% for the observation period. We wrote that on the abstract.

The working standard gases were consumed 300L per year. We replaced the cylinders once every two or three years. We added Table 1 for information of working standard gas, as suggested.

Table 1. The information of working standard gas in observation period

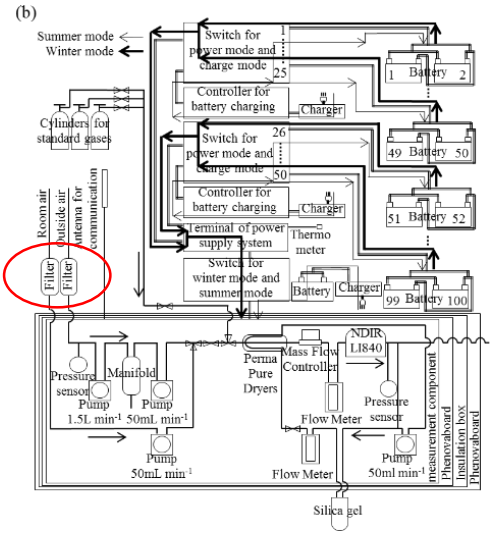
Cylinder No	Before the cylinders installation				After the cylinders replacement				Change of concentration	
	Date of calibration	Calibrated value (ppm)	Pressure of Cylinder (MPa)	Date of installation	Date of replacement	Date of re-calibration	Re-calibrated value (ppm)	Pressure of Cylinder (MPa)	Change amount of the concentration (ppm)	Change rate (ppm $year^{-1}$)
CPC-00449	17-Jun-2009	368.86	10.8	16-Jul-2009	24-Jul-2011	19-Aug-2011	368.82	3.4	-0.03	-0.016
CPC-00447	17-Jun-2009	383.10	11.0	16-Jul-2009	24-Jul-2011	19-Aug-2011	383.24	4.0	0.14	0.065
CPC-00448	17-Jun-2009	403.45	10.8	16-Jul-2009	24-Jul-2011	19-Aug-2011	403.54	3.2	0.09	0.042
CPC-00445	10-Jun-2011	367.94	11.4	25-Jul-2011	25-Jul-2013	6-Aug-2013	368.02	0.8	0.09	0.041
CPC-00450	10-Jun-2011	383.46	11.5	25-Jul-2011	25-Jul-2013	6-Aug-2013	383.42	3.6	-0.04	-0.020
CPC-00451	10-Jun-2011	402.29	11.5	25-Jul-2011	25-Jul-2013	6-Aug-2013	402.37	3.4	0.08	0.036
CPC-00043	23-Jun-2013	367.10	12.8	26-Jul-2013	1-Jul-2016	10-Jul-2016	367.12	2.5	0.02	0.008
CPC-00448	23-Jun-2013	393.17	12.6	26-Jul-2013	1-Jul-2016	10-Jul-2016	393.12	2.5	-0.05	-0.016
CPC-00449	23-Jun-2013	418.59	12.8	26-Jul-2013	1-Jul-2016	10-Jul-2016	418.44	2.5	-0.15	-0.050
CPC-00445	15-Jun-2016	389.18	13.2	2-Jul-2016						
CPC-00450	15-Jun-2016	409.15	13.2	2-Jul-2016						
CPC-00451	15-Jun-2016	429.16	13.2	2-Jul-2016						

22. Page 5, lines 15-18: “However, we subsequently changed the operational time to 21:00–00:28 JST, to avoid local daytime influences from transportation of the air mass around Mt. Fuji that might affect the CO2 concentration over the summit of Mt. Fuji, which is similar to how observations are obtained at MLO.” To my knowledge, CO2 measurements at MLO are continuous and a filter to extract background conditions is applied afterwards. However, most background data are identified at Mauna Loa in the late afternoon, see http://www.esrl.noaa.gov/gmd/ccgg/about/co2_measurements.html for more details.

>Yes. We first though that mid latitude had higher wind velocity than sub-tropical area in Hawaii. Therefore, we took usual daytime for sampling. However, we were afraid that daytime concentration might be influenced by local CO2 fluxes in some cases. After we checked daily variation, we found that daytime sampling looked ok but nighttime seemed better for sampling, as written in the text.

23. Page 5, line 22: did you use an inlet filter when measuring room air?

>We forgot to add an inlet filter of room air in Figure. We added the filter to Figure.



24. Page 5, line 22: ". . . to stabilize the flow line." What does that mean?

> Outside air is sucking by one air pump. It will take some time for the pump to purge the PTFE line from air inlet. During that period, room air is introduced in the NDIR line and also dry air started to circulate to familiarize inside the tube with the fresh air. After that, outside air is introduced and measured.

25. Page 5, lines 23-24: Why do you need to push the air into the analyzer when another pump sits behind?

>This pump is used for sucking air with a higher flow rate (1.5L/min), because we have to purge air in PTFE tube quickly and minimize contamination from the tube. Also, because in winter air inlet may get some moisture frozen, we need a kind of powerful pump to introduced the air from the outside. But other pumps are rather small pump which flow the air by 50 ml/min.

26. Page 5, line 27: why does it need one hour to send the data? How large are the data files? Which time resolution do you store the CO2 data?

>The file size is 192 byte. We added explanation in the text. The communication become difficult under bad weather conditions like the summit covered the cloud. So that we prepared one hour for the data communication. Usually, the communication time is finished just 1 or 2 minutes. But it sometime failed. After the we changed the system from ORBCOM to Iridium, the situation became better. Within 1 hr time window, the system tried to communicate if the system has any data, which could not sent last time, in addition to the new data.

27. Page 5, lines 28-29: "The derived concentration was based on the average of the data from the second, third, and fourth cycles . . .", in other words, the daily average is based on 3 x 8 min = 24 minutes of observations. In fact, it will be even less since you have to discard some data to account for flushing and

signal stabilization after switching from room air to outside air. Add the information how many minutes of data were discarded after switching. Please mention explicitly that the daily averages used below only rely on a very short measurement period.

> We added the explanation about that. The daily average is based on 3 x 6 min data (because we discarded the first 2 minutes data for 8 minutes data) = 18 minutes of observations.".

28. Paragraph 2.5 Continuous measurements in summer: Does the continuous system use the same inlet? Did you use the same reference gases? Does the default system operated all year long also only measure for 3.5 hours a day in summer? The summer system has no dryer. Did you test and quantify the CO2 losses in the Nafion dryer?

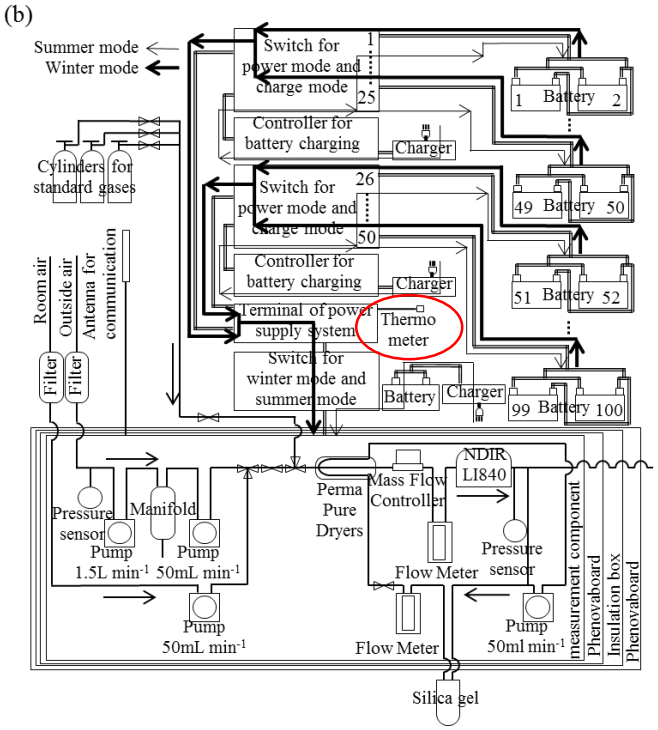
>The continuous system used the same inlet as the battery-powered measurement. The continuous system used other working standard gas (But same scale (NIES09) as the battery-powered measurement). The default system measured CO2 concentration 4 times per day in summer of 2010-2012 to compare bottle sampling measurement. But that, the system worked for 3.5 hours a day.

We checked the measurement of the system without dryer, and found that measurement data matched with our system within 0.3ppm and stability was even better than that. So we only used this system to detect daily variation of CO2 conc at Mt. Fuji.

As for Nafion effect, because we used the same outside air to dry the sample air, the change of CO2 conc must be negligibly small.

29. Paragraph 2.6 Weather data: How does the power supply for the meteorological measurements look like?

>The temperature inside box and room were measured by our system. We forgot to add the sensor of temperature in Figure. We added the sensor to Figure. However, JMA measured outside temperature by their own batteries system. But it must be small size. We added some explanation about it in 2.7 weather data.



30. Page 7, lines 14-19: again, didn't you experience any other interruptions, in particular during the 10 months of unattended operation?

>The interruptions occurred only twice in observation period. It was occurred by lightning (April 2-July 23, 2012 and August 1-18, 2014). We added explanation in 3.2 operation with 100 batteries over 6 years.

31. Paragraph 3.2 and onwards: Use data other than Mauna Loa for comparison and interpretation. E.g. Lulin (Taiwan), Niwot Ridge (USA), Mt. Waliguan (China) etc.; use the marine boundary layer reference (available at http://www.esrl.noaa.gov/gmd/ccgg/mbl/) for comparison which is also available for the Mt. Fuji latitude. How does the difference in latitude (Mt. Fuji – Mauna Loa) influence your comparison?

>Although data from Waliguan is limited, we had some comparison with it. At the lower concentration event in summer, the concentration was comparable with each other. But the data in Waliguan was changed by some reason, at present it is difficult to use it. Mauna Loa showed average of CO2 concentration of mid-latitude in Northern hemisphere. So, it can be used for a typical background CO2. Lulin is located in rather southern part, which is close to our Hateruma station. Hateruma data shows a similar feature to Mt. Fuji in seasonal pattern. To analyze CO2 trend around East Asia, the comparison between Fuji and MLO seemed better.

32. Page 7, line 29: add altitudes for Niwot Ridge and Hakkouda

>We added the altitudes for Niwot Ridge and Hakkoda. Niwot Ridge in the U.S.A. (40.05°N, 105.59°W,

3528 m a.s.l.), and Hakkoda (40.41°N, 140.51°E, 1324 m a.s.l.)

33. Page 8, lines 4 – 8: move this paragraph up to paragraph 2.1

> We moved this paragraph to 2.1.

34. Page 8, line 13: say “18 ppm larger”, add amplitudes for Mt. Fuji and MLO.

>We added amplitude for MLO. It is 8 ppm.

35. Page 8, line 16: Table 1 is not needed, in particular if data are available in a publicly accessible data repository. Did you submit the data to the World Data Centre for Greenhouse Gases?

>We did not submit our CO_2 data to WDCGG yet. But we will release the data on our website and submit to WDCGG after this paper is accepted to the journal. We moved Table 1 to supplementary material.

36. Page 8, lines 22 – 23: “Conversely, the CO2 concentration from December to March at Mt. Fuji was generally higher than at MLO.” February and March are the moths with most intense biomass burning on the Indochinese Peninsula. Do these fires affect the observations at Mt. Fuji?

> We think that the CO2 data of Mt. Fuji is also affected by biomass burning on the Indochinese Peninsula. We added such possibility in the text.

37. Page 9, line 5: add reference to Fig. 1

>We added the reference of (Watanabe et al., 2000)

38. Page 9, lines 12-13: this statement is based on the Fourier-transformed (i.e deseasonalized) data, correct?

>Yes

39. Page 9, lines 20- 21: “. . .the increased rate of growth of CO2 concentration because of accelerated plant respiration over land and weakened photosynthesis activity”. Add reference. Next to vegetation effects, it is also due to more intense biomass burning, see e.g. Betts et al., Nature Climate Change, September 2016).

>Thank you for giving information. We deleted this paragraph, but added about biomass effect at the section of seasonal variation.

40. Page 9, lines 24-26: “For example, the negative values of ΔCO2 concentration have enlarged gradually over the six–year period of 2009–2014, as shown in Fig. 10. Chen et al. (2014) reported that growth of Asian vegetation increased recently.” I doubt that such an effect can be seen in a 6-year time series. Moreover, Chen et al. refer to changes over the last three decades.

>We deleted this paragraph because we have only six-year data of Mt. Fuji. It is difficult for discussing the trend of CO2 concentration.

41. Page 9, lines 26-28: “In particular, remote sensing observations over eastern Siberia have revealed a notable increase in vegetation during recent decades. Correspondingly, the negative values of ΔCO2 concentration have enhanced gradually.” Is there any proof confirming this statement. How about CO2 observations at other Asian sites? Can’t it also be an emission effect with decreasing emissions in summer?

>Yonaguni site in Japan near Taiwan is also decreasing ΔCO2 concentration in summer. But we quit to discuss this phenomenon on this paper because we have only six-year data of Mt. Fuji. Also in our observing period, we admitted some annual variation in trajectory sector fraction from Asian Continent in summer, suggesting that the variation would give more influence to such events.

42. Page 9, lines 28-29 and page 10, lines 6-7: “This phenomenon might be related to the increase of anthropogenic CO2 emissions in China during recent decades” and“. . .the positive trend in winter was not so significant, which might be attributable to the slowing of the growth rate of CO2 emissions in China during 2011–2014.” Sounds like a contradiction.

>We deleted this paragraph. The increase of anthropogenic CO2 emissions in China increased until 2010. But recently (2010-2015) the increase rate is decreasing.

43. Page 9, lines 20-32: “Such event-based phenomena should be evaluated by numerical simulation, but we expected that such signals from regional emissions or absorption changes would be included in the data at Mt. Fuji.” If you expected it, why didn’t you look closer at it?

> We deleted this paragraph. But we looked at it a little closer using trajectory frequency.

Conclusions:

44. As mentioned above, revise the conclusions and add lessons-learnt and an outlook. Tables and Figures: Table 1: not needed, a release of the data in a public data repository is strongly encouraged.

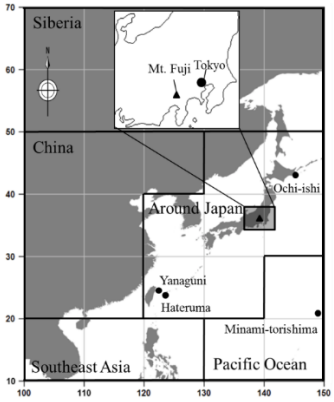
>We revise the conclusion. We moved Table 1 to supplementary material and we will release our CO2 data by our DOI system later.

45. Table 2: not needed, some numbers could be incorporated in Fig. 15.

>We deleted Table 2

46. Fig. 1: add areas of air mass origin (Fig. 11) to Fig. 1 and delete Fig. 11.

>We merged Fig 11 with 1.



47. Fig. 5: what is the nominal value of the standard gas?

> We wrote those values in Table 1.

Table 1. The information of working standard gas in observation period

Cylinder No	Before the cylinders installation				After the cylinders replacement				Change of concentration	
	Date of calibration	Calibrated value (ppm)	Pressure of Cylinder (MPa)	Date of installation	Date of replacement	Date of re-calibration	Re-calibrated value (ppm)	Pressure of Cylinder (MPa)	Change amount of the concentration (ppm)	Change rate (ppm year^{-1})
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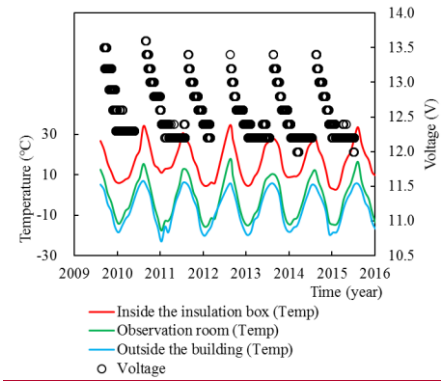
CPC-00450	10-Jun-2011	383.46	11.5	25-Jul-2011	25-Jul-2013	6-Aug-2013	383.42	3.6	-0.04	-0.020
CPC-00451	10-Jun-2011	402.29	11.5	25-Jul-2011	25-Jul-2013	6-Aug-2013	402.37	3.4	0.08	0.036
CPC-00043	23-Jun-2013	367.10	12.8	26-Jul-2013	1-Jul-2016	10-Jul-2016	367.12	2.5	0.02	0.008
CPC-00448	23-Jun-2013	393.17	12.6	26-Jul-2013	1-Jul-2016	10-Jul-2016	393.12	2.5	-0.05	-0.016
CPC-00449	23-Jun-2013	418.59	12.8	26-Jul-2013	1-Jul-2016	10-Jul-2016	418.44	2.5	-0.15	-0.050
CPC-00445	15-Jun-2016	389.18	13.2	2-Jul-2016						
CPC-00450	15-Jun-2016	409.15	13.2	2-Jul-2016						
CPC-00451	15-Jun-2016	429.16	13.2	2-Jul-2016						

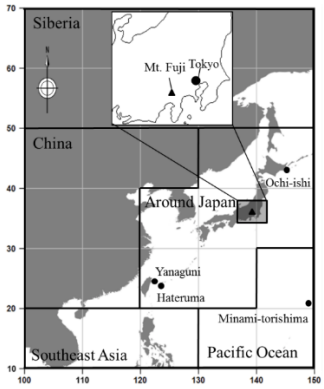
48. Fig. 6, caption: Write “Monthly averages of ambient temperatures outside . . ..”

> We deleted the figure of monthly averages of temperatures and created the figure of daily data of temperatures. Also merged temperatures data and voltage data.



49. Fig. 11: merge with Fig. 1 and delete

>We merged with Fig 11 and 1.



50. Fig. 14: does it show daily averages? Monthly averages? The figure repeats Fig. 9, the long-term evolution is not of real interest here. I suggest to delete it.

>We deleted Fig. 14

51. Fig. 15: only a few trend lines are plotted: for which months? Use open symbols for the months without trend line?

>We deleted Fig. 15 to concentrate the explanation of the information of working standard gas and method of measurement.

52. Fig. 16: add information to Fig. 1 and delete Fig. 16

>We deleted Fig. 16

Minor comments:

53. Page 2, line 11: start with lowercase letter after semicolon.

>We modified this sentence as you indicated.

54. Page 5, line 12: typo “Phenobaboad”

>We modified this sentence as you indicated.