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Atmospheric Chemistry and Physics Discussions

Interactive comment on "In-situ measurement of atmospheric CO_2 at the four WMO/GAW stations in China" *by* S. X. Fang et al.

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Thanks very much for the reviewer for the comments on this manuscript. The following are replies to the reviewer2.

1. A larger part of the paper is dedicated to the analysis/source apportionment of the measured CO2 – parts of this exercise are in my opinion too subjective and should be rewritten or shortened accordingly. Also, I would like to stress that local surface wind should be used with caution for interpretation of source areas of the measured air masses - particularly in complex terrain, as is the case for the stations described in this manuscript, the errors in the interpretation may be considerable. Including other gas species measured at the sites (e.g. CO, CH4, etc.) and atmospheric transport models

in the interpretation, allows a better constrained analysis.

Reply: Yes. Thanks for the suggestion. At the three regional stations, due to the complex terrain, there might be bias when using the local surface wind to identify the local sources and sinks. In the revised manuscript, we erased the argument of surface wind effect on the observed CO2 concentration. Instead, we flagged the CO2 data series on surface wind direction where very adjacent sources are located (less than 10 km) such as villages, factories, train, etc. To do this, we exclude the data that is obviously affected by very local sources. The difference in the new approach is that we first look at the map and identify the potential local sources. We still used three steps to filter the data. 1) To select the data during daytime hours, 2) To flag the CO2 concentrations under certain wind directions where very local sources exist (We identified potential local sources in the following sectors: SSW-SW and N sector for LAN, N for LFS, and S-SSW-. . .-NNW for SDZ.), 3) To flag the rest CO2 data series when surface winds speed is below 1.5 m s-1. The rest of the data is considered to be "regional" representative. However, it is still hard to "exactly" distinguish between regional and local CO2 mole fractions at those stations because there are so many factors affecting the concentration including sources, sinks, terrain, meteorological conditions, long-distance transport etc. It is a good suggestion to use tracer analysis (e.g. carbon monoxide or black carbon) to identify the anthropogenic sources. Regrettably, we do not have the record of CO or BC during the same period. Furthermore, this would add so much content into the manuscript and make the paper to be boringly long. Actually, we plan to prepare additional manuscript to study the effect of transport on GHGs data as well as to identify local regional sources for each station.

The WMO/GAW global station WLG is very remote and its surrounding topography is rather gentle. The surface wind is proved to be suitable for identify the background CO2 information (Zhou et al., 2004). In the revised manuscript, we didn't change the data selection policy. We realized that some statements are too subjective and ambiguous in the original manuscript, in particular in the section of surface wind and its influence

on the CO2 mole fractions. We polished the sentences and re-wrote them, including the section of “Yearly growth rate”. Please refer to the revised draft for more.

2.In this context, Fang et al. 2013 (full citation in this manuscript) has in many aspects a structure that is very similar to identical to the one of this manuscript – partly with additional analysis included (e.g. cluster analysis of backward trajectories in summertime) – which makes it a sort of a “companion paper” to this manuscript (even if the Shangdianzi site is not included in Fang et al. 2013). This is not, but should be, stated in this manuscript. Furthermore, several sections contain text identical or very similar to that in Fang et al. 2013. This is to certain extent understandable for a technical/descriptive part, where the same measurement systems and sites are described, but less in other sections. Please check and make sure that Fang et al. 2013 is referred to in all places where this is applicable and necessary. Some rewriting might be unavoidable.

Reply: Agreed. In the revised draft, firstly, in the “Introduction” section, we inform that the in-situ systems at our stations are measuring CO2 and CH4 at the same time. Based upon the results from 2009 to 2011, we just published a paper on JGR. With similar ideal, we prepared this manuscript. In these two papers, we present the results of CO2/CH4 observed by in-situ systems in Chinese regional stations for the first time. Moreover, using similar regime to CH4 (but the details differ), we filtered the data into “regional” representative to identify the CO2 mole fractions in a larger area. This would make the two papers partly unavoidable similar. We added some sentences to state that in the manuscript.

For example, at the end of section “Introduction”, we added content as: “. . ., initially, those were automatic weather stations only, and in-situ CO2/CH4 measurement systems began in 2009 using CRDS instruments (G1301). In the previous study, we presented and analyzed the first three years CH4 mole fractions observed at LAN, LFS and WLG (Fang et al., 2013). Similarly, here we study the observed CO2 mole fractions at the four stations.”

Secondly, we updated part of the manuscript. For example, we shortened the section of “influence of local surface wind” and analyzed the potential effect of the very local sources. Furthermore, we re-wrote the “Long-term trend” section. At WLG, we merged with the historical CO2 record from 1997, studied the long-term trend and annual growth rate, and compared with other sites. All of those will make the current manuscript more different than before.

Finally, as you suggested, we referred the JGR paper in places where it is necessary. Please refer to the revised manuscript.

3.Otherwise, the paper is well structured, but has room for improving its writing style (mostly in the second half of the text), including punctuation, spelling and consistency of formats and symbols throughout text (e.g. CO2 data, ratios, values, CO2 mole fractions – and, it should be stated at least once in the text that you are actually referring to “atmospheric CO2 dry air mole ratios”). I have marked several sections that should be improved, but the list is not exhaustive.

Reply: Yes, we improved the writing style in the manuscript and tried to avoid symbols such as CO2 data, ratios, values etc. We added a sentence “The CO2 mole fractions in this study are atmospheric CO2 dry air mole ratios.” at the end of section 2.

4. In their reply to the anonymous referee 1, the authors state that an updated manuscript is available. Please note that at the moment of writing this review, it was not available to me (nor did I ïňĄnd it at submission of this review). In my opinion, the paper is suitable for publication in ACP, but only if the comments have been addressed properly.

Reply: Sorry. There was no way for me to upload the revised manuscript when I replied to the referee 1. In this time, we have updated the manuscript upon both referees’ comments and it is ok to upload the latest version now.

SpeciïňĄc comments and technical corrections

5. Page 27288, Line 3 (27288/3): “at 4 atmospheric” – write out 4 “at four atmospheric” and keep this consistent throughout the text

Reply: Agreed.

6. 27288/3-4: to many parentheses in “((Lin’an, LAN), (Longfengshan, LFS), (Shangdianzi, SDZ), and (Waliguan, WLG))

Reply: Agreed. We re-wrote the sentence as: “... The stations are Lin’an (LAN), Longfengshan (LFS), Shangdianzi (SDZ), and Waliguan (WLG), which are regional (LAN, LFS, SDZ) or global (WLG) measurement stations...”

7.27288/4-5: “Cavity Ring Down Spectroscopy” to “Cavity ring-down spectroscopy” Reply: Agreed.

8.27288/8: change “in China’s most economically developed region” to “in China’s economically most developed region” Reply: Agreed.

9. 27288/9: “the northern east of China” to “northeastern China” and check/correct usage and spelling other where in the text

Reply: Agreed.

10. 27288/18: delete “thorough” in “a thorough characterization”

Reply: Agreed.

11.277289/6: “yr” to “years”

Reply: Agreed.

12. 277289/7-8: So far, . . .worldwide. . . Reply: Agreed.

13. 277289/23: delete “Nevertheless,” Reply: Agreed.

14. 277289/26: delete “However,”

Reply: Agreed.

15. 27290/10-11: replace [ ] parentheses with ( ) parentheses

Reply: Agreed.

16. 27290/14-20: the brief history of the stations could be described better (c.f. the information that is accessible at http://gaw.empa.ch/gawsis/ ; when did the different stations get WMO status, what else is/was measured, etc.). Although the acronym GHG is well known, every acronym used in the text must be written out at least once (when ïňĄrst used).

Reply: Agreed.

17. 27290/24: “Yangzi” or “Yangtze” as other where in the text – correct as needed 2.1 Sampling sites: in addition to Fig. 1, refer also to Table 1

Reply: Agreed.

18. 27291/14: 0.6 km in which direction relatively to the station is the railway? Reply: Agreed. We revised the sentence as “...A railway used by diesel-driven trains runs from southwest to northeast...)

19. 27291/19: replace “above the ground” with “a.g.l.”

Reply: Agreed.

20. 27292/10: all four systems were installed precisely on 1. January 2009? Correct “in 1 January” to “on 1 January”

Replay: Actually, the systems were installed and tested in Dec, 2008 (not installed on the same date). According to CMA’s schedule, they all started service officially on 1 Jan, 2009. We re-wrote the sentence as “the individual systems started service on January 1st, 2009...”

21. 2.3 Calibration. . .. I would not refer to the Target gas as “standard” as it is used as “unknown sample” during the measurement in order to check the quality of the

calibration done with the help of the “standards”. In Fang et al. 2013 you write: “The two standards and the target cylinder are analyzed by the system every 12 h.”, which is in contrast to what you write here (“The three standards are analyzed by the system for 5min every 6 h.”). As it is the same systems in the same time period – please clarify.

Replay: Sorry, we made a mistake on interpreting the calibration. We double checked the running sequence at our stations and the Target (T) is indeed measured every 12 hrs. We corrected this now. As you said, the Target is treated as “unknown sample” in our measurements. Both T and air sample run through the same gas line after the valve box (including multi-position valve, mass flow controller etc.). The only difference is that the air sample is from the gas inlet outdoor while the T is from cylinder.

22. From 5 min measurements, you take last 3 min – it is therefore not appropriate to state that 5 min averages are recorded. You mention 97

Replay: We corrected this and added one sentence “. . .The average of the last 3 minutes is used to represent the CO2 concentration in current 5-minute segment . . .”. We mention 97

23. 27294/9: replace “were” with “are” Reply: Agreed.

27295/7-8: I presume used data is for all three years? Please state this clearly. Add “,respectively.” at the end of the sentence.

Reply: Agreed.

24. 27296/18-19: what exactly do you mean by “. . .the standard deviations of the hourly averages are so large that the daily amplitudes could not be calculated.”

Reply: What I mean is that the uncertainty of the hourly average is large in summer and winter. As a result, the peak-to-peak diurnal amplitude could not be calculated (the uncertainty is larger than the discrepancy between peak and valley) in the diurnal variations.

25. 27296/25-26: last sentence could be deleted.

Reply: Agreed.

26. 3.3 Comparison. . . This section should be rewritten (lines 2-5) to improve clarity. In addition: “local sources” add “and sinks”, decide on “night” or “early morning” (or clarify which is for which station. Values in Line 15 should be negative (c.f. Fig. 4). Line 20: the bias you mention is relative to 50 and 80 m, not to the well mixed air above the boundary layer.

Reply: Agreed. Values in Line 15 means data less than 0.2 ± 0.2 ppm in the period from10 to 16 LT at LAN and 0.3 ± 0.2 ppm from 09 to 16 LT at LFS. Although in the midday the CO2 mole fractions at 10 m are lower than top level (due to the uptake by the plant canopy) and the difference is negative. At the two ends of the daytime period, CO2 mole fractions at 10 are higher than the top level.

27. 3.4 Impact. . .. The title of this section is not explicit – impact on what. Please elaborate or replace “impact”.

Reply: Agreed.

28. 27299/8: correct order of words “. . . that mole fractions of most compounds. . ..”

Reply: We re-wrote this section.

29. 27299/15: delete “Obviously,”

Reply: We re-wrote this section.

30. 27299/28: Replace “absorption” with “uptake”

Reply: Agreed.

31. 27300: please explain why it is necessary or advantageous to use the Beaufort scale.

Reply: We want to find the correlation between CO2 concentrations and surface wind

speeds at the four stations, which will give us a clue for the effect of local sources and sinks. The Beaufort wind scale is widely used. We may also use the digital wind speed. However, this may make the sentence hard to read.

32. 27300/18-20: I am not sure to understand this correctly. When looking at Fig. 8, this statement seems wrong (e.g. in Spring, sectors with higher wind speeds as NE and W can have both high (NE) and low (W) CO2 concentrations, etc.). Please clarify.

Reply: When calculate the average CO2 concentration, we do not consider the wind directions. That means all the CO2 mole fractions under certain wind speeds (no matter what is the wind direction) will be averaged. DifferentlyïijŇin Fig. 8, the average CO2 concentration and wind speed on every single direction of 16 sectors are calculated. To avoid misleading, we erased wind speed in Fig. 8.

33. 27301/16-19: Here you could refer to Fig. 4 – and mention in its caption that SDZ is not shown (and the reason for not showing it). From Fig. 4, it seems to me that it would be better to take 10-16 and not 9-17 LT for the "regionally representative" (values close to 0).

Reply: Thanks very much for your correction. Actually, we did take the data from 10 – 16 as discussed in section 3.3.

34. 3.6 Long-term trends As you note in 4. Conclusions, "The relatively short 3 yr record may introduce bias in our analysis of the seasonal variations and estimates of trends.". For this same reason, I do not like the title of section 3.6 – you can rather discuss short-term trends here. But what you could do is take into account also the earlier WLG CO2 data from 1994 on. An almost 20 years-long record would qualify for long-term trend analysis much better.

Reply: Thanks for your suggestion. I changed the title of section 3.6 to "Yearly growth rate". At WLG station, we take into account the historical records (from 1997 to present) and discussed the long-term trend as well as the yearly growth rate. Please refer to

the revised manuscript.

35. 27304/6-8: from Fig. 9, it looks like it is Dec. (not Jan.) for LFS and LAN, and May (not Apr.) for WLG. Please clarify.

Reply: Agreed, thanks for the correction.

36. 27305: this page would beneïňĄt from a partial rewrite Reply: Agreed and we have done it as your suggestion.

37. Table 1.: Correct format/alignments to improve legibility. Correct "Ecnomic".

Reply: Agreed.

38. Table 3.: I suggest adding an * to the column name "Station" (in the footer: * see text (and http://gaw.empa.ch/gawsis/ ?) for station name acronyms and other information).

Reply: Agreed.

39. Fig. 1: scale should be in kilometers. Add in text: ". . . four Chinese WMO. . .".

Reply: Agreed. Thanks for the suggestion.

40. Fig. 4: Label on y-axis – suggest to put it same as on other ïňĄgures ("CO2 mole fraction difference (ppm)")

Reply: Agreed.

41. Fig. 6: put wind speed line on the top layer (is currently covered N-NE section of the graph).

Reply: Agreed. We re-wrote the wind-rose analysis section and erased the original Fig. 6.

42. Figs. 5-8: usage of "concentration" instead of "value" in the legends would be better. Do you really need parentheses around the names of the seasons? When reading carefully the text and looking at the ïňĄgures, there are some inconsistencies.

Only as one example, I ïňĄnd it difïňĄcult to understand how in Fig. 7 that in summer from the WSW direction (direction of Beijing), with relatively high wind speed, the CO2 is less enhanced (and the sector is labeled as “regionally representative”), while the opposite is the case for the other seasons (labeled as “local sources” at comparably elevated wind speeds)? SDZ is located 150 km NE from Beijing – anything that reaches SDZ from Beijing would therefore have a “regional” character.

Reply: Agreed. As answered in the general comments, we re-wrote the section 3.5. We realized that bias may be induced when using surface wind to track the local sources and sinks at the three regional stations. We erased the argument of local surface wind (also Fig. 5-7) and briefly state the locations of very local sources for the LAN, LFS and SDZ. The rest of data are further flagged and consider as regionally representative. At WLG, as it is a WMO/GAW global station and the topography is very even around, we are still using the surface wind to identify the effect of local/regional sources. Please refer to reply to the first comments.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 27287, 2013.