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Interactive comment on “Analysis of accurate ^{13}C and ^{18}O isotope measurements of CO_2 in CARIBIC aircraft air samples from the tropical troposphere, and the upper troposphere/lowermost stratosphere” by S. S. Assonov et al.

Anonymous Referee #1

Received and published: 22 April 2010

General Comments and Ratings:

Overall, this is a good paper that achieves the goal of describing in good detail the high quality measurement of CO_2 mixing and isotope ratio measurements made during the CARABIC project flights. I fully endorse the basic premise of detrending the observations to a common time and think the authors have done a good job with that. Although there is a reasonable amount of data analysis, it seems to me rather unconnected. The reader is left to wonder what the significance of these high quality measurements is.

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In particular, the transition between the tracer-tracer analysis of stratospheric air masses using N₂O and the analysis of $\delta^{13}\text{C}$ using Keeling plots is quite abrupt. The use of CO₂ and its carbon and oxygen stable isotopes for studying the global carbon cycle (as framed in the introduction of the paper, e.g.) is not discussed that much. I get the impression that there was no specific scientific plan for the CO₂ and stable isotope measurements when CARIBIC was conceived, and this paper reflects that. Along these lines, I believe that the paper's title, abstract and introduction should be consistent with the relative lack of carbon cycle analysis. The introduction and abstract should be clear as to what the paper does not deliver – i.e. an analysis of the data focused on the carbon cycle. Only sections 4.24 and 4.25 in the discussion deal with the carbon cycle, and in general, these sections are smaller and weaker than those dealing with UT/LS correlations and mixing. The ultimate result is to make the paper quite disjointed.

However, one point that could be made more strongly in the paper, and to me the most interesting result, is that even at high altitudes, the relatively small amount of CO₂ variability seems to be largely explained by fossil and biological uptake/release as evidenced by the Keeling plot intercepts. This is happening despite the fact that, at an annual mean level, ocean uptake is of the same order as terrestrial uptake, about 2 billion tons C per year, globally.

Another issue with the paper is that it is quite long. There are certainly opportunities to shorten. E.g., there is significant repetition in the text on the points of the data being of high quality and the (from the authors's point of view) the data agreeing with the NOAA network data. Perhaps just one figure could summarize the agreement between the NOAA and CARIBIC data. The paper certainly presents a lot of interesting data and analysis, but there is no coherent 'story' or message running through it, which makes the paper hard to read. I am not sure how to fix this, but I think the paper would be greatly improved if the authors made an effort to explain the connection (is there one?) between the UT/LS mixing analysis and the carbon cycle analysis that follows in the

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text.

I rate the paper as follows (according to the ACP guidelines): 1. Scientific Significance: Good (2) 2. Scientific Quality: Good (2) 3. Presentation Quality: Good (2)

Specific Comments:

1. p.6000 l.28: the claim that this data set is 'important' may be true, but this statement is not backed up in the text.

2. Introduction. As mentioned above, the focus in the introduction on the carbon cycle is a bit misleading, because the paper as a whole is only partially about the carbon cycle. At the very least, this discussion should be abbreviated (the first four paragraphs could be condensed to one or two), and it should be explicitly stated that the nature of the measurements (near the tropopause) do not lend themselves to information on surface-atmosphere fluxes.

3. p6001 l. 16 Replace 'Carbon America' with 'North American Carbon'.

4. l. 23. Some references should be given for stable isotope measurements.

5. p 6003 l. 25. I was not able to find the brief discussion of the future use of the data in models.

6. p 6006 l. 8. The equation should start with two 'deltas' upper-case, followed by lower-case, because this is a difference (thus upper case delta) of isotopic (lower case) delta values.

7. Observational data: For a paper like this, where one of the main goals is to present the data, a clear reference for the data location must be given. It appears that at least some of the data is housed at the World data center for climate. (why not the World data center for greenhouse gases?)

8. p 6009 l19- 26. This entire paragraph is confusing to me and needs to be re-written. First, what is the consistency test that is applied to the CO₂ and $\delta^{13}\text{C}$ trends? Second,

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isn't the $\delta^{13}\text{C}$ 'decrease rate' the same as the 'trend'? Why does one need to combine the trend with the CO_2 increase rate? Also it is not clear what the y-intercepts here refer to. What exactly is being plotted?

9. Section 3.3. How are FT and UT distinguished. Could you generalize and just use 'troposphere'? Please explain and/or cite references.

10. p6012 l12. Should the first instance of 'N₂O' be 'CO'?

11. p6016 l26. Is the line fit in Fig. 7 forced through a point where $\text{N}_2\text{O} = 150$ ppb? If so, the corresponding d^{18}O value can not be 0.5 per mil as the text seems to imply. Please explain. Also, what were the bases for choosing 150 ppb and +0.5 per mil as stratospheric 'starting points'?

12. p6021 l17. The reference CONTRAIL is impossible for a reader to follow and find the source.

13. p6022 l5. Should be 'qualitatively'

14. p6023 l25. The last sentence is unnecessarily vague and is in conflict with p 6024 l 12-13. As mentioned in the general comments, more can be said of the rather remarkable similarity of the Keeling plots, between Caribic 1 and 2 and even to a lesser degree between seasons.

15. p6024 l 11-13. The 0.1 per mil range can be interpreted (more accurately, perhaps) as an indicator of rather strong atmospheric mixing of different source and sink processes since time of 'emission', as opposed to simply the 'large scale similarities' of the source and sink processes. E.g., we know that ocean uptake results in a much different isotopic signature than terrestrial uptake.

16. l 27. As mentioned above the value of -14 per mil is not explained clearly.

17. p6025 l7-8. This similarity is very qualitative. What are the actual values?

18. p6027 l 10. Change to 'help in understanding'

19. | 21. How was it known that the flights ‘crossed large plumes’. Please explain or give a reference.

20. p6030 | 8. The expert group recommendations citation is very awkward.

21. Figure 9 is quite cluttered. Perhaps remove the symbols from the NOAA stations.

22. Figure 10 upper panel. Plot the regression line and print the slope, intercept and r^2 .

23. Figure 14. Remove the box-whisker legend from the plot and explain in the caption, instead.

Technical comments:

1. Throughout the paper, the formatting of citations is often incorrect. E.g. ‘... paper by Callendar (Calendar, 1938)...’ should be ‘...Calendar (1938)...’

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 5999, 2010.

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