

No increase is detected and modelled for the seasonal cycle amplitude of  $\delta^{13}\text{C}$  of atmospheric carbon dioxide

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## I. General

This paper showed the result of stable isotope ( $\delta^{13}\text{C}$ ) in  $\text{CO}_2$  and  $\text{CO}_2$  seasonal variation and its amplitude changes for a long-term period through observation and model investigations. The model and observation showed no significant changes of seasonal amplitude of  $\delta^{13}\text{C}$  while those values are increasing of atmospheric  $\text{CO}_2$ . Authors tried to understand why they showed different characteristics using possible tools and explain it. This is interesting and valued paper to understand carbon cycle and to lead readers why we monitor not only atmospheric  $\text{CO}_2$  but also  $\delta^{13}\text{C}$ . However, for readers, it is hard to say this is well written so that a revision is necessary before publishing. The key revision is for clarification.

1. Some of explanations should be included to methods part rather than result or discussion section. Those explanations can make readers not focus on the main result.
2. There are many abbreviations without full names in the manuscript.
3. Once authors defined a term, please keep the defined term in whole manuscript (e.g., such as  $C_a$  and  $\text{SA}(C_a)$ ).
4. It might be good to reconsider whether the title is representative of whole manuscript. Authors would like to emphasize no significant changes of  $\delta^{13}\text{C}$  seasonal amplitude; however, did not mention about period (a decade or -100 years?), scale (global or Northern Hemisphere or Europe?) and the tool (only model or both of model and observation?). Also the no significant  $\delta^{13}\text{C}$  seasonal amplitude trend can be a trigger to investigate this experiment though, I wonder it can be a title of the manuscript.
5. It seems like very vague of function of result and discussion section. Normally when authors divide into two sections, result section should include only the experimental result which are from suggested method section and explain reasons why experiment show the result in discussion section. In this manuscript, even in result section, the reasons of experimental result were partly discussed which seems like very similar function of discussion section. And also, to discuss the result, in discussion section summary of results was suggested once again. This makes the manuscript very long and not clear. Hope authors reconsider the structure of manuscript and find out effective way to deliver what this paper really would like to say. One of ways is to combine two sections. I would like to suggest good example with good structure, Piao et al.,2018. Please consider the structure of the manuscript.

## II. Specific

1. Title can be reconsidered.
2. L5: Though the authors have done an experiment on global scale, the word “to simulate **local** atmospheric  $\delta^{13}\text{C}(\text{CO}_2)$ ” can make readers misunderstand that this result can have a bias from scale differences. It would be good to mention just “atmospheric  $\delta^{13}\text{C}(\text{CO}_2)$ ” minus local or put more appropriate word in place of “local” (for example global background stations?)
3. L24: It might be wonder  $\delta^{13}\text{C}$  is same to  $\delta^{13}\text{C}_a$  in Line 20. If it is same, please revise and unify all terms in the manuscript. e.g. L27 as well.
4. L25: Recommend that authors can suggest definitions of  $\delta^{13}\text{C}$  before mentioning  $^{13}\text{C}$  and  $^{12}\text{C}$ , especially if  $\delta^{13}\text{C}$  was used differently from  $\delta^{13}\text{C}_a$  (for example:  $^{13}\text{C}/^{12}\text{C}$ , normally expressed relative to a standard as  $\delta^{13}\text{C}(\text{CO}_2)$  in units of per mille (‰)).
5. L78: 19 sites represent of global levels? To avoid the question of error of this experiment, the reason to choose 19 sites can be discussed somewhere in section 2.3 and here explicitly mention as 19 global sites.
6. L82: This sentence is not suitable for introduction. Introduction is not abstract. This is one of results in this manuscript. Hope it can be moved to another section or removed.

*We demonstrate for the first time that the observations at the globally distributed sites show no significant trends in the seasonal cycle amplitude of  $\delta^{13}\text{C}_a$ , consistent with our model chain, but surprising in view of the large trend in the seasonal amplitude of  $\text{CO}_2$ .*

7. Section 2.1. There are too many abbreviations without full name. L92 (EMIC Bern3D-LPX), L93(Bern3D), L94(LPX), L98(DIC), L18 (LUH2 and NMIP), L120 (NCEP/NCAR), L121 (CRU-TS4.05).
8. L123: agents to species.
9. L123: the specific definition of  $E_{\text{control}}$ .
10. L125: Atmospheric  $\text{CO}_2$  to Ca,  $\delta^{13}\text{C}$  to  $\delta^{13}\text{C}_a$ .
11. L127: what does TM3 stand for?
12. L129 and L130: Just write Ca and  $\delta^{13}\text{C}_a$  because they were defined already in previous section.
13. L135:  $^{13}\text{C}$  or  $\delta^{13}\text{C}_a$  ? Those confusions occurred all manuscript.
14. L140: Ca and  $\delta^{13}\text{C}_a$ ? If not, I think those are also redefined as similar format. For example,  $\delta^{13}\text{C}_o$  (ocean) or  $\delta^{13}\text{C}_o$  (observation) etc. This is similar to the L142 and 143.
15. L152 to 153: The authors should mention the URL with last access date from Cooperative Global Atmospheric Data Integration Project and Scripps  $\text{CO}_2$  program. I highly recommend adding that information. This is very important part.
16. Section 2.3: Authors should explain the reason why 19 sites were selected for the experiment and their measurement uncertainty. Also NOAA data and Scripps data have different scale and there is a bias between two data derived from two scales (Lueker et al., 2020)

17. L160: If authors did not use KER and NZD data, it would be good to not discuss here to avoid confusions.
18. L162: Hope you can keep the same term all over the manuscript, for Ca and  $\delta^{13}\text{Ca}$ .
19. L166:  $^{13}\text{C}$  to  $\delta^{13}\text{Ca}$ ?
20. L191: Background  $\text{CO}_2$  mixing ratio is different from observed  $\text{CO}_2$  mixing ratio. This is important part because just observation  $\text{CO}_2$  include local signals but background  $\text{CO}_2$  is selected representative values from all observation data. Authors should discuss this in data section 2.3 as indicating which data were used for the experiment. Also please do not use  $\text{CO}_2$  mixing ratio in place of Ca. If authors defined Ca, please keep the term. Also the unit of data that are used for this paper are not mixing ratio, that is mole fraction (Green book, 2007, Note. Official name is not green book but normally use as green book.)
21. L206: The model..., there are many models in the method section. It would be good to indicate explicitly what model is. For L212 The land biosphere model and L216 The ocean model should be explained for what kind of models were used, as well. Also, it would be good to match all experiment results with 2. Method section.
22. L244: Again,  $\delta^{13}\text{C}$  and  $\text{CO}_2$  are differed from Ca and  $\delta^{13}\text{Ca}$ ?
23. Table 1: It would be helpful to display the stations according to the latitude. Maybe swap Mahe Island and Acension Island?
24. L227: Does “from 1982 to 2012” mean Estandard? Keep the term in whole manuscript.
25. L234: Does “Standard simulation” mean Estandard?
26. L254: South Pole, Palmer, and Halley, L255: Figs.2, S1, and S2.
27. Figure 3. It would be good to add latitude information next to the name of stations. Also next to Panel (a), ‘Data from Scripps...’ can be removed.
28. L284: the unit ‘permil/century’ is difficult to understand through Fig.3.
29. Table 2: Can they be revised?; Observation data  $\text{CO}_2$  to observation  $\text{C}_a$ , GLOBALVIEW- $\text{CO}_2$  to  $\text{CO}_2$ , SCRIPPS to Scripps.
30. L291: SA( $\delta^{13}\text{Ca}$ )
31. L294: This can be moved to Method section.

The Scripps data, including seasonality, are provided as (i) monthly samples, (ii) a fit to these monthly samples, and (iii) the monthly samples but missing values replaced with fitted values. We also used the original, non-gap-filled data and years with at least 9, 10, or 11 monthly values per year in the regression

32. L311: Why do authors analyze model and observation slope? Please add the purpose.
33. L326: SA means SA( $\text{C}_a$ ) or SA( $\delta^{13}\text{C}_a$ )? Or both of SA?
34. L335: Authors mentioned only the diverse range of SA, but the values seem like very significant. The explanations are focused on they are reliable data rather than the meaning of values. Was this discussed somewhere in the manuscript?
35. L336: Does ‘industrial period’ mean Estandard?

36. L350: Does ‘pre-industrial to the reference period’ mean that ‘ $E_{\text{control}}$  to  $E_{\text{standard}}$ ’?
37. L363 to L374: Can we move whole part to Method section? Or combine to Appendix A?
38. L406 to L441: Those are explained already in Section 4 and more similar to summary rather than discussion section. Only differences are adding references more. It would be good to make it simple and clear as suggested general review.
39. L409: What is the number of ‘relatively small uncertainties’? and for ‘no clear trend in the standard case’ in L423.
40. L423, L425: If ‘standard case’ and ‘preindustrial control’ mean that  $E_{\text{control}}$  and  $E_{\text{standard}}$ , please keep the same term.
41. L445: NPP is different from the NPP in section 4.3.2? If same, why do authors invite another term,  $\epsilon_{\text{NPP}}$ , here?
42. Section 5.2: I have quite similar opinion to section 5.1. The manuscript was mixed with result (discussed before) and seems like more conclusion section? It is very vague what authors really would like to say.

#### Reference

Piao et al.(2017) <https://doi.org/10.1111/gcb.13909>

Lueker et al., (2020) <https://escholarship.org/uc/item/4n93p288>

Greenbook(2007) <https://iupac.org/wp-content/uploads/2019/05/IUPAC-GB3-2012-2ndPrinting-PDFsearchable.pdf>