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Biogeosciences Discussions

Interactive comment on “On the application and interpretation of Keeling plots in paleo climate research – deciphering δ^{13}C of atmospheric CO_2 measured in ice cores” *by* P. Köhler et al.

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The three referees (2 anonymous and Guy Munhoven) made various specific comments to our MS, but all shared the common need, that the MS needs streamlining and shortening to focus its results and conclusions and to make it accessible for the readers.

We understand the need to focus our study and will do so in our revision, but we like to add, that our former experience was, that in general more information on specific scenarios is wanted by readers and referees. This was one of our reasoning to make this study as broad as possible and not be based on some examples.

The suggestions how the MS should be streamlined were different. While referee #3

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suggested to broaden on each process, in which we lay out evidences from other studies, how we treated them in our approach, ending with a hypothesis which should be tested, the other referees suggested to concentrate on some example. We feel, that the suggestion of referee #3 would lead to a further extension of the MS, and also this was in detail performed in our previous article [Köhler et al.(2005)], but without considering the consequences for the Keeling plot approach. We therefore concentrate in our revised MS on three examples in the glacial/interglacial discussion which shows the range of results, and then summarise results from all other processes in a short subsection and in a Table.

Our detailed responses and changes made in the revised version of the MS follows:

- Referee #1 (Guy Munhoven)

  Guy Munhoven is very optimistic about our theoretical extension of the Keeling approach by including the ocean as third reservoir. Besides the suggestion to focus our simulation experiments to some specific examples he also sees the need for a further in-depth discussion and analysis of the consequences of our theoretical approach. He also suggests in great detail in his referee report in which direction the theoretical approach might be developed and how a newly derived equation might be discussed and how this might lead to a deeper understanding of the system. We could see the benefit of this in-depth discussion for the scientific understanding, but feel, that these would be based merely on Guy Munhoven's contribution. We therefore offered him co-authorship for the revised version of the MS, which he kindly accepted.

  1. Page 515, lines 20ff: We change the description of ε to *fractionation* as suggested.
  2. Page 520, lines 20-24: The referee remarks that the original Keeling approach was *per se* only applicable for fast exchange processes. Our theo-

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retical extension of the approach by the introduction of the ocean as third reservoir is extending the applicability to slow processes.

It was further asked if it is possible to estimate the impact of considering the ocean as one single homogeneous reservoir. As laid out in our response to the following comment, we understand our approach as a way to estimate the long-term impact of a perturbation of the carbon cycle by terrestrial carbon uptake/release. In this long-term perspective it is reasonable to restrict the ocean to one single reservoir. What is not possible is to use this theoretical framework to derive answers for time periods, in which equilibration between atmosphere and ocean has not finished. For this purpose, a single ocean reservoir is certainly a too strong simplification. One would need to distinguish not only surface and deep oceans, but also would need to include the different carbon pumps which introduce vertical gradients in DIC and δ^{13}C. This would result in a mathematical framework, which can not be solved analytically in an easy way and is not the intention of our approach.

3. Page 522, line 8: We do not see why Eq 12 and Eq 4 should be equivalent. Eq 4 and 7 and Eq 3 and 6 are equivalents (mass balance equations for a two and a three reservoir system). This is also highlighted in the revision of the MS.

   The referee suggests that Eq 12 might be rewritten in a similar way as Eq 5 (the isotopic signature of the atmosphere as a function of the inverse of the carbon content of the atmosphere). This is done in the revision.

4. Page 523, lines 6-14: The referee states that our conclusion that $\delta^{\Delta A}$ is not defined for $B = 0$ is incorrect. It is further extended, for what reasons this might be the case and that from a lengthly reformulation of Eq 12 one might get a much clearer functional relationship between $\delta^{\Delta A}$ and δ^{B}. From our point of view the discussion about the definition of $\delta^{\Delta A}$ for $B = 0$ is only interesting if one is interested in a further theoretical development of our approach (which we are not), but not for our application, which is the search

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for an explanation for the anomalies in the Keeling plot approach from its expected results. For the case that $\delta^{\Delta A}$ is in a strict mathematical sense defined for $B = 0$ (as argued by the referee) a fundamental assumption of the whole approach (that the atmosphere/ocean system is perturbed by the uptake or release of terrestrial carbon) is not valid anymore and therefore not necessary to be discussed in greater detail.

However, we see the arguments that the reformulation of eq 12 will indeed lead to a deeper understanding of the functional relationship and will therefore be performed in our revision. We will present $\delta^{\Delta A}$ as a function of initial values and known parameters only and show in an Appendix how this equation was developed. This reformulation and the following discussion of the new equation is based on the contribution of Guy Munhoven, for which we offered him co-authorship of the revised version of the article.

It was asked what the meaning of the limiting value $\delta^{\Delta A}_{\delta \mathrm{C} \to 0}$ is. This was already answered in the initial MS (page 532, lines 1-6): $\delta^{\Delta A}_{\delta \mathrm{C} \to 0}$is an embedded feature of the system configuration (fractionation during gas exchange, buffer factor, initial distribution of carbon between oceanic and atmospheric reservoirs). It determines how much of the original isotopic signature of the perturbation stays air-borne (in the atmosphere). The referee refers also to this paragraph, but finds the result of our comparison of the theoretically derived value of $\delta^{\Delta A}_{\delta \mathrm{C} \to 0}$ with those from the analysis of the experiments counterintuitive. We therefore expand on this topic here: In Fig 3a one can learn, that $\delta^{\Delta A}$ depends only slightly on the amplitude of the perturbation (B). In the observed range of the amplitude B (0 to 1000 PgC) the amplitude chosen in our simulations (10 PgC) is close to 0 PgC, the value to which the anomaly is converging in $\delta^{\Delta A}_{\delta \mathrm{C} \to 0}$. From the gradient of the functional relationship between $\delta^{\Delta A}$ and B we expect only gradual changes in $\delta^{\Delta A}$ if the amplitude is reduced further. This can also be learnt from a comparison of simulations with different amplitudes as summarised in Table 2: The y-

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intercept of the prior/after regression model is the same for two different amplitudes (5 or 10 PgC) in B. In conclusion, we can say that for small anomalies the y-intercept y_0 of the Keeling plot is similar to the $\delta^{\Delta A}_{\delta C \to 0}$ which is determined by the system configuration. Still existing offsets between y_0 and $\delta^{\Delta A}_{\delta C \to 0}$ are caused by the carbon pumps in the ocean, as laid out on page 532.

5. Page 526, lines 4-15: We took the Point Barrow data as representative of the seasonal cycle because we were interested in both long time series in CO_2 and δ^{13}C and larger seasonal amplitudes. The seasonality in both CO_2 and δ^{13}C is higher in the high northern latitudes and therefore Point Barrow seemed to be a good choice for our investigations.

6. Page 528, lines 1-2: The referee has reservations about our statement that a certain fraction of of the signal is explainable by the Keeling plot approach. We followed his arguments, that this might not in general be the case and abstained from using the ratio and revised our conclusions accordingly.

7. Page 532, lines 1ff: It was asked why the y-intercepts for the re-equilibration model fall close to $\delta^{\Delta A}_{\delta C \to 0}$. An explanation why the prior/after model (model 2) leads to similar results as $\delta^{\Delta A}_{\delta C \to 0}$ was explained in detail in point 4 of our response to referee #1.

8. Page 534, lines 1-5: The referee asks if the Keeling plot is not invalid if ε_{TB} is strongly changing? In principle, yes. It is one of the assumption of the approach, that the isotopic signature of the source which is emitting carbon to the atmosphere is not changing. If it is changing, it can not be expected that the y-intercept of the Keeling plot will correctly record the isotopic signature of the source, indeed, there is no well defined isotopic signature as it is changing over time. However, we here explore the possibilities and limitations of the approach and have already violated the assumption of a two reservoir system fundamentally. We therefore believe, that the violation of

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this second assumption will not be of greater importance than the violation of the first and is therefore of minor importance. Furthermore, the detailed discussion of the changes in the terrestrial biosphere based on these three different scenarios, in which ε_{TB} was changing is omitted in the revised version due to the streamlining of the MS.

9. Page 538, lines 8-16: It is stated by the referee that the existence of $\delta^{\Delta A}_{\delta \mathrm{C} \to 0}$ of the system with variable ε_{AO} requires the derivative of ε_{AO} with respect to B to exists for $B = 0$. As mentioned above, our theoretical framework has no physical meaning for the case $B = 0$. This exercise would therefore be only of academical value. Furthermore, it has to be stressed again, that all experiments in which the perturbation of the carbon cycle has its origin not in the terrestrial biosphere are not covered with our theoretical approach. Comparing nevertheless simulation experiments with variable marine and terrestrial carbon cycle in our context is merely a test bed to learn, if those restricted information retrievable from the Keeling plot approach are useful to distinguish between different processes.

10. Page 538, lines 1-5: This paragraph is no longer part of the revised MS. But, indeed, the way we stressed the application of regression functions over short time windows in our initial draft was probably violating our approach. As we clearly point out now, this extended Keeling plot approach sheds light on long-term processes, in which equilibration between atmosphere and ocean has occurred.

11. Page 554, caption Table 1: The referee suggested that the interpretation of the ratio $y_0/\delta^{13}C_{ant}$ as the fraction which can be explained by the Keeling might be wrong. In our streamline procedure we deleted Table 1 and revised our conclusion concerning this issue accordingly.

12. We took up all the technical corrections.

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- Referee #2

  This referee suggests to focus the article and avoid the large amount of details given so far. He also comments on the fact that we were fitting straight lines to data that clearly should not be approximated with straight line. We follow his comments by sharpening the focus and avoid most of our earlier fitting exercises. However, we like to point out that one main reason for these numerous linear fitting models was to test individual hypotheses, how these data can be explained in a systematic way.

  Our response to his/her specific and details comments follow:

  1. Abstract: Our interpretation of the anthropogenic rise in CO_2 was questioned. We revised our discussion and conclusions accordingly.
  2. page 517, eq 4: This is an exact equation and not an approximation.
  3. page 522, line 6: We took up the suggestion to explain the phrase "effective carbon signature of the isotopic change in the atmosphere" when it first appeared in the text.
  4. page 531, line 5: We agree with the referee that this third regression model fails to explain the data accurately in most cases and refrain from using it again.
  5. page 531, line 28: Various intercepts were calculated to investigate the range of possible responses and to understand if some hidden information contained in the Keeling plot might evolve. The referee here asked what can be learnt from this amount of experiments. This range of experiments was performed to see what scenarios yield to results which are closest to the observation. We extended the MS on this intention of our investigations.
  6. All the eight specific comments we taken up in the revision of the MS.

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- Referee #3

1. One main point of the referee is that the number of results and the sort of presentation needs revision and streamlining. He explicitly suggests a procedure, what might be done to focus the article:

   1. Outline a single hypothesis (broad up by the authors or by former studies), about carbon cycle changes.
   2. Explain expected impacts on δ^{13}C and the Keeling plot.
   3. Describe how we implement this hypothesis in our modelling framework.
   4. Evaluate the hypothesis in the light of our new results.

   While this approach is in principle an elegant way to tackle this issue, we have one main reason why we do not proceed as suggested here: This outline was more or less used in our previous study [Köhler et al.(2005)]. There, the processes sumarised in Table 3 in [Köhler et al.(2006)] were analysed with respect to their impact on atmospheric CO_2 and atmospheric δ^{13}C. However, a Keeling plot analysis was not performed previously.

   Nevertheless, the idea behind this suggestions is to improve the red line through the article, which we follow on by focusing on some important examples. We therefore took up the alternative suggestions of the other referees to reduce the number of examples to a few cases only.

2. It was stated that the conclusions of the MS is, that the Keeling plot approach cannot be effective in deciphering long-term carbon cycle dynamics. It was therefore suggested that we state some examples where the Keeling plot might be applicable. As we will lay out clearly in the revision of the MS, our theoretical framework of the three reservoir system gives an explanation of the Keeling plot analysis for the long-term effect of terrestrial carbon release. Indeed, different processes cannot be deciphered by the approach, but for the terrestrial case the y-intercept can be understood. One way to further extend this mathematical framework for the oceanic carbon cycle might be

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to subdivide the oceanic reservoir. In doing so a conceptual understanding of the other y-intercepts would emerge, but this exercise lies beyond the scope of the MS.

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