Rebuttal to review comments on “Rethinking climate, climate change, and their relationship with water” – Round 2

by Demetris Koutsoyiannis

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Key:

Review comment.

Response.

Quotation from manuscript.

Quotation from other papers.

Academic Editor

Dear Author,
Thank you for responding to the reviewers' comments. We propose that the manuscript should be further revised (minor revision) in order to address the additional comments provided by reviewers 1 and 2. After that, the manuscript should be published in the Water Journal.
Looking forward to hearing from you
With kind regards
Vasileios Tzanakakis (on behalf of all Guest Editors)

I appreciate the positive attitude of the Editor and the professional handling of the review process.

I have addressed the review comments in the manner that I describe in full detail below. In addition, I have substantially improved Appendix E, by processing and including an additional data set. I have done this latter work to better address a comment by Reviewer 1 in Round 1, even though he (or she) did not have any related comment in the present Round 2.

Reviewer 1

I thank the author for his responses to my comments. There needs to be further clarification in the paper.

It has been a big pleasure for me to interact with Reviewer 1, an apparently knowledgeable scientist and also a kind person. Further clarification is provided below and also in the paper.

In my review of the original submission I commented that ‘As an overall comment a significant proportion of the text seems to wander and should be tightened up. A number of comments are vague, and unsubstantiated’. The author has responded with ‘However, being an old man now, I have developed through the years a style of writing which expresses myself. Some like it (cf. Reviewer 4), some not, but my personal taste is to avoid a stereotypical stylized text. I hope the Reviewer can tolerate that.' This did not really address my point as to the structure of a scientific and rigorous paper. The guidelines for the 'Water' journal specify that 'Water has no restrictions on the length of manuscripts, provided that the text is concise and comprehensive'. I will leave it to the Editor to decide whether the text is 'concise' and appropriate for the journal.

I am thankful to the Reviewer for tolerating my style of writing, even though he (or she) clearly does not like it.

Another response I had some difficulty with was in connection with my comments on atmospheric CO2 levels since the Miocene and also the last ~1 Ma. Important that the author updates himself with the relevant literature on this issue. For a start, I recommend reading (and making reference to)

Ying Cui, Brian A. Schubert and A. Hope Jahren, 2020: A 23 m.y. record of low atmospheric CO2. Geology, 48, 888-892, doi: 10.1130/G47681.1 and

Gavin L. Foster, Dana L. Royer and Daniel J. Lunt, 2017: Future climate forcing potentially without precedent in the last 420 million years. Nature Communications, 8, 14845, doi: 10.1038/ncomms14845.

As to the last ~1 Ma, perhaps he is not aware the numerous deep ice cores from low-accumulation regions of the East Antarctic Plateau which go back about 0.8 Ma, far further than the Vostok core to which he refers. A good starting point for reading would be the papers of

Edward J. Brook, and Christo Buizert, 2018: Antarctic and global climate history viewed from ice cores. Nature, 558, 200-208, doi: 10.1038/s41586-018-0172-5, and

Augustin, L. et al., 2004: Eight glacial cycles from an Antarctic ice core. Nature, 429, 623-628, doi: 10.1038/nature02599.

First off, I appreciate the suggestions of these references, whose usefulness is wider than the scope of this paper. I have now cited all these and some more, in the following new paragraph in section 5, above the bulleted paragraphs (where the suggestions are included as references 51-57):

> The importance of the effect of the different greenhouse gases, or other agents affecting climate, is not necessarily related to the research efforts and scientific publications on each of them. Arguably, the fact that the CO_2 has been so heavily and repeatedly studied particularly in paleoclimatology studies [e.g. 49, 51-57] does not suggest that it is more important a greenhouse gas than water. A simpler explanation is that CO_2 concentration is easier to study because its change in time is smoother, because its spatial heterogeneity is much lower than that of water vapour, and because it can be detected in ice and sediment cores, stomatal complexes, etc.

With reference to the discussion about the Miocene, what I had written in the Rebuttal for Round 1 is this:

I strongly doubt about the Miocene. My perusal of paleo data does not verify this; see Figure 3 in Koutsoyiannis and Kundzewicz (2020; Atmospheric Temperature and CO2: Hen-Or-Egg Causality? Sci, 2, 83. doi: 10.3390/sci2040083).

Below, I have copied the relevant graphs from the suggested references and also some more, which have been reference in Koutsoyiannis and Kundzewicz (2020).

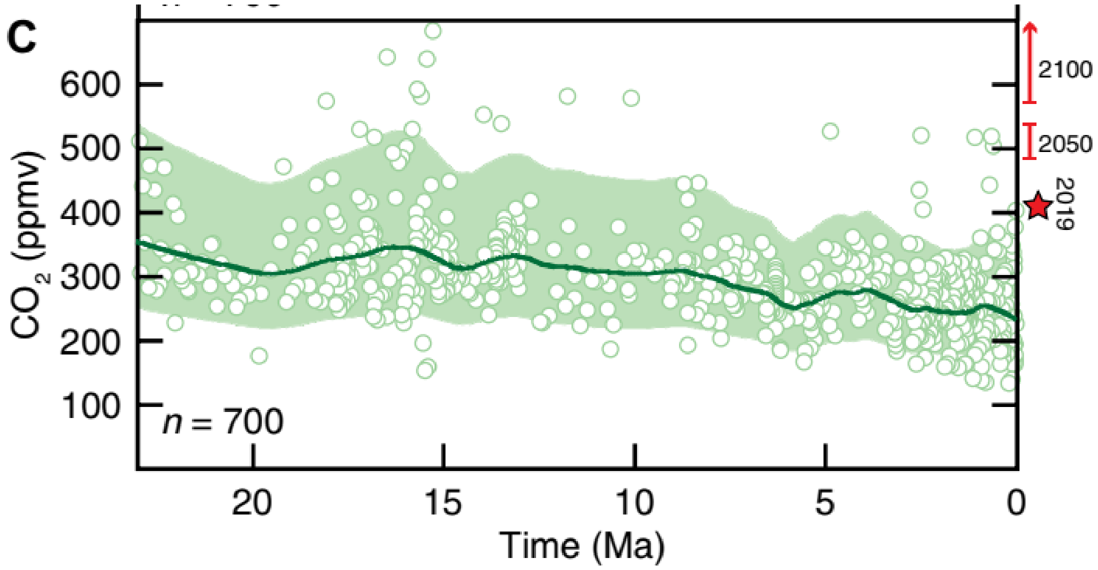


Figure 1. Reconstruction of late Cenozoic (23–0 Ma) CO2 using C3 plant remains….[From Cui et al., 2020.]

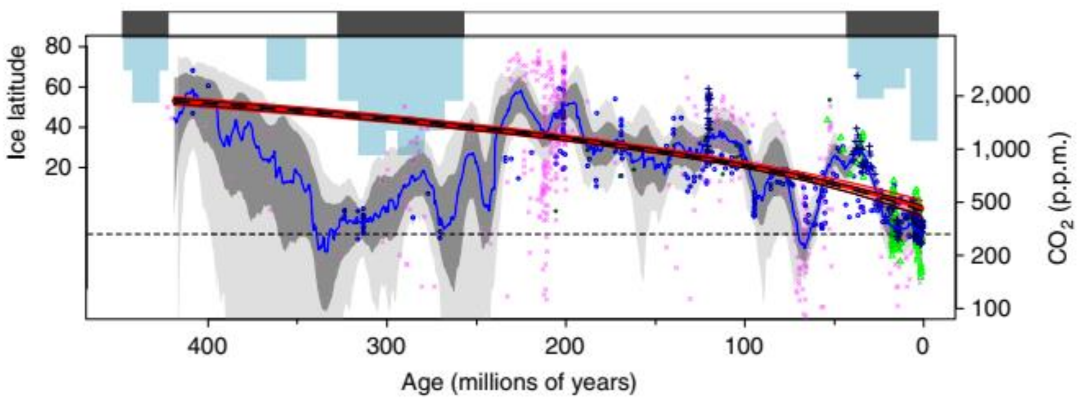


Figure 1 | Temporal evolution of climate and atmospheric CO2. … [From Foster et al., 2017/]

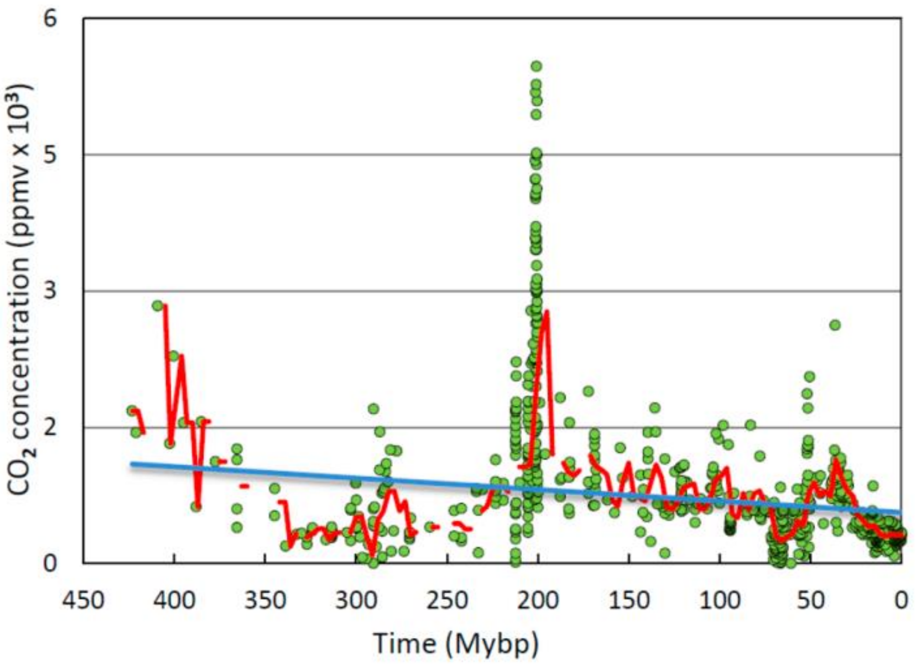


Figure 4. Atmospheric carbon dioxide (CO2) concentration proxies over the Phanerozoic Eon… [From 37. Davis, W.J. The relationship between atmospheric carbon dioxide concentration and global temperature for the last 425 million years. Climate 2017, 5, 76.]

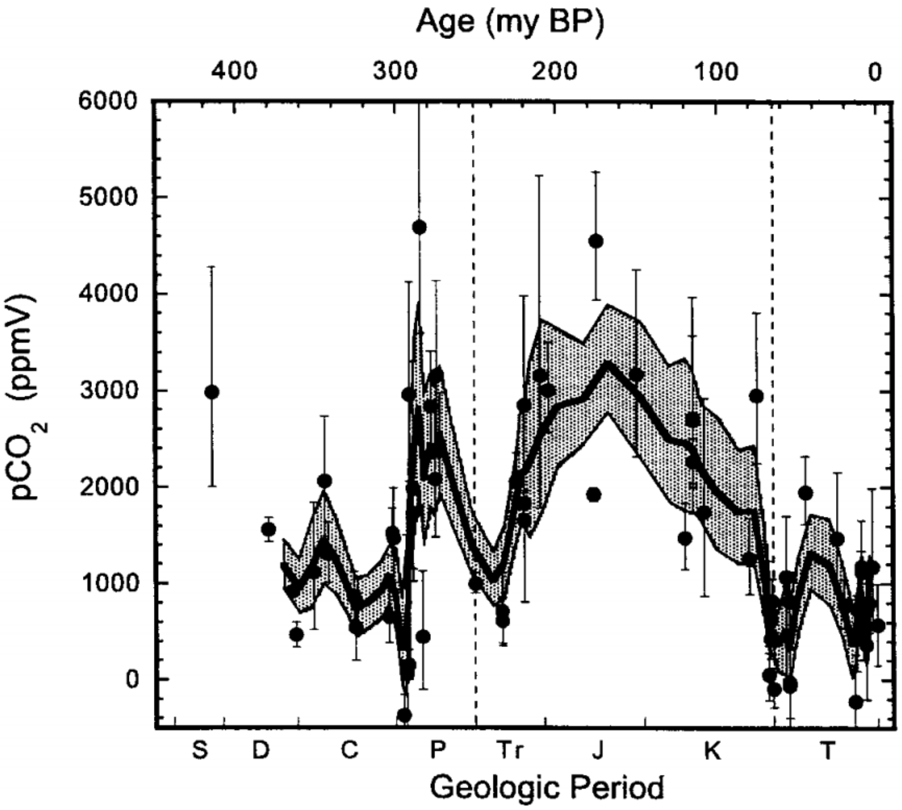


Figure 10. Results of the PCO_2 paleobarometer… [From Ekart, D.D.; Cerling, T.E.; Montanez, I.P.; Tabor, N.J. A 400 million year carbon isotope record of pedogenic carbonate: implications for paleoatmospheric carbon dioxide. Am. J. Sci. 1999, 299, 805–827.]

Given that the Miocene extends from 23 to 5.3 million years ago, the above graphs amplify my doubt about what the Reviewer asserted. Most of the graphs contain data values of CO_2 concentration exceeding 1000 ppm during the Miocene.

In addition, I reiterate the point I made in the Rebuttal of Round 1 that we cannot compare data values that refer to different time scales/resolutions. I do not think that the time scale of the new references suggested by the Reviewer is finer than 1000 years. For example, the Review paper by Brook and Buizert refers to the data from the following paper:

Higgins, J.A.; Kurbatov, A.V.; Spaulding, N.E.; Brook, E.; Introne, D.S.; Chimiak, L.M.; Yan, Y.; Mayewski, P.A.; Bender, M.L. Atmospheric composition 1 million years ago, *Proceedings of the National Academy of Sciences* **2015**, 112 (22) 6887-6891; doi: 10.1073/pnas.1420232112.

By inspecting the Supporting Information of the latter paper, one sees ages of consecutive data values greater than 1000 years and high age uncertainties.

A characteristic figure of Higgins et al., which is for the EPICA data, is plotted below, superimposed to that in Koutsoyiannis and Kundzewicz (2020) for the Vostok data. For about 200+ million years the data agree, but in older times there is disagreement in the timing.

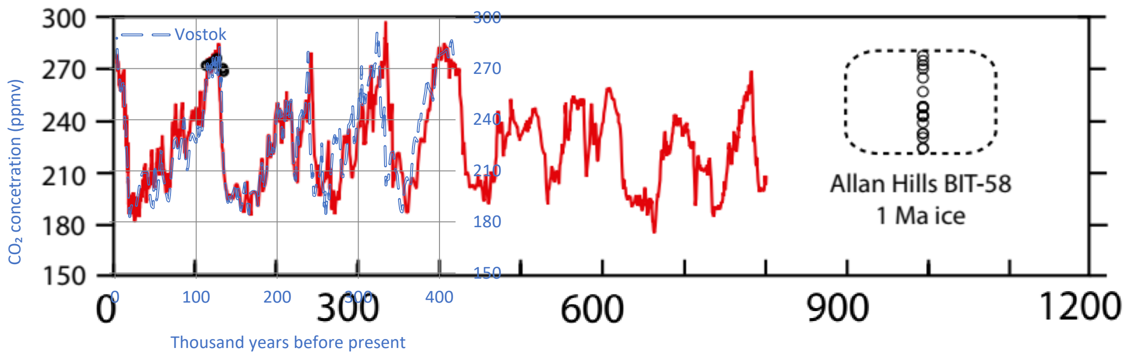


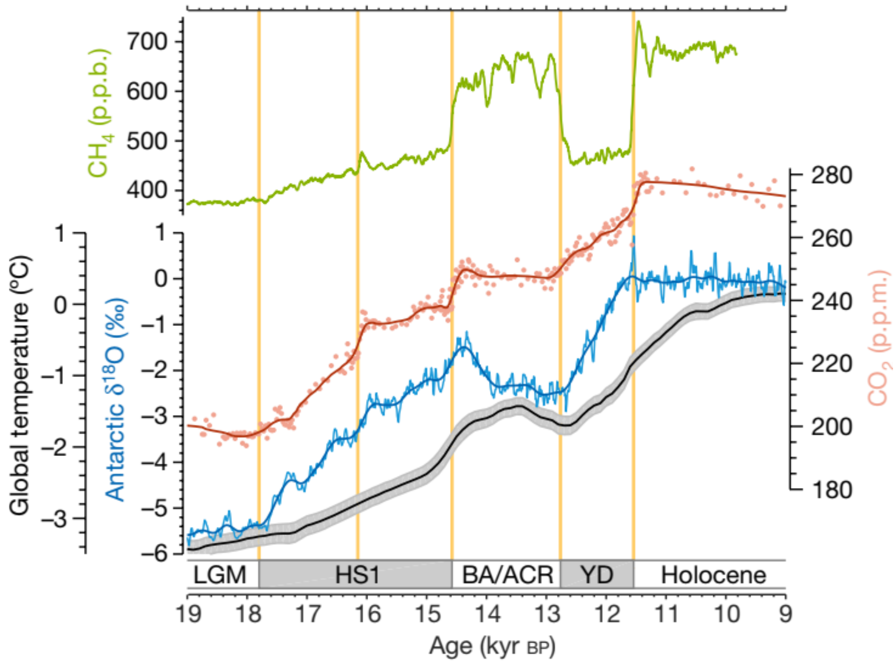
Fig. 1. Records of […] (B) CO2 [….] from the Allan Hills BIA (Site 27; black line and black symbols between 115 and 250 ka) compared with records from […] EPICA […] ([…] red […] lines) …. [From Higgins et al., 2015.]
The figure in blue is superposition of the Vostok time series reproduced from Koutsoyiannis and Kundzewicz (2020).

I do not know what the reason of the disagreement is, yet I think that the Vostok data are more thoroughly studied, in particular with respect to the difference between the ice and air age, which reaches about 6000 years [Barnola, J.M., Pimienta, P., Raynaud, D. and Korotkevich, Y.S., 1991. CO2-climate relationship as deduced from the Vostok ice core: A re-examination based on new measurements and on a re-evaluation of the air dating. Tellus B, 43(2), 83-90.]. On the other hand, in Fig. 3 of Higgins et al. one reads “assuming a zero gas age/ice age difference”.

I wish to stress that all this discussion is of general academic and scientific interest but does not influence at all the content of the paper. Therefore, all these details have not been included in the body of the paper.

Regarding the author's rebuttal comment of '… I wonder how the Reviewer knows what happened "in reality, during the Holocene" ', suffice it to say that this period, starting after the finish of the LGM, is universally regarded as one of low variability in most climate parameters. As a particular aspect of this, Box 2 in Brook and Buizert's paper shows very little CO2 variability over that period.

My comment was about the expression "in reality". Indeed, the paper by Brook and Buizert, reproduced below, shows a flat line for the Holocene, but I doubt if we can call a graph with paleoclimatic estimations "reality".



Box 2. CO2 and temperature phasing during the last deglaciation [From Brook and Buizert, 2018]

Other research results do not indicate such constancy, nor do all estimates agree. To provide an example, the following figure from the indicated paper tells a different story.

In addition, I wish to reiterate my comment that we cannot compare annual data with those that are from large time scales. For example, the time resolution in the graph below is, on the average, more than 100 years and is some cases more than 200 years.

In any case, I do not know what the reality actually is and, again, the discussion is of general academic and scientific interest. It does not influence at all the content of the paper and, therefore, all these details have not been included in the body of the paper.

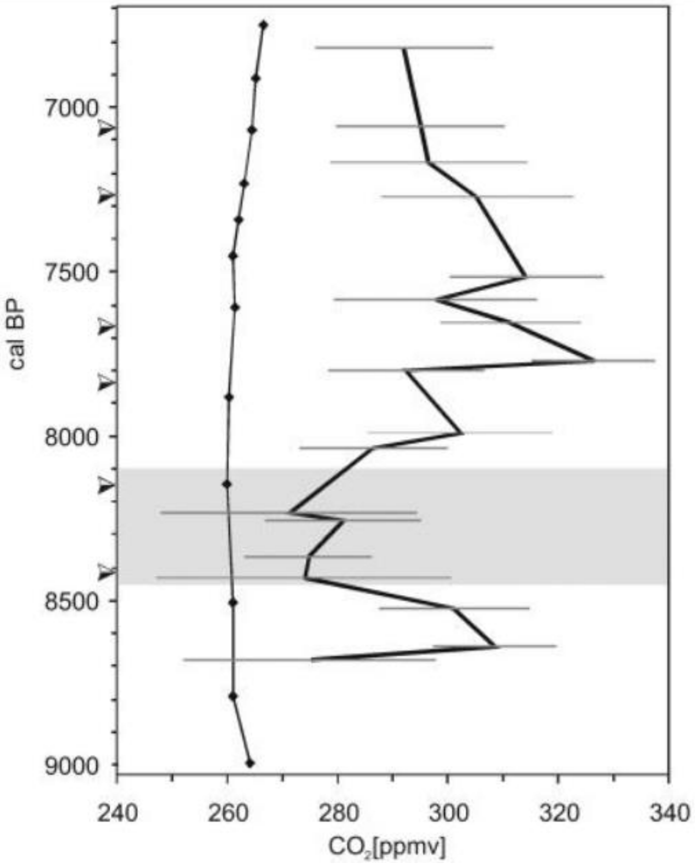


Fig 2. Reconstructed CO2 concentrations for the time interval between ≈8,700 and ≈6,800 calendar years B.P. based on CO2 extracted from air in Antarctic ice of Taylor Dome (left curve [….] and SI [= stomatal index] data […] (right curve)…. [From Wagner F, Aaby B, Visscher H. Rapid atmospheric CO2 changes associated with the 8,200-years-B.P. cooling event. Proc Natl Acad Sci USA. 2002 Sep 17;99(19):12011-4. doi: 10.1073/pnas.182420699.]

I was a little puzzled by the author's response to my comment on the carbon budget. He comments that '… I respectfully disagree that the carbon budget could ever be balanced or close to it. In my view, balanced system is only a dead system, because it is the imbalances, whether small or big, that produce change, i.e., alive systems.' In preindustrial times there were huge SEASONAL fluxes betewwnt the ocean, atmosphere and biosphere, but on an ANNUAL basis these averaged out to zero. However, it could not be argued that it was a 'dead' system.
The author's presents the appropriate budget equation, i.e.,

Change of storage in the atmosphere = natural emissions – natural sinks + human emissions

From my comments above, if we are considering timescales of > 1 year, the first two terms on the right side of the equation will cancel. Hence the statement he makes 'that each of natural emissions and natural sinks are more than 20 times greater than human emissions' is (as I indicated before) meaningless for the overall trend of CO2 (and is only true – and irrelevant in the present context - if one is only considering the seasons). As I commented in my first review, examination of (and reference to) the 'Keeling Curve' (https://keelingcurve.ucsd.edu/) will help the author understand this.
The part of the text must be rewritten.

With all respect, I do not think that this part of the text needs to be rewritten. But I am providing below detailed explanation in support of my opinion.

First off, the Reviewer suggests to examine the "Keeling Curve" and understand his (or her) own assertions based on such examination. However, I have indeed studied it very recently, along with professor Zbigniew Kundzewicz, and published another paper on it in the MDPI journal Sci, whose details I have given both in the manuscript and in my reply to the review comments of Round 1 (I also repeated the details above).

The Reviewer's comment would be pertinent to that paper (Koutsoyiannis and Kundzewicz, 2020) and not the current one. Therefore, I am discussing it only in this Rebuttal and do not include the discussion in the current paper. For the reader's convenience, I am copying below some figures from that paper, starting with its Figure 6.

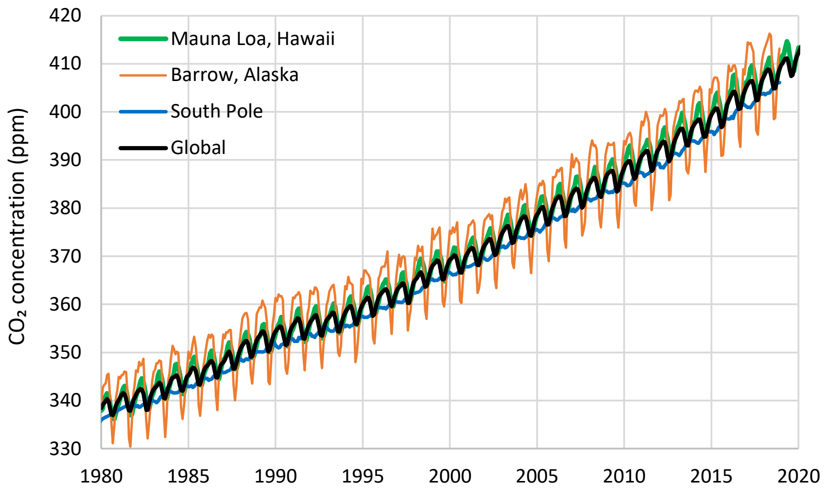


Figure 6. Plots of the data series of atmospheric CO_2 concentration measured in Mauna Loa (Hawaii, USA), Barrow (Alaska, USA), and South Pole, and the global average. [From Koutsoyiannis and Kundzewicz, 2020.]

This figure shows that not only have we studied the so-called "Keeling Curve", which refers to the atmospheric CO_2 concentration at Mauna Loa, but we also studied two other locations and the global atmospheric CO_2 concentration.

All curves show increasing atmospheric concentration as well as the effect seasonality. These are the data, reliable measured, and that is fact. All other statements that the Reviewer or I may make are interpretations. Clearly, his (or her) interpretation is different from mine as evident from his (or her) statement:

> *Hence the statement he makes 'that each of natural emissions and natural sinks are more than 20 times greater than human emissions' is (as I indicated before) meaningless for the overall trend of CO_2 (and is only true – and irrelevant in the present context - if one is only considering the seasons).*

What the cause of the increase is, is not fact. Even though the standard interpretation is that the increase has been caused by human emissions, which in turn caused increased of temperature, yet this is not a fact.

Our own perusal of the CO_2 concentration and temperature resulted in a different, if not opposite, story. Quoting from the abstract of Koutsoyiannis and Kundzewicz (2020):

> *While both causality directions exist, the results of our study support the hypothesis that the dominant direction is $T \rightarrow CO_2$. Changes in CO_2 follow changes in T by about six months on a monthly scale, or about one year on an annual scale. We attempt to interpret this mechanism by involving biochemical reactions as at higher temperatures, soil respiration and, hence, CO_2 emissions, are increasing.*

Our results are illustrated in Figures 11 and 12 in Koutsoyiannis and Kundzewicz (2020), copied below. Even the graphical illustration suffices to convey the message quoted from the abstract. But we have also developed a theoretical stochastic framework for causality, whose application confirmed with statistical results what the figures say. That is, temperature increase causes CO_2 increase.

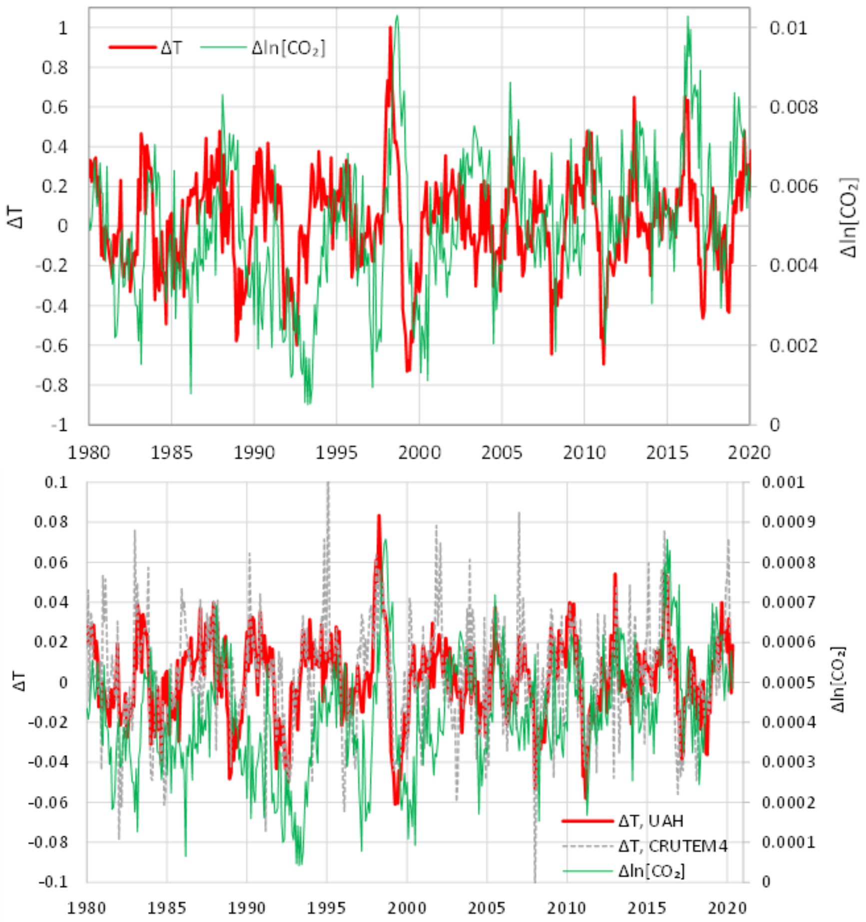


Figure 11. Differenced time series of UAH temperature and logarithm of CO_2 concentration at Mauna Loa at monthly scale. The graph in the upper panel was constructed in the manner described in the text. The graph in the lower panel is given for comparison and was constructed differently by taking differences of the values of each month with the previous month and then averaging over the previous 12 months (to remove periodicity); in addition, the lower graph includes the CRUTEM4 land temperature series. [From Koutsoyiannis and Kundzewicz, 2020.]

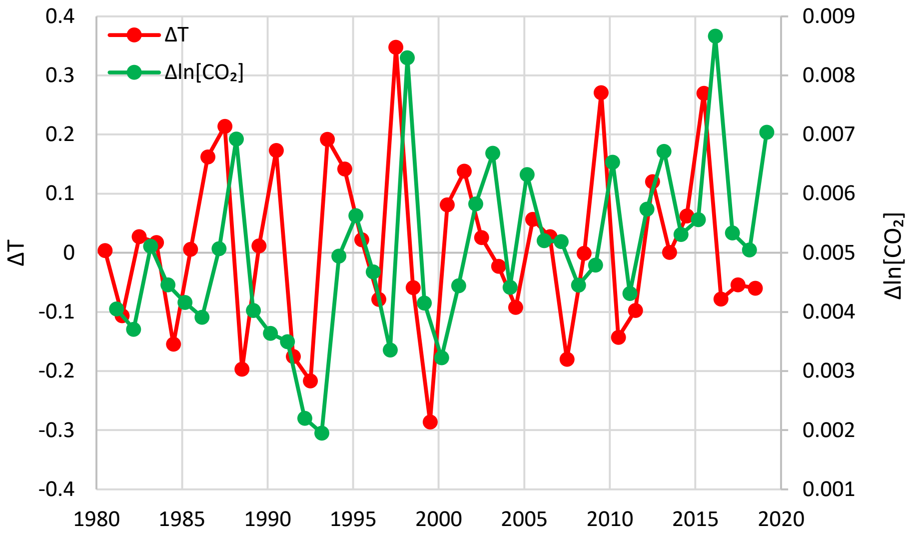


Figure 12. Annually averaged time series of differenced temperatures (UAH) and logarithms of CO_2 concentrations (Mauna Loa). Each dot represents the average of a one-year duration ending at the time of its abscissa. [From Koutsoyiannis and Kundzewicz, 2020.]

Furthermore, I am glad that the Reviewer found appropriate my budget equation, i.e.,

Change of storage in the atmosphere = natural emissions – natural sinks + human emissions

Interestingly however, the Reviewer states:

> *In preindustrial times there were huge SEASONAL fluxes betewwnt the ocean, atmosphere and biosphere, but on an ANNUAL basis these averaged out to zero. However, it could not be argued that it was a 'dead' system.*

Here I must stress that in all above graphs the seasonality effect has been "removed" by techniques discussed in the paper. So, our results are not affected by seasonality. Coming now to preindustrial times, the above equation becomes:

Change of storage in the atmosphere = natural emissions – natural sinks

The reviewer opines that there was balance in natural emissions and sinks and thus the left-hand side of the equation would be zero at all scales above annual. I respectfully disagree and I see no reason at all that this would ever happen. I reiterate my assertion that only in dead systems there is such balance. I have studied this problem in my paper:

D. Koutsoyiannis, A random walk on water, *Hydrology and Earth System Sciences* 2010, 14, 585–601, http://dx.doi.org/10.5194/hess-14-585-2010.

I note that the latter paper examines the water balance, in connection to the vegetation dynamics, rather than the carbon dynamics, but I believe the results are of general validity. According to these results and quoting from that paper:

> *once the system reaches its equilibrium state, it becomes a "dead" system, exhibiting no change.*

and

> *We can easily imagine that if the system dynamics were different, so as to drive it to its "dead" equilibrium state, there would not be uncertainty in the future. The nonlinear type of dynamics we used is the agent that made the system "alive", i.e. changing and not dying. Apparently, what makes the system alive is the same agent that creates the uncertainty. Only dead systems are certain – and this might be useful to recall when thinking about eliminating uncertainty.*

More specifically, in Koutsoyiannis (1010) I intentionally devised a foolishly simple, caricature system shown in Figure 1 of that paper and also copied below. The dynamics of the caricature system consists of the water balance equation (linear) and a very simple equation for the vegetation dynamics (nonlinear), which make a 2D nonlinear dynamical system in discrete time. To make the system as foolishly simple as possible, I even assumed that the system input (infiltration, φ) is constant in all time steps. Yet this produces a system output (transpiration, τ) and system state variables (soil water, x, and vegetation cover, v) changing all the time, as seen in Figure 4 of Koutsoyiannis (2020) also copied below. Even the variability of the state variables changes all the time as seen in Figure 12 of Koutsoyiannis (2020) also copied below.

Note that the caricature system does not exhibit any type of seasonality.

I hope this suffices to clarify my claim about changing equilibrium in alive (non-dead) systems.

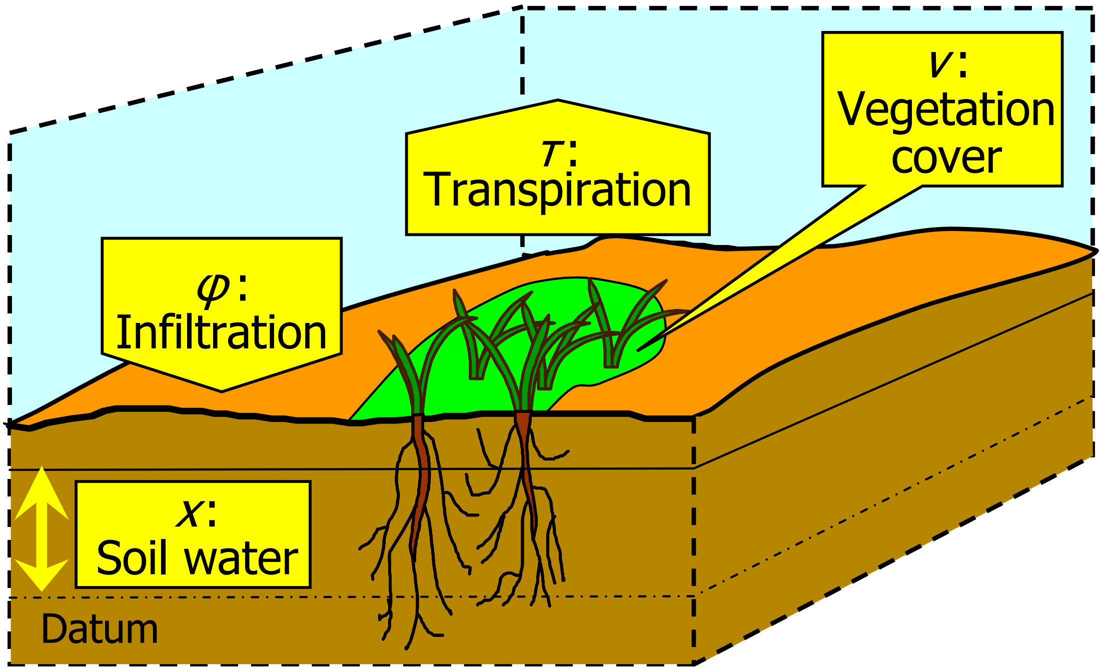


Figure 1: A caricature hydrological system for which the toy model is constructed. [From Koutsoyiannis, 2010.]

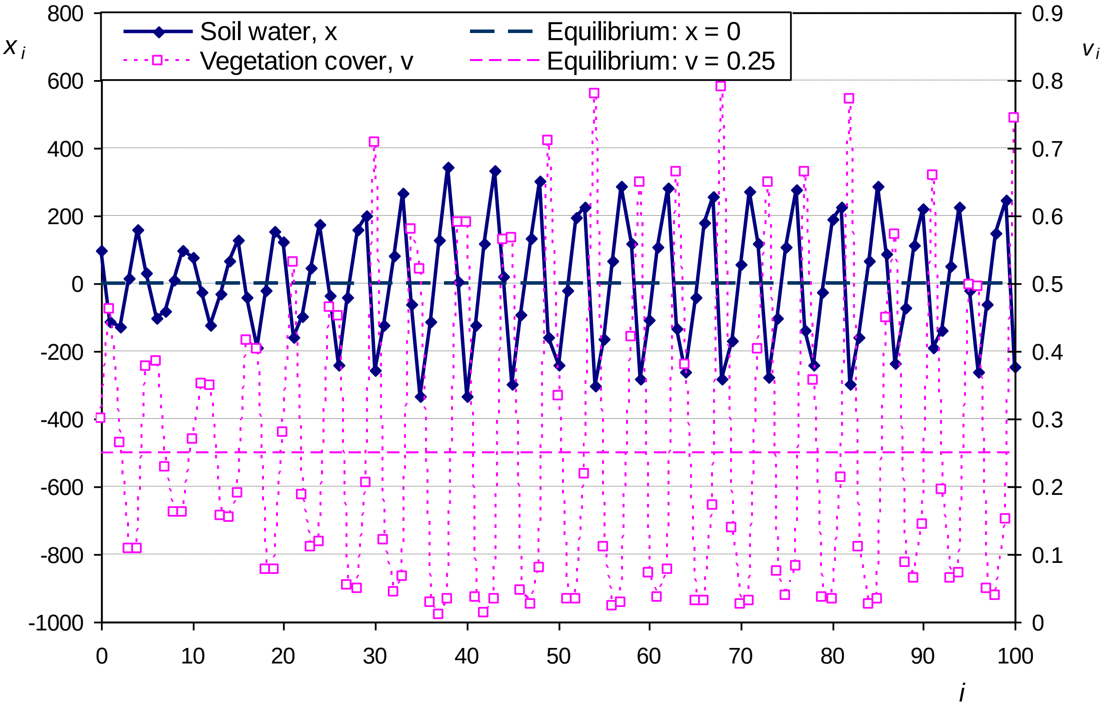


Figure 4: System trajectory for 100 time steps assuming initial conditions x_0 = 100 mm and v_0 = 0.30. [From Koutsoyiannis, 2010.]

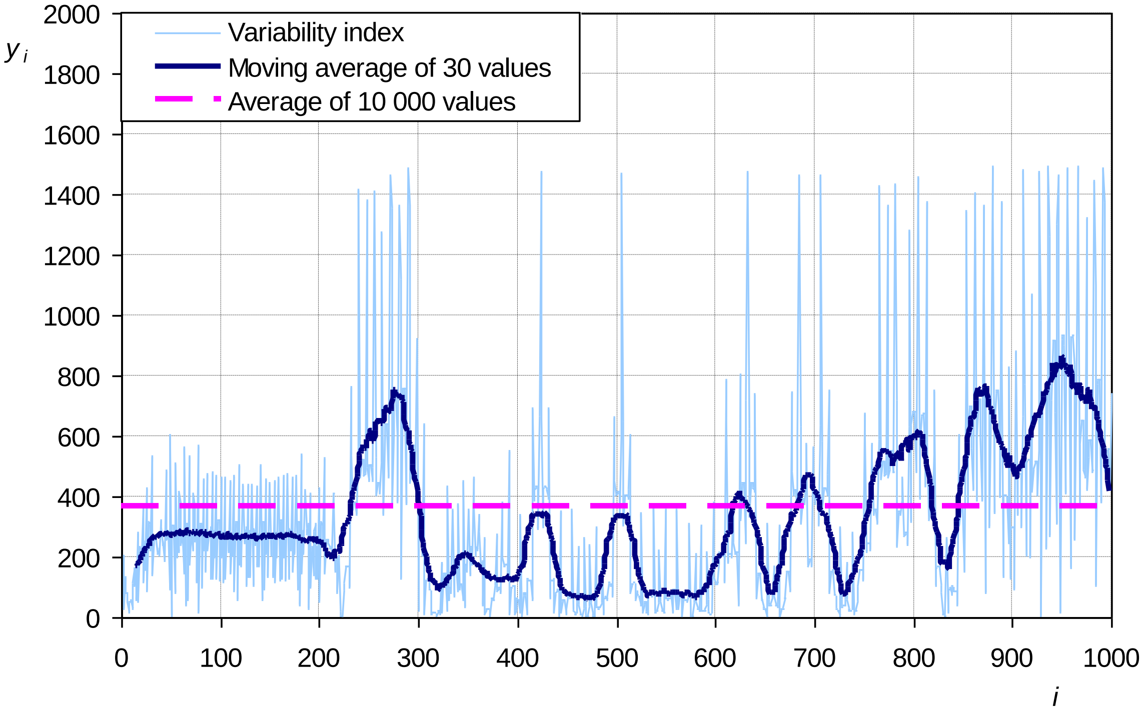


Figure 12: Evolution of the soil water x_i, the variability index y_i, as well as their moving averages of length 30, for time i up to 1000. [From Koutsoyiannis, 2010.]

Once these points have been addressed in an appropriate manner I will be able to recommend acceptance.

I believe I have fully addressed the Reviewer's comments, either in the paper or in this Rebuttal report or in both.

Reviewer 2

> As the author's response to the first round of reviews was generally contrary and combative, and indeed "helped me to strengthen the paper against them" (referring to this reviewer in the Acknowledgements), I have no further comments to the author regarding this revision. I wish the author good luck in his retirement.

I thank the Reviewer for his (or her) generosity and wishes.

Reviewer 4

> I am proposing further procedure for publishing of the paper. I am satisfied with the answers on my review.

I thank the reviewer for his positive attitude in both review rounds.