

From hen's egg to serpent's egg: Peer reviews and other attacks on science for silencing voices opposing the "climate crisis" narrative

by Demetris Koutsoyiannis

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Sources of images: (left) Flyte so Fancy¹; (right) Wikipedia²

Crime begins with propaganda, even if such propaganda is for a good cause (Hans Fritzsche³)

In the four-year period 2020-2023, I published a dozen of papers on climate. Four of them, prepared jointly with other colleagues, investigated causality in climate. Two of these four were published in the MDPI journal *Sci* and two in the *Proceedings of The Royal Society A – Mathematical, Physical and Engineering Studies*. Their details are:

1. D. Koutsoyiannis, and Z. W. Kundzewicz, 2020. Atmospheric temperature and CO₂: Hen-or-egg causality?. *Sci*, 2 (4), 83, [doi: 10.3390/sci2040083](https://doi.org/10.3390/sci2040083).
2. D. Koutsoyiannis, C. Onof, A. Christofides, and Z. W. Kundzewicz, 2022. Revisiting causality using stochastics: 1.Theory. *Proceedings of The Royal Society A*, 478 (2261), 20210835, [doi: 10.1098/rspa.2021.0835](https://doi.org/10.1098/rspa.2021.0835).
3. D. Koutsoyiannis, C. Onof, A. Christofides, and Z. W. Kundzewicz, 2022. Revisiting causality using stochastics: 2. Applications. *Proceedings of The Royal Society A*, 478 (2261), 20210836, [doi: 10.1098/rspa.2021.0836](https://doi.org/10.1098/rspa.2021.0836).
4. D. Koutsoyiannis, C. Onof, Z. W. Kundzewicz, and A. Christofides, 2023. On hens, eggs, temperatures and CO₂: Causal links in Earth's atmosphere. *Sci*, 5 (3), 35, [doi: 10.3390/sci5030035](https://doi.org/10.3390/sci5030035).

¹ *How many eggs can a hen lay - The Lifecycle of Laying Hens*, <https://www.flytesofancy.co.uk/blogs/information-centre/how-many-eggs-can-a-hen-lay>.

² *The Serpent's Egg*, [https://en.wikipedia.org/wiki/The_Serpent%27s_Egg_\(film\)](https://en.wikipedia.org/wiki/The_Serpent%27s_Egg_(film))

³ The quoted phrase by Hans Fritzsche (of the Reich Ministry of Public Enlightenment and Propaganda in Nazi Germany; https://en.wikipedia.org/wiki/Hans_Fritzsche) is taken from: L. Goldensohn, Hans Fritzsche interview, in *The Nuremberg Interviews*, Ed. by R. Gellately, Vintage Books, New York, 2005.

In these we examined the potentially causal relationship between the atmospheric carbon dioxide concentration ($[CO_2]$) and atmospheric temperature (T). While the established narrative, supported by conventional wisdom, is that increased $[CO_2]$ causes increase in T , in paper #1 we questioned this conviction and put the relationship of the two in a Plutarchean hen-or-egg framework.

In paper #2 we developed an advanced stochastic methodology for identifying potential causality, which in paper #3 we applied to several problems, including the $T - [CO_2]$ relationship. We concluded that there is a unidirectional, potentially causal link between T as the cause and $[CO_2]$ as the effect. The reverse relationship (i.e., that promoted by the established narrative) was excluded as violating a necessary condition of causality.

In paper #4 we provided more detailed analyses and additional evidence enhancing the validity of the results of paper #3.

I have now published one more paper using several proxy series, extending over the entire Phanerozoic or over parts of it, as well as instrumental data of the modern period. The details are:

5. D. Koutsoyiannis, 2024. Stochastic assessment of temperature – CO_2 causal relationship in climate from the Phanerozoic through modern times. *Mathematical Biosciences and Engineering*, 21 (7), 6560–6602, [doi: 10.3934/mbe.2024287](https://doi.org/10.3934/mbe.2024287).

The extensive analyses confirm the findings of the earlier papers for the modern period and expand them for 500 million years in the past. The results converge to the single inference that change in temperature leads and that in carbon dioxide concentration lags.

Publishing papers that challenge conventional wisdom is not easy at all. Indeed, I struggled to publish each one of the papers that contradict the established climate narrative⁴. I still struggle to publish others which are being reviewed or have been rejected.

The attacks continued after publication of these papers. With “attack” I do not mean criticism, which is healthy and welcomed, as it contributes to improvement of papers and correction of possible errors. What I mean by “attack” is an attempt to block publication of a paper and to silence a voice that does not comply with the established narrative. I also include in “attack” an attempt to force a publisher to retract a published paper.

Such attacks on our papers are mentioned in a long discussion in Judith Curry’s blog⁵. However, these were the exceptions, as most of the criticisms of the papers in the blog were healthy. The extent of the discussion suggests that the papers raised wide interest. It can be regarded as an interesting case of post-publication crowd-reviewing, which our work withstood well. In addition, the discussion offered independent confirmation of our results,

⁴ My papers and other documents related to climate can be accessed from my web site, <https://www.itia.ntua.gr/en/search/?authors=koutsoyiannis&tags=climate>. The full list of my journal papers can be accessed at <https://www.itia.ntua.gr/en/byauthor/Koutsoyiannis/0/>. The full list of my works can be accessed at <http://www.itia.ntua.gr/en/search/?title=&authors=koutsoyiannis>.

⁵ <https://judithcurry.com/2023/09/26/causality-and-climate/>.

based on a different methodology, and shed light on the physical processes that justify the causality direction found.

I have gathered all comments (about 1000, 18% of which were my own replies) into the following 370-page “book”:

6. A. Christofides, D. Koutsoyiannis, C. Onof, and Z. W. Kundzewicz, 2023. *Causality, Climate, Etc.* Climate Etc. (Judith Curry's blog), [doi: 10.13140/RG.2.2.21608.44803](https://doi.org/10.13140/RG.2.2.21608.44803).

What follows is another example of an attack. It refers to the review of paper #5. The first version of the paper was submitted on 29 March 2024, after an invitation by the journal. It received three constructive reviews favouring its publication. The editor’s decision was major revision. I addressed all review comments and submitted a revised version on 25 May 2024, along with a detailed report with replies to review comments. All three original reviewers were satisfied with the way I addressed their comments and they recommended publication of the revised manuscript.

However, something unusual happened as two additional reviewers were involved, who tried to block the publication of my paper. Interestingly, the comments of these additional reviewers were focused on papers #1 – #4 and not on #5, the one under review, despite the fact that their comments were purported to be about paper #5. Using material from online attacks on the earlier papers, these hostile reviewers adopted the same tactics claiming that our methodology was inadequate or that our work contained errors.

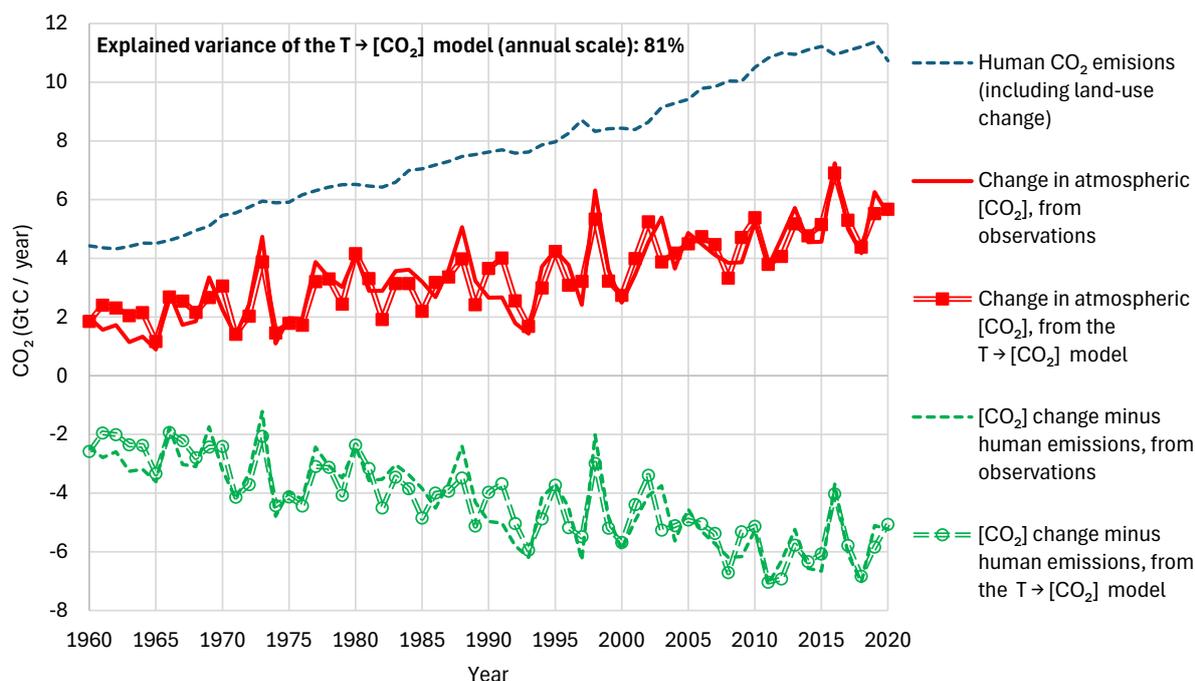
However, if errors were found, then we would either correct them or retract the papers. But not a single error was found. And none of the papers were retracted, despite the efforts of the attackers.

Eventually, paper #5 was accepted for publication, without further changes, based on the rebuttal of these review comments, which I copy in the next pages. In other words, the attacks failed. As explained in the next pages, normally I would not respond to these comments, but once I was forced to do, I thought that it would be fun to make this rebuttal public, as it also responds to earlier attacks.

The only notable change I made to the final paper is that I deleted the motto in its beginning. I explain why in the rebuttal. Yet, I used that motto in the present document as I think it is more relevant to this.

Thus, the next 41 pages is the part of my rebuttal report containing my responses to these two additional reviewers.

A side product of this rebuttal is the following graph, whose initial version is contained in the rebuttal report. The version immediately following below is somewhat improved and contains a detailed description of the graph, its use in the attacks, and its inappropriateness to assess the causality direction. The rebuttal contains detailed instructions how to reproduce the graph.



This graph was used in several versions—but, of course, without the $T \rightarrow [\text{CO}_2]$ model curves—as the main weapon (sort of “Scientific Sword Excalibur”) in the attacks to our causality papers. It seems to have first appeared (for years 1960-2005) in Cawley’s (2011) paper (in *Energy and Fuels*). Cawley also used his version in his attack in *PubPeer*, with the (admitted) purpose to force retraction of our papers in *Proceedings of The Royal Society A*. It was also used in a blog with the same purpose. Subsequently, Engelbeen used his own version to dispute our papers in Judith Curry’s blog (Climate Etc.). He uploaded/linked his version eight times in the same blog discussion. More recently, an anonymous reviewer of my 2024 paper in *Mathematical Biosciences and Engineering* used Cawley’s original version to attack the paper under review, aiming at its rejection. The above version of the graph (initially produced for my rebuttal to that review) contains also the results of our $T \rightarrow [\text{CO}_2]$ toy model—Equation (10) in our *Sci* (2023) paper. Notice the impressive percentage of model’s explained variance (81% on annual scale, becoming 99.9% on monthly scale). Despite being used as “Sword Excalibur”, this graph, in its original versions, says nothing about causality. It does not contain any information about time precedence. Only my above version says something: that the $T \rightarrow [\text{CO}_2]$ potential causality is consistent with the data.

Produced by Demetris Koutsoyiannis

Key for the pages that follow:

| Review comment.

Response.

Quotation from manuscript.

Quotation from other documents.

Appendix B: Response to additional reviewers' comments of Round 2 on "Stochastic assessment of temperature – CO₂ causal relationship in climate from the Phanerozoic through modern times"

by Demetris Koutsoyiannis

Reviewer 4

R2-4.1. The author proposes a method for studying causality (X causes Y) based on the regression of one variable (Y) on lagged values of the other (X). On page 22, the author acknowledges that there are other methods for studying causality, such as "Granger causality."

Thanks for the summary. However, it is not accurate. I clearly state that I study the causality between processes, not variables. I start Section 3.2, *Stochastic methodology*, as follows:

The stochastic methodology used here for identifying potential causal links was developed in [1,2,3] and is based on the impulse response function (IRF) between two stochastic processes $\underline{x}(t), \underline{y}(t)$, denoted as $g(h)$ where h denotes time lag, based on the convolution...

This simple statement clarifies that: (a) I use a new methodology, based on stochastic processes, and (b) that the methodology was already peer-reviewed and published, as seen in the above three references. It was also extensively reviewed at a post-publication phase [4]. Quoting from an even newer paper (Koutsoyiannis, 2024 [5]):

The latter study [3] raised wide interest and was subsequently discussed in several forums, among which most representative is Judith Curry's blog [6]. With its about 1000 comments, 18% of which were replies by the principal author, this extended discussion, equivalent in length to a book of 370 pages [4], can be regarded as an interesting case of post-publication crowd reviewing, in which the study withstood.

In addition, this post-publication crowd reviewing offered independent confirmation of our results based on a different methodology (using cross-spectral analysis). Based on these facts, I think that it is justified not to repeat in the present paper the full scientific details of the method, the mathematical and factual proofs, and the comparisons with other methods. Any reviewer or reader can find all those in the given references.

As the reviewer correctly notes, I briefly refer to other methods in the following statement:

As detailed in [2,3], there exist a variety of other methods for estimating IRF and for inferring causality but our method differs conceptually and computationally from them, including from the so-called "Granger causality" [7,8] and the framework proposed by Pearl and collaborators [9-11]

I clarify that this statement refers to (a) other methods for estimating IRF, (b) the so-called "Granger causality" (which, despite its name, does not identify causality but potential for prediction) and (c) the framework proposed by Pearl and collaborators. All these are out of the scope of the present paper, but the interested reviewer or reader may find any detail of these methods in the 2+2 references given above or may refer to our own papers

(Koutsoyiannis et al. [1-3]), as well as to our earlier paper, Koutsoyiannis and Kundzewicz [12], also referred to in my new paper.

R2-4.2. I am more familiar with the latter option than with the method proposed by the author. In Granger causality, two models are studied: one involving the regression of y on its past values, and another involving the regression of y on its past values as well as the past values of X . Subsequently, the significance of the coefficients associated with the lagged values of X is examined. Specifically, it is tested whether including X improves the prediction of y or if Y 's past values alone are sufficient for prediction.

I understand that the reviewer is more familiar with Granger's method, as it is an old one (from 1969) while my colleagues' and my methodology is new. I agree with the reviewer when she/he says "*it is tested whether including X improves the prediction of y or if Y 's past values alone are sufficient for prediction*". Our framework has many differences with Granger's method, including this one (copied from Koutsoyiannis et al. [1]):

A second difference is that our focus is upon maximizing not the predictability per se, but the lucidity in identifying the (potentially causal) relationship between two processes \underline{x}_τ and \underline{y}_τ . This can be seen by comparing Granger's expression in equation (2.11) with our expression in equation (3.20). To estimate y_τ , the former includes terms y_i for times earlier than τ while the second does not. Such terms may increase predictability but say nothing about a potentially causal relationship between the two processes \underline{x}_τ and \underline{y}_τ ; rather, they may obscure that relationship, as autocorrelation is by definition symmetric in time.

In other words, as identification of causality (instead of improvement of predictability) is concerned, the inclusion of term y_i for earlier times obscures (rather than improves) the performance.

I clarify that I appreciate Granger's method (originally developed for econometrics) and by no means do I want to devalue it. It has been successfully applied in several fields but also misused. Granger himself was aware of its misuses as in his Nobel Lecture [13] he stated: "*Of course, many ridiculous papers appeared.*"

R2-4.3. This methodology also has its weaknesses, such as determining the appropriate lags for the regression and including lags for variable X that differ from those in the regression of Y on itself.

I agree. Our method is much stronger in determining lags.

R2-4.4. Another approach involves modeling each series using a model that, when filtering the series, produces another with white noise structure, and calculating the cross-correlation between the two residual series. Depending on the location (right or left of 0) where significant cross-correlation values appear, causality in one direction or the other is indicated.

We had applied the method that seems to be preferred by the reviewer in our earlier paper (Koutsoyiannis and Kundzewicz [12]). Copying from Section 4.1 of that paper:

Yet, we can define a dominant direction of causality based on the time lag η_1 maximizing cross-correlation. Formally, η_1 is defined for a specified ν as

$$\eta_1 := \arg \max_{\eta} |r_{\underline{x}\underline{y}}(\nu, \eta)| \quad (13)$$

We can thus distinguish the following three cases:

If $\eta_1 = 0$, then there is no dominant direction.

If $\eta_1 > 0$, then the dominant direction is $\underline{x}_\tau \rightarrow \underline{y}_\tau$.

If $\eta_1 < 0$, then the dominant direction is $\underline{y}_\tau \rightarrow \underline{x}_\tau$.

Justification and further explanations of these conditions are provided in Appendix A.3.

The interested reviewer or reader may see Appendix A.3 of that paper (Koutsoyiannis and Kundzewicz [12]) for details.

The above approach, which seems to be preferable by the reviewer, was our initial attempt in trying to identify causality. In our later works (Koutsoyiannis et al. [1-3]) we substantially advanced the initial methodology and in the present paper I use the newer and more advanced methodology.

The reviewer may infer the superiority of the new methodology from the fact the old methodology is merely a (non-realistic) special case of the new methodology, in which the impulse response function is reduced to a Dirac delta function. This is clearly stated in the beginning of Section 3.2:

The stochastic methodology used here for identifying potential causal links was developed in [1,2,3] and is based on the impulse response function (IRF) between two stochastic processes $\underline{x}(t), \underline{y}(t)$, denoted as $g(h)$ where h denotes time lag, based on the convolution:

$$\underline{y}(t) = \int_{-\infty}^{\infty} g(h)\underline{x}(t-h)dh + \underline{v}(t) \quad (3)$$

where $\underline{v}(t)$ is another stochastic process representing the part that is not explained by the causal link. Notice that we use the Dutch notational convention, in which stochastic variables and processes are underlined, while common variables and functions are not.

To see that the function $g(h)$ is the impulse response function (IRF) of the system $(\underline{x}(t), \underline{y}(t))$, we set $\underline{v}(t) \equiv 0$ and $\underline{x}(t) = \delta(t)$ (the Dirac delta function, representing an impulse of infinite amplitude at $t = 0$ and attaining the value 0 for $t \neq 0$), and we readily get $\underline{y}(t) = g(t)$. On the other hand, if we set $g(h) = b \delta(h - h_0)$ (with constant b and h_0), which means that the IRF is zero for every lag except for the specific lag h_0 , then equation (1) becomes $\underline{y}(t) = b\underline{x}(t - h_0) + \underline{v}(t)$. This special case is equivalent to simply correlating $\underline{y}(t)$ with $\underline{x}(t - h_0)$ at any time instance t . It is easy to find (cf. linear

regression) that in this case the multiplicative constant b is the correlation coefficient of $\underline{y}(t)$ and $\underline{x}(t - h_0)$ multiplied by the ratio of the standard deviations of the two processes. In general, however, we expect that the actual $g(h)$ is not a Dirac delta function but a continuous one over some domain. Thus, the IRF is a much more powerful tool than correlation, as it integrates the correlations in the entire spectrum of lags.

R2-4.5. I have serious doubts that the proposed method detects causality in the aforementioned sense.

I believe it is healthy (and should be the rule in science) to have doubts and express them. Since we have published our methodology in three peer-reviewed journal papers, and also presented it in conferences, I invite the reviewer to peruse the methodology and independently test it, based on her/his doubts.

R2-4.6. The results presented are quantified numerically without a study of the significance of these values. For example, equation (5) clearly quantifies whether the regression on X eliminates information about Y or not. If the two variances are similar (coefficient e close to 0), it indicates that X does not cause Y . However, a value of coefficient e close to 1 indicates the opposite. No test is proposed to contrast these hypotheses. In this sense, the values that appear, for example, in Table 1 are not informative. In the "Explained Variance Causal" column; is the value 0.62 different from zero or is the value 0.11 different from zero, or are both different from zero?

The reviewer is right that a value of e close to 0 indicates nonexistence of causality and a value close to 1 indicates the opposite (i.e. potential causality). Apparently, a value 0.62 provides more evidence than a value of 0.11. This is something common in statistical methods of inference by induction, as opposite to deduction. Giving a significance level of how this value differs from zero, does not make the method deductive. It remains inductive and inferior than before, because it became affected by an arbitrary choice of a significance level, as well as by several assumptions underlying the hypothesis. For example, in her/his comment R2-4.8 below, the reviewer correctly points out the effect of intrinsic correlation, which most statistical tests disregard, thus making wrong inference.

I do not know how familiar the reviewer is with new developments casting doubts on the usefulness of significance testing of "difference from zero". The following extract from Iliopoulou and Koutsoyiannis [14] provides a summary; it refers to statistical testing of trends in hydrology, but it is also relevant to other geophysical disciplines and other types of tests:

The most established technique to evaluate fitted trends is statistical hypothesis testing, i.e. a statistical inference technique that estimates the probability of an outcome as far from what is expected as the observed under the assumption that the null hypothesis is true (Gauch, 2003 [15]). The latter is known as the p -value and is compared to predefined significance levels, in order to reject or not the null hypothesis. This is a scientific method for model evaluation, which has been in part misused. For instance, its misuse in hydrology has been showcased by seminal studies (e.g., Cohn and Lins, 2005 [16]; Koutsoyiannis and Montanari, 2007 [17]; Serinaldi et al., 2018 [18]) which have established the fact that for hydrological, non i.i.d. data the null hypothesis, which tacitly contains independence, is a priori wrong, and its rejection, if correctly interpreted,

should point out to the wrong independence assumption. Still, the common practice has been to misinterpret outcomes in favour of trends. Part of the statistician community argues against the concept of significance testing (Nuzzo, 2014 [19]; Wasserstein and Lazar, 2016 [20]; Amrhein and Greenland, 2018 [21]; Trafimow et al., 2018 [22]; Wasserstein et al., 2019 [23]), with the main critique summarized in the statement of the American Statistical Association that “the widespread use of ‘statistical significance’ (generally interpreted as ‘ $p \leq 0.05$ ’) as a license for making a claim of a scientific finding (or implied truth) leads to considerable distortion of the scientific process” (Wasserstein and Lazar, 2016 [20]).

In particular, the seminal paper Cohn and Lins [16] explains the dramatic impact on statistical inference, based on significance testing, of the model assumed for the process studied (or the inappropriateness thereof, e.g. the neglect of stochastic dynamics such as Long-Term Persistence). I highlight the following phrase from this paper: “*In changing from one test to another, 25 orders of magnitude of significance vanished.*” Additional information can be found in my recent book “*Stochastics of Hydroclimatic Extremes*” [24], where I explain theoretically and with examples that a time series is not a sample and that statistical tests and their related significance are not valid when dealing with time series. Instead, we need advanced Monte Carlo techniques, information of which I give in chapter 7 of the book. I have tried to make this clear more than 20 years ago (Koutsoyiannis, 2003 [25]).

That said, it is self-evident that if the reviewer or any interested reader believes that a statistical test is relevant, she/he may feel free to develop one and publish it. My colleagues and I have published all mathematical details of the methodology in Koutsoyiannis et al. [1-3] and any interested reader may retrieve them from there in order to develop her/his test.

But clearly this is totally out of the scope of the current paper. I have done a lot of work in the present paper—but on another scope as seen in its title. Its length is already 50 pages. I hope the reviewer and any reader would accept the fact that the scope of the paper is that described by the title, the abstract and the introduction of the paper and not any other stuff that I have studied in earlier papers (jointly with other coauthors) or I do not think it deserves studying.

R2-4.7. The restriction (6), where all g_i are positive, is very restrictive.

Again this is something that has been explained in my earlier publications. Quoting from Section 3.3 of Koutsoyiannis et al. [1]:

In contrast to Granger’s analysis of causality (...), which treats the processes in discrete time by definition, here we treat them in continuous (i.e. natural) time, and we only convert them to discrete time for estimation purposes. If we think of the processes in natural time, we understand that a causality relationship is not an instantaneous one. In other words, if $\underline{x}(t')$ affects $\underline{y}(t)$, where $t' < t$, it is reasonable to assume that, for small h , $\underline{x}(t' \pm h)$ will also affect $\underline{y}(t)$. Therefore, the IRF, $g(h)$, is not a Dirac delta function, but one with some domain, $\mathbb{h} \subseteq \mathbb{R}$, of nonzero (and potentially infinite) measure, where $g(h) \neq 0$ for $h \in \mathbb{h}$. It is also reasonable to assume that $g(h)$ is a continuous function and has the same sign for all $h \in \mathbb{h}$. The latter can be justified as follows. If $\underline{x}(t')$ is positively correlated with $\underline{y}(t)$, then it is reasonable that $\underline{x}(t' \pm h)$ are also positively correlated with $\underline{y}(t)$. Without loss of generality, in what follows we will assume that $g(h) \geq 0$ for $h \in \mathbb{h}$

(if it were $g(h) \leq 0$, we would reflect $\underline{x}(t)$, i.e. replace it with $-\underline{x}(t)$, and hence $g(h)$ would also be reflected becoming nonnegative).

Here we clarify that the problem of identifying causality is different from that of recovering the full system dynamics. The former and not the latter, is the scope of our study. We note that, while there exist oscillatory nonlinear systems, in which the sign of $g(h)$ could alternate, we avoid subsuming them under the causality notion, particularly when causality is inferred from data in an inductive manner. This choice is consistent with Cox's (1992 [26]) conditions for causality, according to which the effect "*shows a monotone relation with 'dose'*" of the cause. Here we note that in our framework the "dose" is not regarded as an instantaneous event, but one with some time span (see details in Supplementary Information, section SI1.2).

R2-4.8. The method for determining h_c on page 21 is based on the calculation of cross-correlation on the series X and Y. This cross-correlation is affected by the intrinsic correlation of each series, and the significance study of each value at each lag can be erroneous.

I agree. The significance of this cross-correlation value can be erroneous. That is the reason why I do not calculate at all and did not give the significance level in the paper (see also my reply to comment R2-4.6 above). For the same reason, I give the emphasis on the other two indices, the mean (time average), μ_h , and the median, $h_{1/2}$, of the sequence g_j . Yet I additionally give the value of the cross-correlation (without its significance), for the completeness of the presentation. As mentioned in section 3.2 for μ_h and $h_{1/2}$:

extensive analyses in [1] showed that their estimation is quite robust.

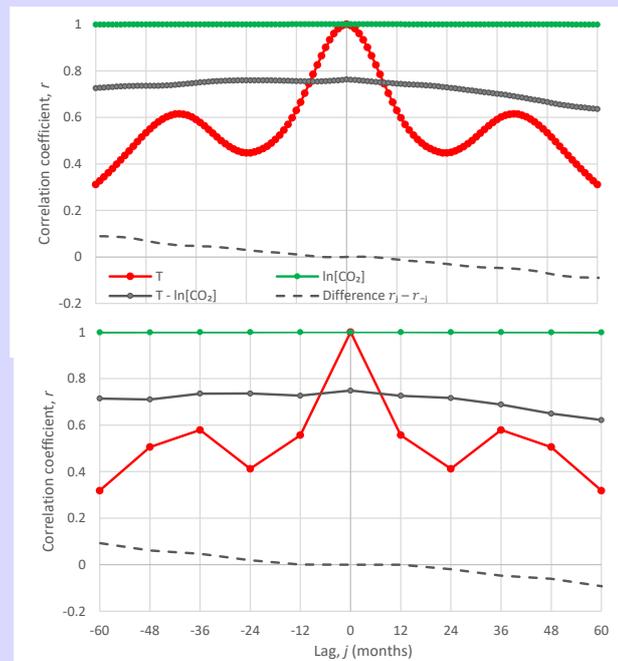
R2-4.9. Apply the "Granger Causality" method to the data and compare it with your method.

I must thank the reviewer for reminding me of my youth, some 40 years ago, when I was a seaman in the Greek Navy for 25 months. This is because this last comment sounds like a command that I must execute.

So: **Yes Sir!** I have already executed it four years ago. Quoting from Section 5.1 of Koutsoyiannis and Kundzewicz [12]:

Somewhat more informative is Figure 9, which depicts lagged cross-correlations of the two processes, based on the methodology in Section 4.1 but without differencing the processes. Specifically, Figure 9 shows the cross-correlogram between UAH temperature and Mauna Loa $\ln[\text{CO}_2]$ at monthly and annual scales; the autocorrelograms of the two processes are also plotted for comparison. In both time scales, the cross-correlogram shows high correlations at all lags, with the maximum attained at lag zero. This does not hint at a direction. However, the cross-correlations for negative lags are slightly greater than those in the positive lags. Notice that to make this clearer, we have also plotted the differences $r_j - r_{-j}$ in the graph. This behaviour could be interpreted as supporting the causality direction $[\text{CO}_2] \rightarrow T$. However, we deem that the entire picture is spurious as it is heavily affected by the fact that the autocorrelations are very high and, in particular, those of $\ln[\text{CO}_2]$ are very close to 1 for all lags shown in the figure.

In our investigation, we also applied the Granger test on these two time series in both time directions. To calculate the p -value of the Granger test, we used free software (namely the function GRANGER_TEST [27,28]). It appears that in the causality direction $[\text{CO}_2] \rightarrow T$, the null hypothesis is rejected at all usual significance levels. The attained p -value of the test is 1.8×10^{-7} for one regression lag ($\eta = 1$), 1.8×10^{-4} for $\eta = 2$, and remains below 0.01 for subsequent η . By contrast, in the direction $T \rightarrow [\text{CO}_2]$, the null hypothesis is not rejected at all usual significance levels. The attained p -value of the test is 0.25 for $\eta = 1$, 0.22 for $\eta = 2$, and remains above 0.1 for subsequent η .

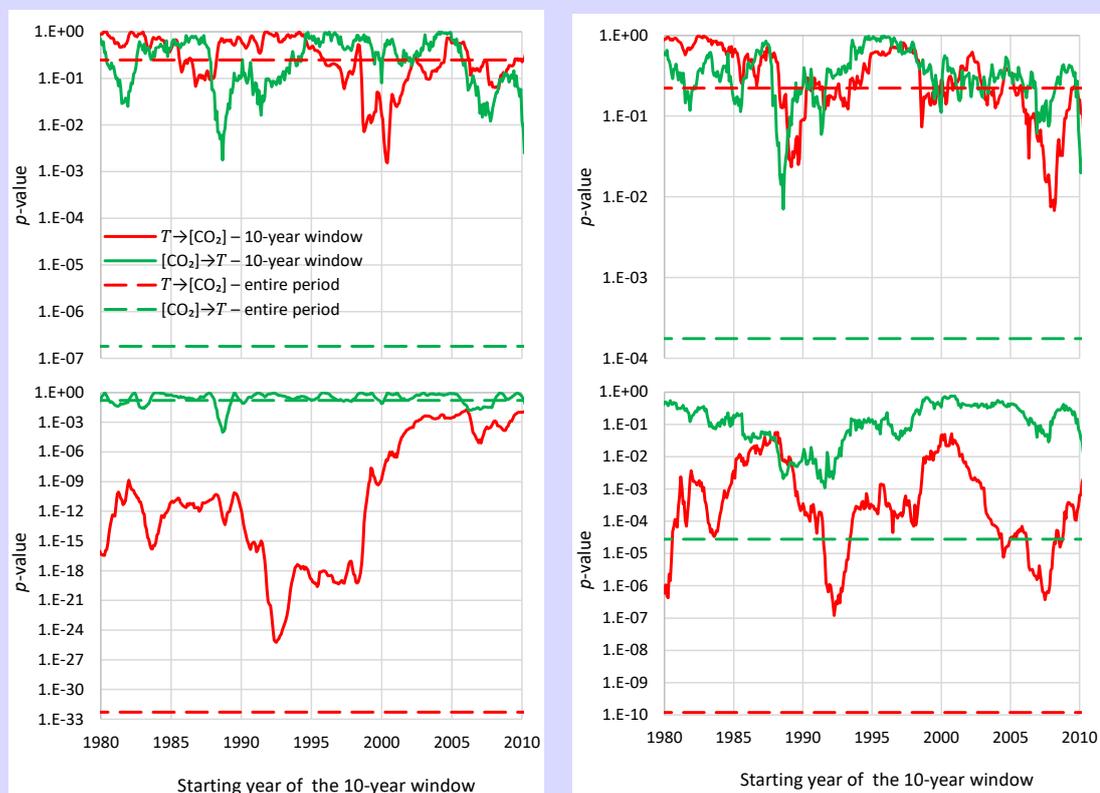


[Figure R2-4.1; reproduced from Koutsoyiannis and Kundzewicz [12]; original caption follows]

Figure 9. Auto- and cross-correlograms of the time series of UAH temperature and logarithm of CO_2 concentration at Mauna Loa.

Therefore, one could directly interpret these results as unambiguously showing one-way causality between the total greenhouse gases and temperature and, hence, validating the consensus view that human activity is responsible for the observed rise in global temperature. However, these results are certainly not unambiguous and, most probably, they are spurious. To demonstrate that they are not unambiguous, we have plotted, as shown in the upper panels of Figure 10, the p -values of the Granger test for moving windows with a size of 10 years for number of lags $\eta = 1$ and 2. The values for the entire length of time series, as given above, are also shown as dashed lines. Now the picture is quite different: each of the two directions appear dominating (meaning that the attained significance level is lower in one over the other) in about equal portions of the time. For example, for $\eta = 2$, the $T \rightarrow [\text{CO}_2]$ dominates over $[\text{CO}_2] \rightarrow T$ for 58% of the time. The attained p -value for direction $T \rightarrow [\text{CO}_2]$ is lower than 1% for 1.4% of the time, much higher than in the opposite direction (0.3% of the time). All of these observations favour the $T \rightarrow [\text{CO}_2]$ direction.

To show that the results are spurious and, in particular, affected by the very high autocorrelations of $\ln[\text{CO}_2]$ and, more importantly, by its annual cyclicity, we have “removed” the latter by averaging over the previous 12 months. We did that for both series and plotted the results in the lower panels of Figure 10. Here, the results are stunning. For both lags $\eta = 1$ and 2 and for the entire period (or almost), $T \rightarrow [\text{CO}_2]$ dominates, attaining p -values as low as in the order of 10^{-33} . However, we will avoid interpreting these results as unambiguous evidence that the consensus view (i.e., human activity is responsible for the observed warming) is wrong. Rather, what we want to stress is that it is inappropriate to draw conclusions from a methodology which is demonstrated to be so sensitive to the used time windows and data processing assumptions. In this respect, we have included this analyses in our study only (a) to show its weaknesses (which, for the reasons we explained in Section 4.2, we believe would not change if we used different statistics or different time series) and (b) to connect our study to earlier ones. For the sake of drawing conclusions, we contend that our full methodology in Sections 4.1 and 4.3 is more appropriate. We apply this methodology in Section 5.2.



[Figure R2-4.2; reproduced from Koutsoyiannis and Kundzewicz [12]; original caption follows]

Figure 10. Plots of p -values of the Granger test for 10-year-long moving windows for the monthly time series of UAH temperature and logarithm of CO_2 concentration at Mauna Loa for number of lags (**left**) $\eta = 1$ and (**right**) $\eta = 2$. The time series used are (**upper**) the original and (**lower**) that obtained after “removing” the periodicity by averaging over the previous 12 months.

Also, quoting from Section 5.2 of the same paper (Koutsoyiannis and Kundzewicz [12]), referring to the differenced time series:

While, as explained in Sections 4.2 and 5.1, the Granger test has weaknesses that may not help in drawing conclusions, for completeness and as a confirmation, we list its results here:

- For the monthly scale and the causality direction $[\text{CO}_2] \rightarrow T$, the null hypothesis is not rejected at all usual significance levels for lag $\eta = 1$ and is rejected for significance level 1% for $\eta = 2-8$, with minimum attained p -value 1.8×10^{-4} for $\eta = 6$.
- For the monthly scale and the causality direction $T \rightarrow [\text{CO}_2]$, the null hypothesis is rejected at all usual significance levels for all lags η , with minimum attained p -value 2.1×10^{-8} for $\eta = 7$.
- For the monthly scale, the attained p -values in the direction $T \rightarrow [\text{CO}_2]$ are always smaller than in direction $[\text{CO}_2] \rightarrow T$ by about 4 to 5 orders of magnitude, thus clearly supporting $T \rightarrow [\text{CO}_2]$ as dominant direction.
- For the annual scale with fixed year specification and the causality direction $[\text{CO}_2] \rightarrow T$, the null hypothesis is not rejected at all usual significance levels for any lag η , thus indicating that this causality direction does not exist.
- For the annual scale with fixed year specification and the causality direction $T \rightarrow [\text{CO}_2]$, the null hypothesis is not rejected at significance level 1% for all lags $\eta = 1-6$, with minimum attained p -value 5% for lag $\eta = 2$, thus supporting this causality direction at this significance level.
- For the annual scale with fixed year specification, the attained p -values in the direction $T \rightarrow [\text{CO}_2]$ are always smaller than in direction $[\text{CO}_2] \rightarrow T$, again clearly supporting $T \rightarrow [\text{CO}_2]$ as the dominant direction.

We note that the test cannot be applied for the sliding time window case and, hence, we cannot provide results for this case.

[...]

In brief, all above confirm the results of our methodology that the dominant direction of causality is $T \rightarrow [\text{CO}_2]$.

Reviewer 5

Note: The reviewer's citations, originally denoted as [1] etc., have been changed to [[1]] etc. so as to be distinguished from the citations of my rebuttal report. Those citations that refer to my own paper, which the reviewer also denotes as [1] etc., have been changed to {1} etc., again to be distinguished from the citations of my rebuttal report.

R2-5.1. This paper investigates the use of an existing statistical method for causal inference to investigate the causality linking CO2 and temperature. Sadly the inference that for the instrumental data T causes CO2 is demonstrably incorrect. The error is due to the differencing of the time series, which decouples the long term linear trend entirely from the analysis. The finding that T causes CO2 in paleoclimate is overly simplistic, but entirely

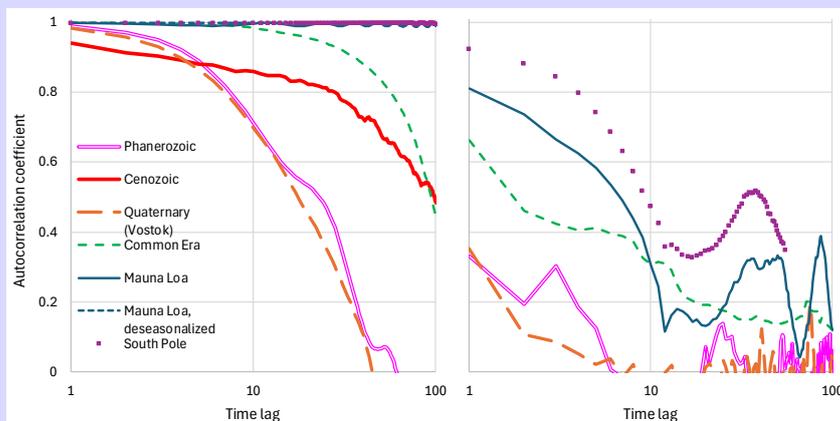
uncontraversial. There is no clear statement of the mathematical novelty of this paper (the moving average analysis of the instrumental data? However that is a relatively minor contribution and the results are unreliable because of the differencing). I see no scope for the paper being modifiable to a state where it warrants publication.

I understand that the reviewer does not want my paper to be published. This is clear in the last sentence, *“I see no scope for the paper being modifiable to a state where it warrants publication”*.

But it is also clear that the reviewer’s critiques do not apply to the present paper. They apply to papers that have already published (Koutsoyiannis and Kundzewicz [12]; Koutsoyiannis et al. [1-3]). For *“the differencing of the time series”* which the reviewer regards to be an error has been thoroughly explained and justified in all four papers. In the present paper I also apply the method with non-differenced series where applicable. Apparently, the reviewer has not seen it in the paper as she/he seems to have consulted several texts that (unsuccessfully) have attacked our earlier papers (not the present one).

The reviewer’s claim that *“the results are unreliable because of the differencing”* is totally wrong. Only its negation is right, i.e. *“the results are **reliable** because of the differencing”* or *“the results would be unreliable **without** the differencing”*. As shown in Koutsoyiannis and Kundzewicz [12] and reproduced above in my reply to comment R2-4.9 (by Reviewer #4), when the autocorrelations are close to 1 for the lags of interest, without differencing the *“results are certainly not unambiguous and, most probably, they are spurious”*. In the present paper, the autocorrelations are close to 1 at all lags of interest for the instrumental data, as seen in the following part of section 4.1 and the Figure 9 of the paper:

Figure 11 shows the empirical autocorrelation functions of the $[CO_2]$ series, original and differenced. In the instrumental series, the autocorrelations are almost 1, even for lags as high as 100. This prohibits any inference from the original series. However, their differenced series have reasonable positive autocorrelations, which make inference possible. The proxy series have high autocorrelations at small lags, but reasonable ones at large lags. For those we examined both the original and the differenced series, provided that the latter are positive.



[Figure R2-5.1; original caption follows]

Figure 11 Autocorrelation functions of $[CO_2]$ series: **(left)** original; **(right)** differenced. The differenced series autocorrelation for Cenozoic is not plotted as it is mostly negative. The time lag is in discrete time j , i.e. dimensionless, and, to make it dimensional, we should multiply by the time step Δ of each series ($h = j\Delta$).

The reason that autocorrelations close to 1 do not enable inference on causality, has been explained in several places of the earlier works. As an example, below I am reproducing the section SI2.2 from the Supplementary Information of Koutsoyiannis et al. [2]:

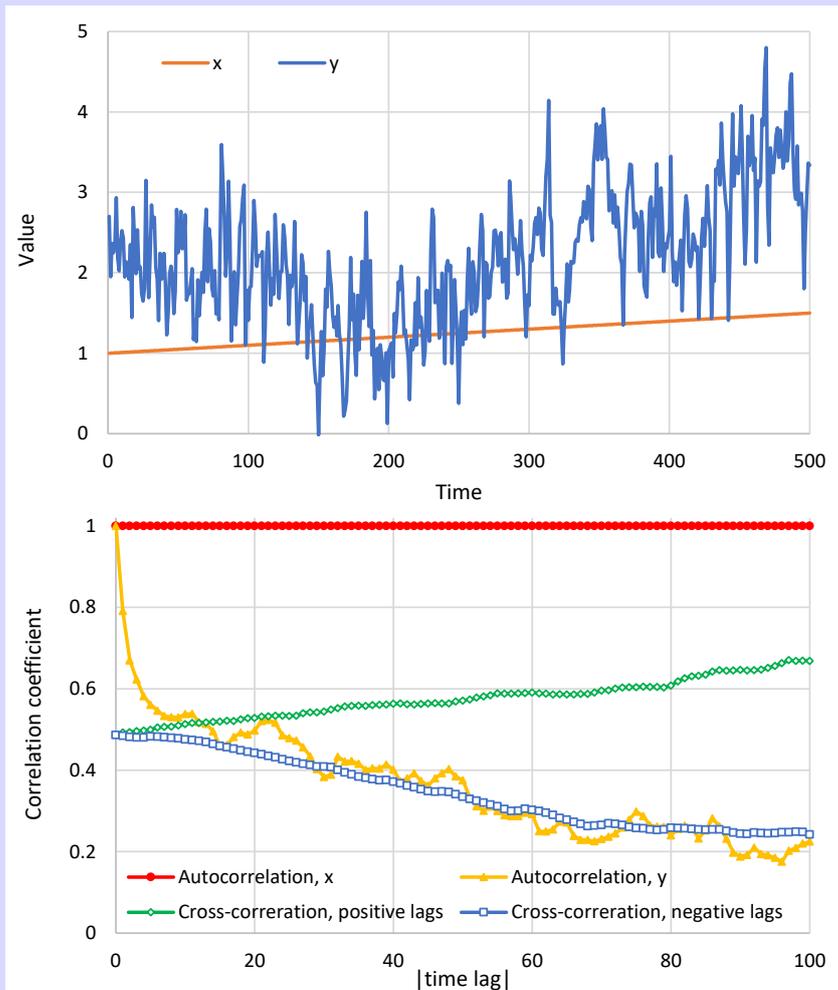
SI2.2 On high autocorrelations and spurious IRF estimates

As stated in the main papers (Koutsoyiannis et al., 2022a,b [1,2]), high autocorrelation results in increased estimation uncertainty and may even result in spurious causality claims. To illustrate this, we devise a synthetic example, in which the processes \underline{x}_τ and \underline{y}_τ are, by construction, independent of each other and with high autocorrelation.

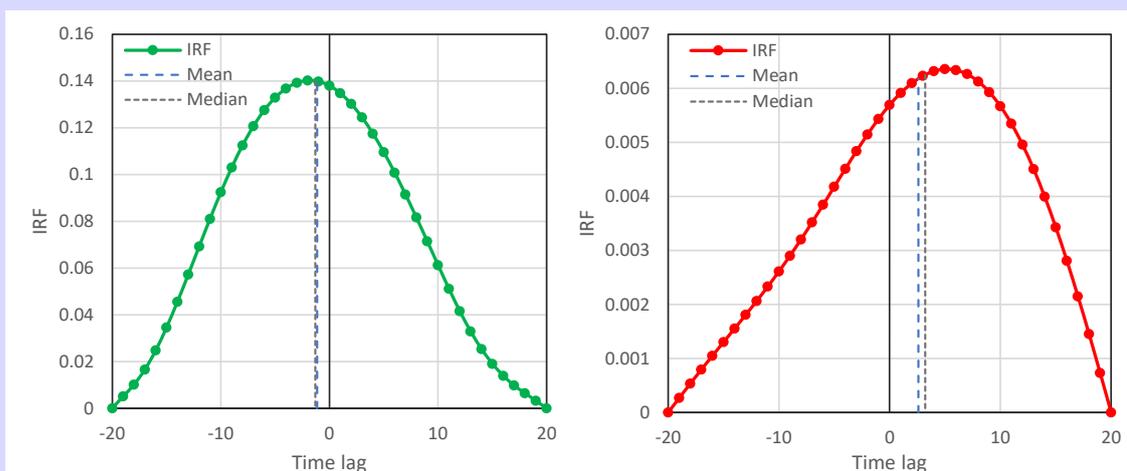
Specifically, two time series x_τ and y_τ , each of length 500, are generated independently from each other. The time series x_τ is constructed by the deterministic rule $x_\tau = 1 + 0.001\tau$. If its values are treated statistically, the resulting autocorrelation estimate is constant for all lags, $\hat{r}_{xx}(h) = 1$. The time series y_τ is generated from a Hurst-Kolmogorov process with Gaussian distribution and with a high Hurst parameter, $H = 0.95$, reflecting strong long-range dependence (LRD). By now, it is well known (e.g. Cohn and Lins, 2005 [16]; Koutsoyiannis, 2013 [29]) that realizations of processes with LRD look “trendy” even though the processes are stationary. This is evident in Figure SI2.5 (upper), which depicts both time series. Their auto- and cross-correlations, estimated using standard statistical estimators, are shown in Figure SI2.5 (lower). Interestingly, while by construction the cross correlations are $r_{yx}(h) = 0$ for any lag h , their estimates $\hat{r}_{yx}(h)$ appear very high, i.e., 0.46 ± 0.21 in the plotted interval of lag h , $(-100,100)$.

Because of the high cross-correlations, if we estimate the IRF with the proposed method, as seen in Figure SI2.4, spurious Hen-or-Egg (HOE) causality is identified in both directions $\underline{x} \rightarrow \underline{y}$ and $\underline{y} \rightarrow \underline{x}$. The dominant causality direction appears to be $\underline{y} \rightarrow \underline{x}$ with mean lag $\mu_h = 2.6$, median lag $h_{1/2} = 3.2$ and explained variance ratio $e = 0.47$. All these are obviously invalid estimates (as there are no true lags in this case and the true value of the explained variance ratio is $e = 0$), even though the calculations are correct.

Naturally, a remedy in such spurious cases is to reduce the autocorrelations. This becomes possible if instead of the time series x_τ and y_τ we study the differenced time series $\Delta x_\tau := x_\tau - x_{\tau-1}$ and $\Delta y_\tau := y_\tau - y_{\tau-1}$. Taking the differences is definitely reasonable: if x_τ causes y_τ , then a change in x_τ should cause a change in y_τ . In our example, we will have constant $\Delta x_\tau = 0.001$ and hence the cross-covariances would be zero, which will exclude any causality claim.



[Figure R2-5.2; reproduced from Koutsoyiannis et al. [3]; original caption follows]
Figure SI2.3 (upper) Time series of the synthetic example described in the text. **(lower)** Auto- and cross-correlation function estimates for the two time series.



[Figure R2-5.3; reproduced from Koutsoyiannis et al. [3]; original caption follows]
Figure SI2.4 IRFs for the synthetic example of spurious IRF estimation due to high autocorrelation of Figure SI2.3 for causality directions (left) $\underline{x} \rightarrow \underline{y}$ and (right) $\underline{y} \rightarrow \underline{x}$. For the estimated IRFs the number of weights is $2J + 1$ with $J = 20$.

If the reviewer thinks that the above four papers, which were prepared jointly with other colleagues, are "*demonstrably incorrect*" then I invite her/him to demonstrate that with mathematical and logical arguments, and publish her/his demonstration in a scientific journal. This would be more constructive than trying to block the publication of my new paper as an anonymous reviewer by repeating what she/he saw in attacks of earlier papers on the internet (some of which he also cites; see comment R2-5.14 below).

In addition, publishing her/his demonstrations would give the opportunity of a public dialogue.

R2-5.2. The paper is perhaps overlong, but the formatting, use of language and structure are all satisfactory.

I welcome this remark.

R2-5.3. Abstract

The fundamental problem with this paper is evident in the final sentence of the abstract:

"These results contradict the conventional wisdom, according to which the temperature rise is caused by [CO₂] increase."

Firstly, this is not merely "conventional wisdom", it is the conclusion of over a century of scientific research (known as the "enhanced greenhouse effect" - EGHE), including a well-understood causal mechanism, and compelling experimental and observational evidence of the process as a whole, and its component parts. If a physics-free, purely statistical method, contradicts such strong scientific finding, then it would be prudent to be deeply skeptical of the statistical method (which does not have the benefit of a causal mechanism or evidence for the component parts, or consistency with other scientific findings). In this case, the flaw in the statistical method is easily identified (differencing of the time series), it is an error that has been made on numerous occasions, e.g. Salby and Humlum et al. etc., it has also previously been drawn to the author's attention prior to the submission of the current paper.

From research on the EGHE and on the carbon cycle, we know that there are causal mechanisms in **both** directions: The EGHE means that as CO₂ levels rise, all things being otherwise equal, there will be a rise in global temperature, modified by climate feedbacks, such as water vapour feedback. However, multiple carbon cycle feedback mechanisms mean that changes in global temperatures will affect the level of atmospheric CO₂. Like the EGHE, there is also considerable research on carbon cycle feedback. It is the interplay between these forces that results in a dynamic equilibrium, that has kept the Earth's temperature and its atmospheric CO₂ reasonably stable until large scale anthropogenic emissions disturbed this equilibrium, coinciding with the industrial revolution. Any model of the relationship between global temperatures and atmospheric CO₂ that does not include these feedbacks is at variance with observed reality and should be discarded - it doesn't match what we observe to be the case.

Yes, "conventional wisdom" could be the result of over-century research. Longevity does not make it unconventional. Neither makes it correct. It is well known that Aristotle's geocentric system was conventional wisdom for about 18 centuries, on which conventional research of

its time was based. At the same time, Arisctarchus's heliocentric system has been rejected for 18 centuries. On the other hand, Aristotle's correct explanation of the Nile's floods was rejected for 21 centuries (Koutsoyiannis, and Mamassis [30]).

Given that conventional wisdom is not necessarily correct, I think I have the right to challenge it in a scientific context.

The statement cited by the reviewer from the abstract reflects the content of the paper and is 100% accurate. No modification is required.

Note that the statement does not say that conventional wisdom is wrong. It says that our results contradict conventional wisdom. If conventional wisdom is right, then we are wrong. This is likely but must be proven with mathematical and logical arguments using the scientific method. None of the attacks made so far, including the one by Reviewer #5, has any of these characteristics.

There may be "*physics-free, purely statistical methods*", but when applied to physical problems, statistical methods become parts of physics. Clockwise physics, without using probability and statistics, has been conventional wisdom for a couple of centuries but has proved to be weak and inadequate. Hence, stochastics has long ago been incorporated into physics. This occurred one century and a half ago, but admittedly, many of us, including this reviewer, are not updated on this fact yet and continue to contrast physics and statistics. Therefore, I am providing the following information in bulleted form (along with my apology for being didactic):

- Statistical physics (cf. Boltzmann, Gibbs, Planck) used the probabilistic concept of entropy (which is nothing other than a quantified measure of uncertainty defined within the probability theory) to explain fundamental physical laws (most notably the Second Law of thermodynamics), thus leading to a new understanding of natural behaviors and to powerful predictions of macroscopic phenomena. Atmospheric processes are explained by statistical physics in all respects (thermodynamic equilibrium, blackbody radiation, transport processes)
- Quantum theory (cf. Heisenberg) has emphasized the intrinsic character of uncertainty and the necessity of probability in the description of nature.
- Developments in numerical mathematics for applications in physics (cf. Metropolis) highlighted the effectiveness of stochastic methods in solving physical problems that are even purely deterministic, such as numerical integration in high-dimensional spaces and global optimization of non-convex functions (where stochastic techniques, e.g., stochastics-based evolutionary algorithms and simulated annealing, are in effect the only feasible solution in complex problems that involve many local optima).

This extends even beyond physics. Thus,

- Genetics (cf. Mendel) and evolutionary biology have emphasized the importance of stochasticity (e.g., in gametes fusion, selection and mutation procedures, and environmental changes) as a driver of evolution.

- Developments in mathematical logic, and particularly Gödel's incompleteness theorem, challenged the almightiness of deduction (inference by mathematical proof). This necessitates the use of induction in physical problems, whose theoretical basis is offered by the field of stochastics

It is completely untrue that in our papers we do not *"benefit of a causal mechanism or evidence for the component parts, or consilience with other scientific findings"*. Here I repeat the related parts from our earlier papers.

From Koutsoyiannis and Kundzewicz [12], Section 6:

The omnipresence of positive lags on both monthly and annual time scales and the confirmation by Granger tests reduce the likelihood that our results are statistical artefacts. Still, our results require physical interpretation which we seek in the natural process of soil respiration.

Soil respiration, R_s , defined to be the flux of microbially and plant-respired CO_2 , clearly increases with temperature. It is known to have increased in the recent years [31,32]. Observational data of R_s (e.g., [33,34]; see also [35]) show that the process intensity increases with temperature. Rate of chemical reactions, metabolic rate, as well as microorganism activity, generally increase with temperature. This has been known for more than 70 years (Pomeroy and Bowlus [36]) and is routinely used in engineering design.

The Figure 6.1 of the latest report of the IPCC [32] provides a quantification of the mass balance of the carbon cycle in the atmosphere that is representative of recent years. The soil respiration, assumed to be the sum of respiration (plants) and decay (microbes), is 113.7 Gt C/year (IPCC gives a value of 118.7 including fire, which along with biomass burning, is estimated to be 5 Gt C/year by Green and Byrne [37]).

We can expect that sea respiration would also have increased. Moreover, outgassing from the oceans must also have increased as the solubility of CO_2 in water decreases with increasing temperature [38,39]. In addition, photosynthesis must have increased, as in the 21st century the Earth has been greening, mostly due to CO_2 fertilization effects [40] and human land-use management [41]. Specifically, satellite data show a net increase in leaf area of 2.3% per decade [41]. The sums of carbon outflows from the atmosphere (terrestrial and maritime photosynthesis as well as maritime absorption) amount to 203 Gt C/year. The carbon inflows to the atmosphere amount to 207.4 Gt C/year and include natural terrestrial processes (respiration, decay, fire, freshwater outgassing as well as volcanism and weathering), natural maritime processes (respiration) as well as anthropogenic processes. The latter comprise human CO_2 emissions related to fossil fuels and cement production as well as land-use change, and amount to 7.7 and 1.1 Gt C/year, respectively. The change in carbon fluxes due to natural processes is likely to exceed the change due to anthropogenic CO_2 emissions, even though the latter are generally regarded as responsible for the imbalance of carbon in the atmosphere.

From Koutsoyiannis et al. [2], Section 3:

In other words, it is the increase of temperature that caused increased CO₂ concentration. Though this conclusion may sound counterintuitive at first glance, because it contradicts common perception (and for this reason we have assessed the case with an alternative parametric methodology in the Supplementary Information, section SI2.4, with results confirming those presented here), in fact it is reasonable. The temperature increase began at the end of the Little Ice Period, in the early 19th century, when human CO₂ emissions were negligible; hence other factors, such as the solar activity (measured by sunspot numbers), as well as internal long-range mechanisms of the complex climatic systems had to play their roles.

A possible physical mechanism for the [CO₂] increase, as a result of temperature increase, was proposed by Koutsoyiannis and Kundzewicz [12] and involves biochemical reactions, as, at higher temperatures, soil respiration, and hence natural CO₂ emissions, are increasing. In addition, as pointed out by Liu et al. [42] the influence of El Niño on climate is accompanied by large changes to the carbon cycle, with the pantropical biosphere releasing much more carbon into the atmosphere during large El Niño occurrences. Noticeably, in a very recent paper, Goulet Coulombe and Göbel [43] seem to confirm the finding by Koutsoyiannis and Kundzewicz [12], yet they deem it an *“apparently counterintuitive finding that GMTA [global mean surface temperature anomalies] explains a larger portion of the forecast error variance of CO₂ than vice versa”*. To *“resolve”* it, they *“explore a last avenue, that of using annual CO₂ emissions”*. However, using anthropogenic CO₂ emissions, which contribute only a small portion (3.8%) to the global carbon cycle (Koutsoyiannis [44]), as a principal variable is definitely less meaningful than using the atmospheric CO₂ concentration.

We believe that counterintuitive results, such as those about the causal link between temperature and CO₂ concentration conveyed in this paper, can indeed open up avenues of research. However, these avenues of research might not resolve the issue in a way compatible with what intuition dictates. In the history of science, such avenues were often created when established ideas were overturned by new findings.

The following extract from Koutsoyiannis et al. [3], Section 9 provides additional information and also disputes the Goulet Coulombe and Göbel [43] “avenue”:

In terms of the carbon cycle [...], several physical, chemical, biochemical and human processes are involved in it. The human CO₂ emissions due to the burning of fossil fuels have largely increased since the beginning of the industrial age. However, the global temperature increase began succeeding the Little Ice Period, at a time when human CO₂ emissions were very low. To cast light on the problem, we examine the issue of CO₂ emissions vs. atmospheric temperature further in the Supplementary Information, where we provide evidence that they are not correlated with each other. The outgassing from the sea is also highlighted sometimes in the literature among the climate-related mechanisms. On the other hand, the role of the biosphere and biochemical reactions is often downplayed, along with the existence of complex interactions and feedback. This role can be summarized in the following points, examined in detail and quantified in Appendix A1.

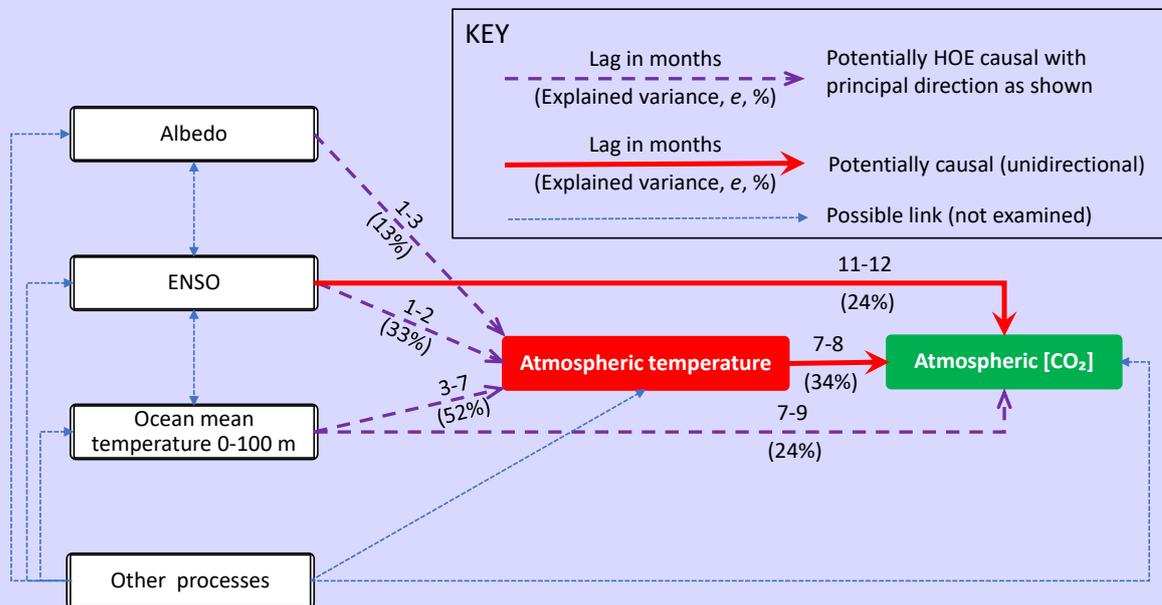
- Terrestrial and maritime respiration and decay are responsible for the vast majority of CO₂ emissions [45, Figure 5.12].
- Overall, natural processes of the biosphere contribute 96% to the global carbon cycle, the rest, 4%, being human emissions (which were even lower in the past [44]).
- The biosphere is more productive at higher temperatures, as the rates of biochemical reactions increase with temperature, which leads to increasing natural CO₂ emission [12].
- Additionally, a higher atmospheric CO₂ concentration makes the biosphere more productive via the so-called carbon fertilization effect, thus resulting in greening of the Earth [40,46], i.e., amplification of the carbon cycle, to which humans also contribute through crops and land-use management [41].

In addition to the biosphere, there are other factors that drive the Earth's climate in periodic and non-periodic way. Orbital parameters of Earth's revolution change quasi-cyclically in a multi-millennial scale (variations in eccentricity, axial tilt, and precession of Earth's orbit), as interpreted by Milanković [47-51], and changes in the orbit geometry influence the amount of insolation. The non-periodic drivers of the Earth's climate variability include volcanic eruptions and collisions with large extraterrestrial objects, e.g., asteroids. An important climate driver is water in its three phases [44]. Another apparent factor is solar activity (including solar cycles) and the solar radiation (im)balance on Earth (e.g., albedo changes; see [44] and Appendix A2). Notably, a recent study [52], by assessing 20 years of direct observations of energy imbalance from Earth-orbiting satellites, showed that the global changes observed appear largely from reductions in the amount of sunlight scattered by Earth's atmosphere.

ENSO and ocean heating, both of which affect temperature, are examined in Appendices A3 and A4, respectively. The results of Appendices A2–A4 are summarized in the schematic of Figure 13. Changes in all three examined processes, albedo, ENSO and the upper ocean heat, precede in time the changes in temperature and even more so those in [CO₂]. Generally, the time lags shown in Figure 13 complete a consistent picture of potential causality links among climate processes and always confirm the $T \rightarrow [\text{CO}_2]$ direction.

The examined processes in the Appendices are internal to the climatic system. Other processes affecting T , not examined here, could also be external (e.g., solar and astronomical [53,54] and geological [55-59]). Generally, in complex systems, an identified causal link, even though it gives some explanation of a phenomenon, raises additional questions, e.g., what caused the change in the identified cause, etc. In turn, causal links in complex systems may form endless sequences. For this reason, it is naïve to expect complete answers to problems related to complex systems or to assume that a complex system is in permanent equilibrium and that an external agent is needed to “kick” it out of the equilibrium and produce change. Yet the investigation of a single causal link between two processes, as is the focus of this paper, provides useful information, with possible significant scientific, technical, practical, epistemological and philosophical

implications. These are not covered in this paper. Readers interested in epistemological and philosophical aspects of causality are referred to Koutsoyiannis et al. [1], while those interested in the perennial changes in complex systems are referred to Koutsoyiannis [60,61].



[Figure R2-5.4; reproduced from Koutsoyiannis et al. [3]; original caption follows]

Figure 13. Schematic of the examined possible causal links in the climatic system, with noted types of potential causality, HOE or unidirectional, and its direction. Other processes, not examined here, could be internal of the climatic system or external.

Finally, Appendix A.1 from Koutsoyiannis et al. [3] provides quantification of changes in natural CO₂ emissions due to temperature changes:

The greatest part of the inflows is due to the respiration of the biosphere, i.e., the biochemical reaction whereby living organisms convert organic matter (e.g., glucose) to CO₂, releasing energy and consuming molecular oxygen [62]. As seen in Figure A1 (and in several publications, e.g., [31]), respiration has increased in recent years, the main reason for this being the increased temperature. Photosynthesis, the biochemical process that removes CO₂ from the atmosphere, producing carbohydrates in plants, algae and bacteria using the energy of light [62], has also increased, resulting in the greening of Earth [40,41] due to the increased atmospheric concentration of CO₂, which is plants' food.

It is not difficult to quantify the increase in respiration due to the temperature rise. The mechanism has been known in chemistry for more than a century. The rate of a chemical reaction k_T at temperature T is an increasing function of T , given by the Arrhenius equation [63]:

$$k_T = A \exp\left(-\frac{a}{R_*T}\right) \quad (\text{A1})$$

where A and a are free parameters and R_* is the universal gas constant. Typically, the rate is measured in moles per unit volume, but it can readily be expressed in mass units. Expressing the relationship at a reference temperature T_0 and dividing with (A1), we obtain:

$$\frac{k_T}{k_0} = \exp\left(-\frac{a}{R_*}\left(\frac{1}{T} - \frac{1}{T_0}\right)\right) \quad (\text{A2})$$

Taking the logarithms and setting $\Delta T := T - T_0$ we find

$$\begin{aligned} \ln\left(\frac{k_T}{k_0}\right) &= -\frac{a}{R_*}\left(\frac{1}{T} - \frac{1}{T_0}\right) = \frac{a}{R_*T_0}\left(1 - \frac{T_0}{T}\right) = \frac{a}{R_*T_0}\left(\frac{\Delta T}{T_0 + \Delta T}\right) \\ &= \frac{a}{R_*T_0}\left(\frac{\Delta T}{T_0} - \left(\frac{\Delta T}{T_0}\right)^2 + \left(\frac{\Delta T}{T_0}\right)^3 - \dots\right) \end{aligned} \quad (\text{A3})$$

and assuming that $\Delta T/T_0$ is small (nb., T_0 is of the order of 300 K, while typical values of ΔT is of the order of 1–10 K). We can neglect all terms beyond first order and find:

$$\frac{k_T}{k_{T_0}} = \exp\left(\frac{a}{R_*T_0^2} \Delta T\right) = \left(\exp\left(\frac{a}{R_*T_0^2}\right)\right)^{\Delta T} = Q_1^{\Delta T} = Q_{10}^{\Delta T/10} \quad (\text{A4})$$

where

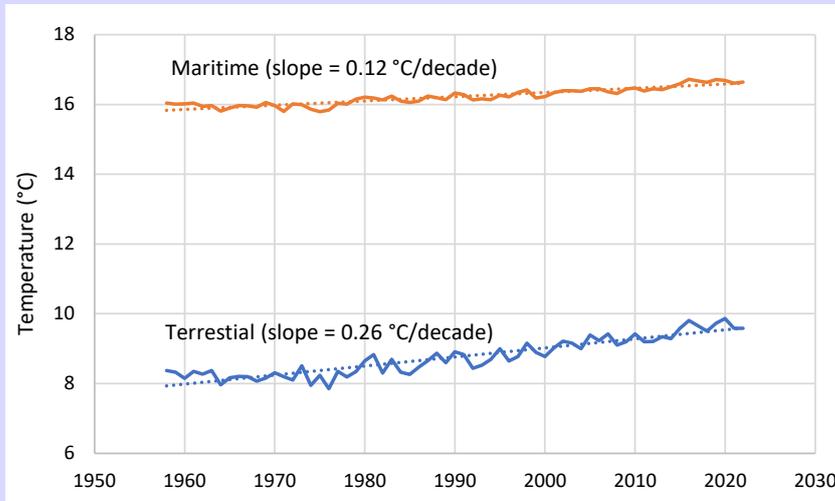
$$Q_1 := \exp\left(\frac{a}{R_*T_0^2}\right), \quad Q_{10} := Q_1^{10} \quad (\text{A5})$$

Notice that both Q_1 and Q_{10} are dimensionless numbers > 1 . The exponential expression in which Q_{10} is raised to power $\Delta T/10$ is known as the Q_{10} model [64].

The exponential increase of the process rate with temperature is a general chemical behavior, also extending to biochemical reactions. This is not a hypothesis or speculation but a proven fact that is widely used in engineering. For example, the metabolic rate in wastewater and sewer systems is expressed by the so-called effective BOD (EBOD, with BOD standing for biochemical oxygen demand). It has been known for more than 75 years that the metabolic rate increases with temperature, as microorganism activity generally increases with temperature. The relationship of EBOD with temperature has been expressed by Pomeroy and Bowlus [36] as $[\text{EBOD}] = [\text{BOD}] (1.07)^{T-20}$, which is similar to (A4), where the reference temperature is $T_0 = 20^\circ\text{C}$ and $Q_1 = 1.07$ ($Q_{10} = 2.0$). The latter relationship is the standard of engineering design in sewer systems.

To apply this framework to find the increase of respiration in the last 65-year period investigated in our study, we first retrieved the global temperature separately for land and sea from the NCEP/NCAR Reanalysis data set. These are shown on an annual timescale in Figure A2. The resulting linear trends, also shown in Figure A2, yield an

increase $\Delta T = 1.69\text{ }^{\circ}\text{C}$ for the terrestrial and $0.78\text{ }^{\circ}\text{C}$ for the maritime part for the 65-year period.



[Figure R2-5.5; reproduced from Koutsoyiannis et al. [3]; original caption follows]

Figure A2. Evolution of global land (terrestrial) and sea (maritime) temperature at 2 m from the NCEP/NCAR Reanalysis data set, retrieved from the ClimExp platform, and resulting slopes of linear trends.

Now the literature gives representative average Q_{10} values of 3.05 for terrestrial respiration [64] and 4.07 for maritime respiration [65]. If R_B and R_E denote the respiration rate at the beginning and the end of the 65-year period, and $\Delta R := R_E - R_B$, then according to (A4),

$$\frac{R_E}{R_B} = Q_{10}^{\Delta T/10} \quad (\text{A6})$$

and hence

$$\Delta R = R_E \left(1 - \frac{1}{Q_{10}^{\Delta T/10}} \right) \quad (\text{A7})$$

For the above given values of Q_{10} and ΔT , the expression in parentheses becomes 0.172 for the terrestrial part and 0.104 for the maritime part. Multiplying these by the R_E values shown in Figure A1, i.e., 136.7 and 77.6 Gt C/year, respectively, we find $\Delta R = 23.5$ and 8.1 Gt C/year, respectively, i.e., a total global increase in the respiration rate of $\Delta R = 31.6$ Gt C/year. This rate, which is a result of natural processes, is 3.4 times greater than the CO_2 emission by fossil fuel combustion (9.4 Gt C/year including cement production).

R2-5.4. Returning to the abstract:

"Its application to instrumental measurements of temperature (T) and atmospheric carbon dioxide concentration ($[\text{CO}_2]$) over the last seven decades provided evidence for a unidirectional, potentially causal link between T as the cause and $[\text{CO}_2]$ as the effect. "

This is easily shown to be an incorrect inference - it is directly refuted by the Earth's "carbon budget" data (see e.g. [[1]]).

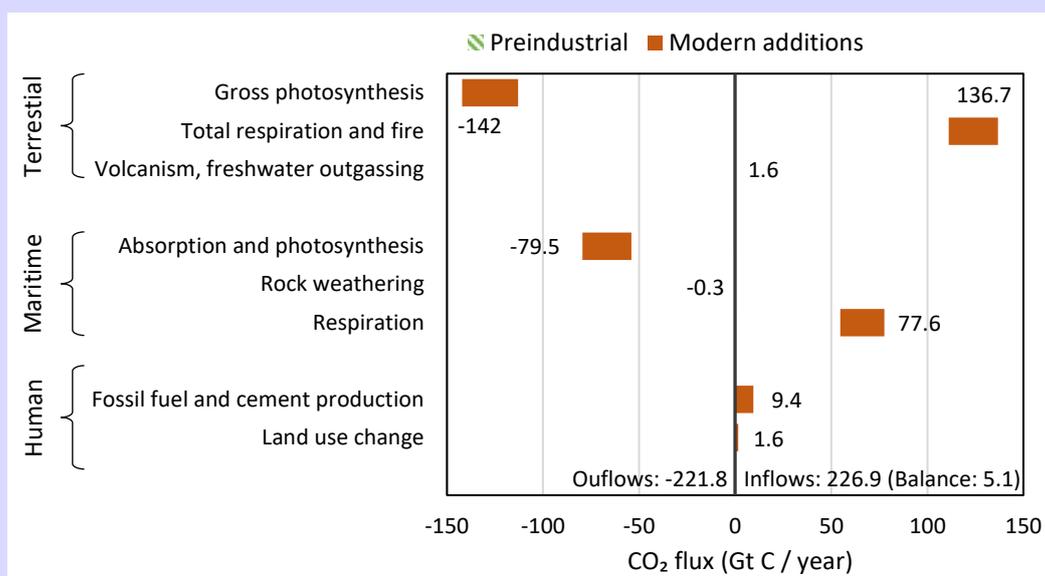
My statement is 100% correct. It follows the statement “As a *result of recent research*, a new stochastic methodology of assessing causality was developed.” And this stochastic methodology, described in Koutsoyiannis et al. [1-3] gave exactly the results summarized in the next statement, quoted by the reviewer.

Furthermore, these findings are 100% consistent with Earth's carbon budget data. The reviewer recommends citing the reference to Cawley (2011) [66]. However, we have used the newer data of the most recent (2021) IPCC's Assessment Report (AR6) [32]. And our findings are fully consistent with both the IPCC and Cawley data.

Most probably, the reviewer has not seen the part of the paper Koutsoyiannis et al. [3], where we use the IPCC carbon budget data. Hence, I am reproducing this part from Appendix A.1 here:

Here we follow the IPCC's [32] account in its recent (2021) Assessment Report (AR6). Its schematic (Figure 5.12 in that Report) does not hide (a) the imbalances in the different parts of Earth and (b) the fact that the natural carbon inputs and outputs in the atmosphere change over time—even though the IPCC's schematic implicitly assumes that “natural” is the budget that occurred in the preindustrial age (1750) and that any change that occurred since is anthropogenic. Interestingly, in an alternative view by Hansen et al. [67], civilization always produced greenhouse gases and aerosols, and humans likely contributed to the increase of both in the past 6000 years, thus resulting in climate forcings.

Based on the IPCC's representation, we have summarized in Figure A1 the information given in IPCC's schematic, in terms of annual carbon balance. When seen in the entire picture, the human emissions due to fossil fuel combustion (9.4 Gt C/year including cement production) is a small part (4%) of the total CO₂ inflows to the atmosphere.



[Figure R2-5.6; reproduced from Koutsoyiannis et al. [3]; original caption follows]

Figure A1. Annual carbon balance in the Earth's atmosphere, in Gt C/year, based on the IPCC [32] estimates. The balance of 5.1 Gt C/year is the annual accumulation of carbon (in the form of CO₂) in the atmosphere.

As per the graph contained in Cawley (2011) [66], namely its Figure 2, which the reviewer reproduces below, I show in my reply to her/his next comment (R2-5.5) that it is fully consistent with our findings.

R2-5.5. Briefly stated, the carbon cycle obeys conservation of mass, so the annual change in atmospheric CO₂, C', is equal to the difference in total emissions into the atmosphere, and total uptake from the atmosphere, i.e.

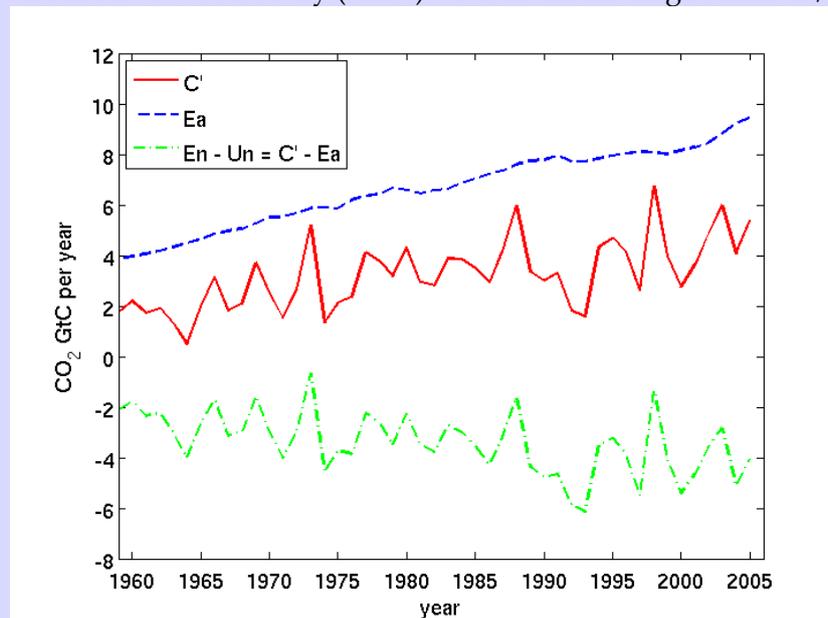
$$C' = E_a + E_n - U_n$$

Where E_a is total emissions from anthropogenic sources, E_n is total emissions from all natural sources and U_n is total uptake from the atmosphere by all natural sinks (note there is no non-negligible anthropogenic uptake, there is no significant amount of carbon capture and storage currently in action). Note that natural sources do not differentiate between CO₂ from natural and anthropogenic, so U_n is a combination of both in proportion to their atmospheric mixing ratio.

Via basic algebra, we obtain

$$C' - E_a = E_n - U_n$$

In other words the difference between natural emissions and uptake by natural sinks (i.e. the net action of the natural carbon cycle) is equal to the difference between the annual rise in CO₂ and the level of anthropogenic emissions, both of which are reliably known - C' from e.g. Mauna Loa Observatory (MLO) data and E_a from government/commercial records.



[Figure R2-5.7 (by Reviewer #5 without a caption)]

If we look at the carbon budget data, we see that every year since 1960 (when the MLO data became available), the annual change in atmospheric CO₂ has been less (about half) the level of anthropogenic emissions, which means that the natural environment must necessarily have been a net carbon sink. The natural environment has been **opposing** the increase, and we can see that this opposition has been increasing with time.

This rules out a change in global temperature being the cause of the rise in atmospheric CO₂ through some response of the natural carbon cycle.

This comment is most interesting and I will reply to it in full detail. Generally, while Reviewer's #5 comments do not seem to be plagiarized in the sense recently revealed and studied by Piniewski et al. [68], this particular comment is a paraphrasis of Cawley's [69] comment in the pubpeer site, created by Rice [70]. The notations, equations and the figure are precisely the same (without paraphrasis) as in Cawley's [69] comment.

Clearly then, this comment, which was posted in 2022, refers to the paper Koutsoyiannis et al. (2022) [2]. The reason why Rice [70] and Cawley [69] created the pubpeer site and posted their comments are revealed by Rice [71] in his blog. He says:

I thought I might simply highlight that I started a PubPeer thread about this paper and Gavin Cawley has already posted a couple of useful comments. A PubPeer thread about the Zharkova et al. paper produced quite an extensive comment thread and probably played a role in it being retracted. You might argue that a paper shouldn't be retracted just because one of the case studies produces results that are almost certainly wrong. On the other hand, you might also argue that if one of their case studies produces such results that it rather undermines their whole method. You might also argue that it's rather embarrassing that one of the Royal Society's journals could publish a paper with what is, these days, a very obviously wrong result.

In other words, these two gentlemen are the supreme judges of what is scientifically wrong and right—as well as able to dictate to scientific organizations, including the Royal Society, what they are allowed to publish and what they should retract.

In a later post, Rice [72] uncovers two things: (a) they emailed to the Royal Society to retract our papers, and (b) that the Royal Society responded negatively (how dared they!). Specifically, Rice [72] writes:

When the Proceedings of the Royal Society A paper came out last year, I emailed the editor to point out that they'd published a paper with a rather nonsensical result. I didn't get a response. However, I was cc'd into a response to someone else who had also complained. This response was, unfortunately, rather dismissive and somewhat insulting. The response said that the criticism had been discussed with the board member and subject editor who handled the paper. According to them, the criticism misinterpreted what was in the paper and was not well-founded. Apparently, the result was scientifically intriguing and would be of interest to many of their readers.

While my coauthors and I are generally responsive to comments and criticisms by anyone interested (and we have stated in our papers that we welcome dialogue), we had decided not to respond to the comments of this group in pubpeer and in their blog for two reasons: (a) because their purpose was not the scientific dialogue, but our silencing and (b) because of the low level of their criticism.

I am very much disappointed that, because of a surprising editorial decision to invite additional reviewers, I am forced now, two years after, to reply to this old comment, referring to a paper of 2022. And this I must do in the framework of the peer review of my new paper in 2024!

So here is my reply, which, however, I do not include in the present paper, because it is totally out of its scope. It refers to my earlier papers. My reply, in full detail, to this particular comment is only part of this report.

The equations and the graph copied from Cawley [66,69] by the reviewer, as well as the interpretation given is totally irrelevant to causality. A necessary condition to assess causality is time precedence and there is nothing in the Reviewer's #5 and Cawley [69] comments related to time lead or lag.

The equations and the graph are fully consistent with our approach in Koutsoyiannis et al. [1-3], and with their finding that the temperature change leads and the [CO₂] change lags, which excludes the possibility that [CO₂] change is a cause of temperature change.

Initially I include the following quotation from Koutsoyiannis et al. [3], Section 9, which contains a simple and brief model of the covariation of the two variables:

As already clarified, the scope of our work is not to provide detailed modeling of the processes studied but to check causality conditions. We highlight the fact that the relationship we established explains only about 1/3 of the actual variance of $\Delta\ln[\text{CO}_2]$. This is not negligible for investigating causality, but also leaves a margin for many other climatic factors to act.

Nonetheless, our results can certainly be improved if we change our scope to more detailed modeling. To illustrate this, we provide the following toy model. Based on our result that the T -[CO₂] system is potentially causal with direction $\Delta T \rightarrow \Delta\ln[\text{CO}_2]$, we estimate $\Delta\ln[\text{CO}_2]$ as

$$\Delta\ln[\text{CO}_2] = \sum_{j=0}^{20} g_j \Delta T_{\tau-j} + \mu_v \quad (8)$$

and we proceed a step further, assuming that the mean μ_v also depends on past temperature, averaged at timescale m , i.e.,

$$\mu_v = \alpha(T_m - T_0) \quad (9)$$

where T_m is the average temperature of the previous m years, and α and T_0 are constants (parameters). Such a simple linear relationship is supported by the above-listed points related to the productivity of the biosphere. Equation (9) will result in negative values μ_v if $T_m < T_0$ and positive otherwise.

By re-evaluating the IRF coordinates g_j simultaneously with the parameters of Equation (9), we find the modified version of the IRF plotted in Figure 14. The optimized additional parameters are $m = 4$ (years), $\alpha = 0.0034$, $T_0 = 285.84$ K. Similarly to [1], we used a common spreadsheet software solver to perform the optimization, adding the two parameters α and T_0 to the unknown coordinates g_j of the IRF and performing the (nonlinear) optimization for all unknowns (m was found by trial-and-error). A graphical

comparison of the actual $\Delta\ln[\text{CO}_2]$ and $[\text{CO}_2]$ with those simulated by the model of Equations (8) and (9) is given in Figure 15. The explained variance for $\Delta\ln[\text{CO}_2]$ was drastically increased from 34% to 55.5% and that for $[\text{CO}_2]$ is an impressive 99.9%.

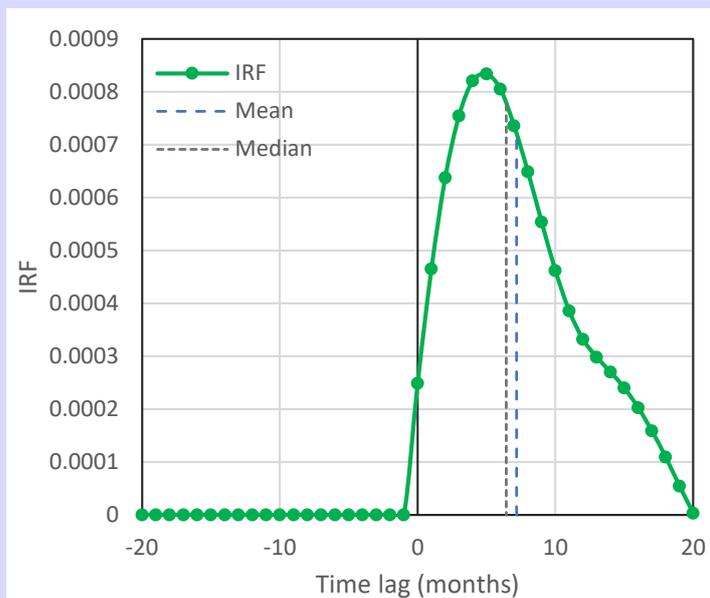
For the convenience of the readers who are interested in repeating the calculations, we also give a parametric expression of IRF and summarize the toy model as:

$$\Delta\ln[\text{CO}_2] = \sum_{j=0}^{20} g_j \Delta T_{\tau-j} + \mu_v, \quad (10)$$

$$g_j = 0.00076 j^{0.67} e^{-0.2j}/K, \quad \mu_v = 0.0034 (T_4/K - 285.84)$$

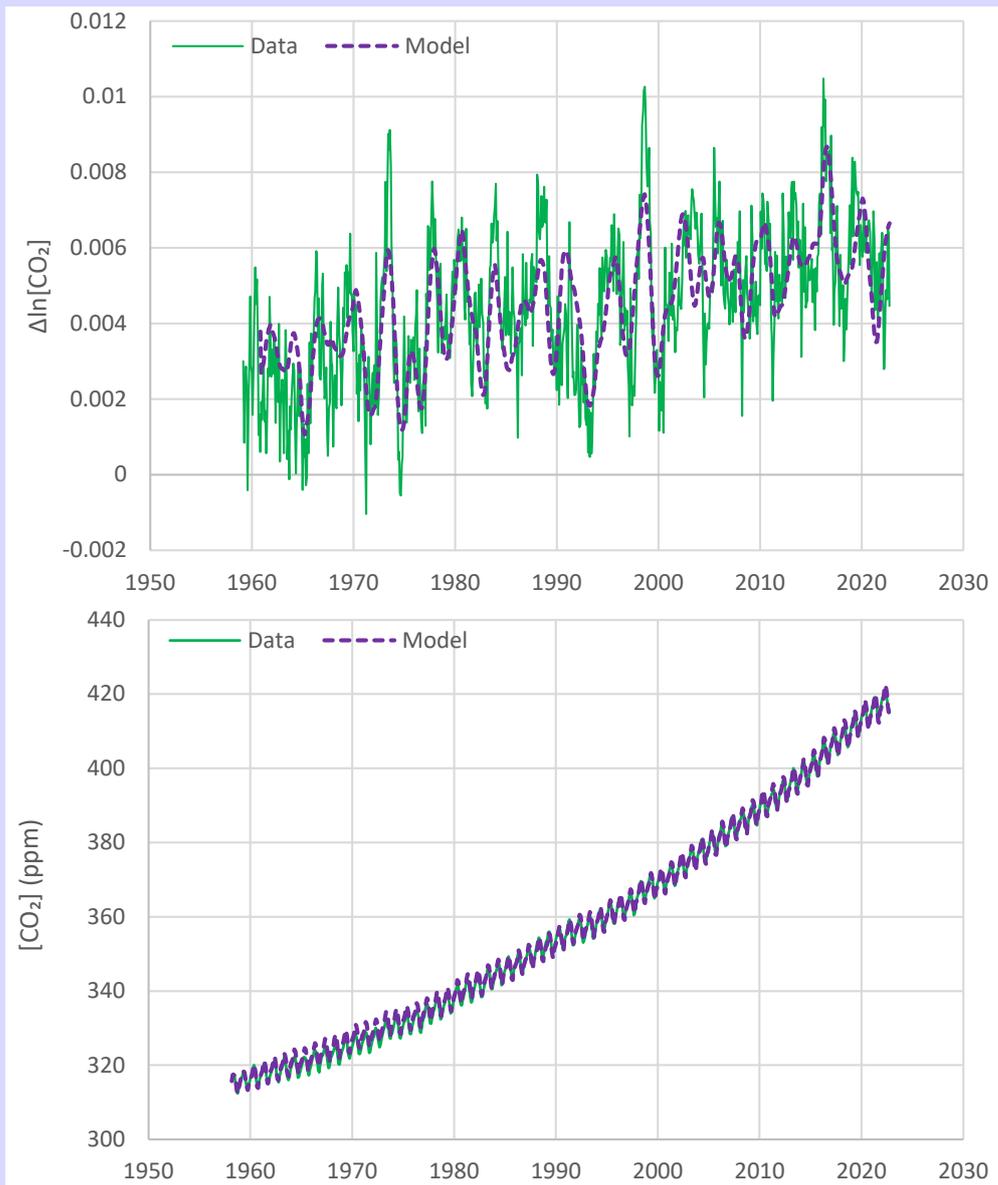
(where K is the unit of kelvin).

We emphasize, however, that we do not exploit the impressive result of explained variance of 99.9% to assert that we have built a decent model, even though this toy model is both accurate (in the lower panel of Figure 15, the simulated curve is indistinguishable from the actual) and parsimonious (the model expression in (10) contains only 5 fitted parameters). We prefer to highlight the fact that the hugely complex climate system entails high uncertainty, and its study needs reliable data that provide the basis for the modeling and testing of hypotheses.



[Figure R2-5.8; reproduced from Koutsoyiannis et al. [3]; original caption follows]

Figure 14. Modified IRF for temperature–CO₂ concentration based on the NCEP/NCAR Reanalysis temperature at 2 m and Mauna Loa time series, respectively, similar to Figure 2 but with IRF coordinates simultaneously optimized with the parameters of Equation (9).



[Figure R2-5.9; reproduced from Koutsoyiannis et al. [3]; original caption follows]

Figure 15. Comparison of the actual $\Delta \ln[\text{CO}_2]$ (upper) and $[\text{CO}_2]$ (lower) with those simulated by the model of Equations (8) and (9).

Now I use this toy model to reproduce Figure R2-5.7, i.e. Cawley's [66,69] and Reviewer's #5 graph. The resulting graph is seen in Figure R2-5.10 below. This is the original Figure R2-5.7 on which I overlay the results of our model in equation (10) in Koutsoyiannis et al. [3], also reproduced above. If the reviewer wants to test and reproduce it independently, here are the steps:

1. I digitized the "Ea" curve from Figure R2-5.7, i.e. Cawley's [66,69] and Reviewer's #5 graph.
2. I took the monthly global temperatures from the NCEP/NCAR reanalysis, as described in Koutsoyiannis et al. [3].
3. I applied the model of equation (10) to produce monthly values of $\Delta \ln[\text{CO}_2]$.

4. I aggregated the values of $\Delta \ln[\text{CO}_2]$ to find monthly values of $\ln[\text{CO}_2]$ and then exponentiated them to find monthly values of $[\text{CO}_2]$.
5. I aggregated from monthly scale to annual scale, and I took the annual differences of $[\text{CO}_2]$, i.e. C' .
6. I calculated the differences $C' - E_a$
7. I plotted the annual series of C' and $C' - E_a$.

It is seen that the model results, which are clearly based on the finding that temperature leads and $[\text{CO}_2]$ lags, is fully consistent with the Cawley's [66,69] and Reviewer's #5 graph.

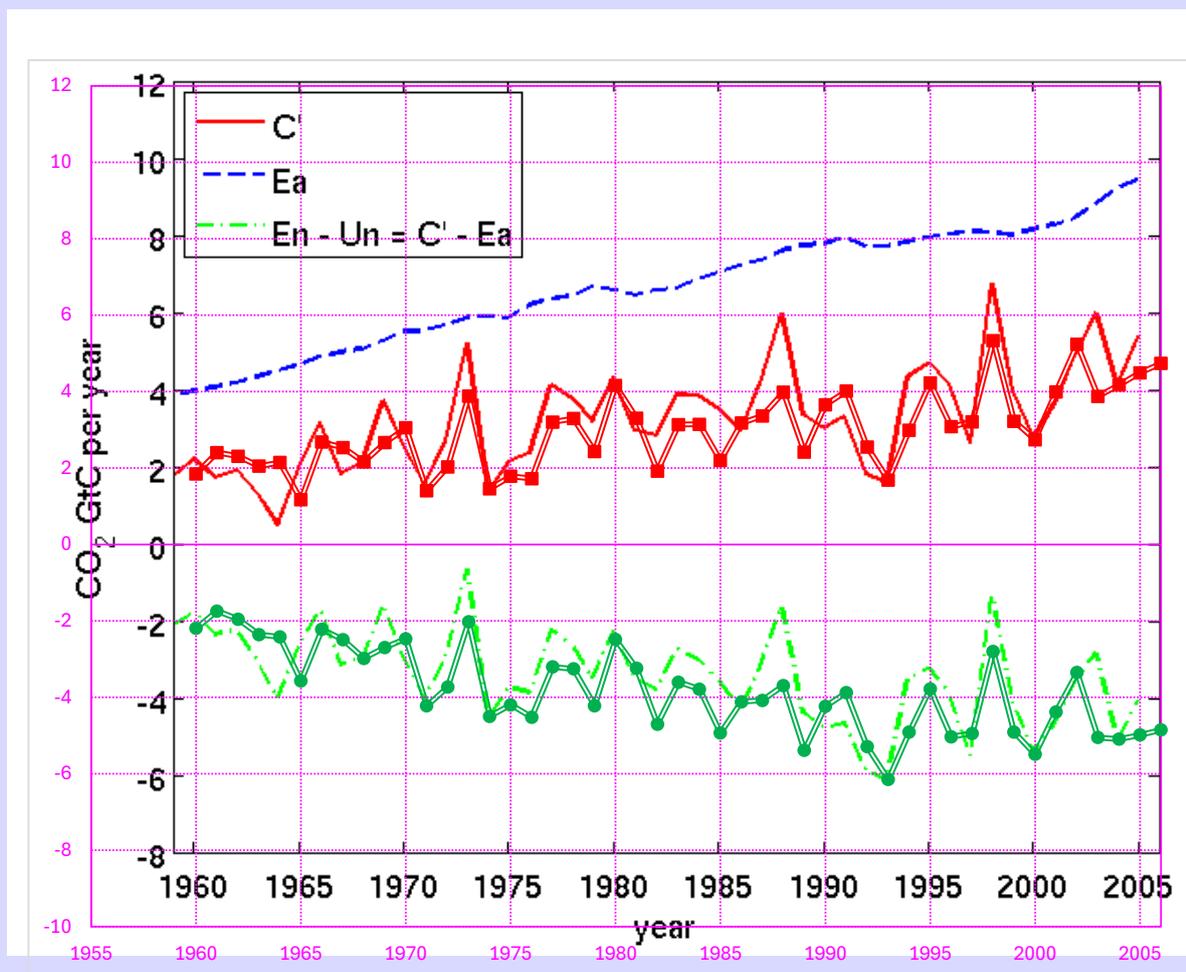


Figure R2-5.10 (new). Plots of the results of model of Equation (10), overlaid to the graph of Cawley [66,69] and Reviewer #5, seen in Figure R2-5.7.

One may notice that Cawley's [66,69] and Reviewer's #5 graph stops at year 2005. Since I took this effort, I expanded the graph up to year 2020, also using the most recent time series of emissions and of $[\text{CO}_2]$ as described in Koutsoyiannis et al. [3]. This is seen in Figure R2-5.11 (new). The model performance is excellent, explaining 81% of the annual variability.

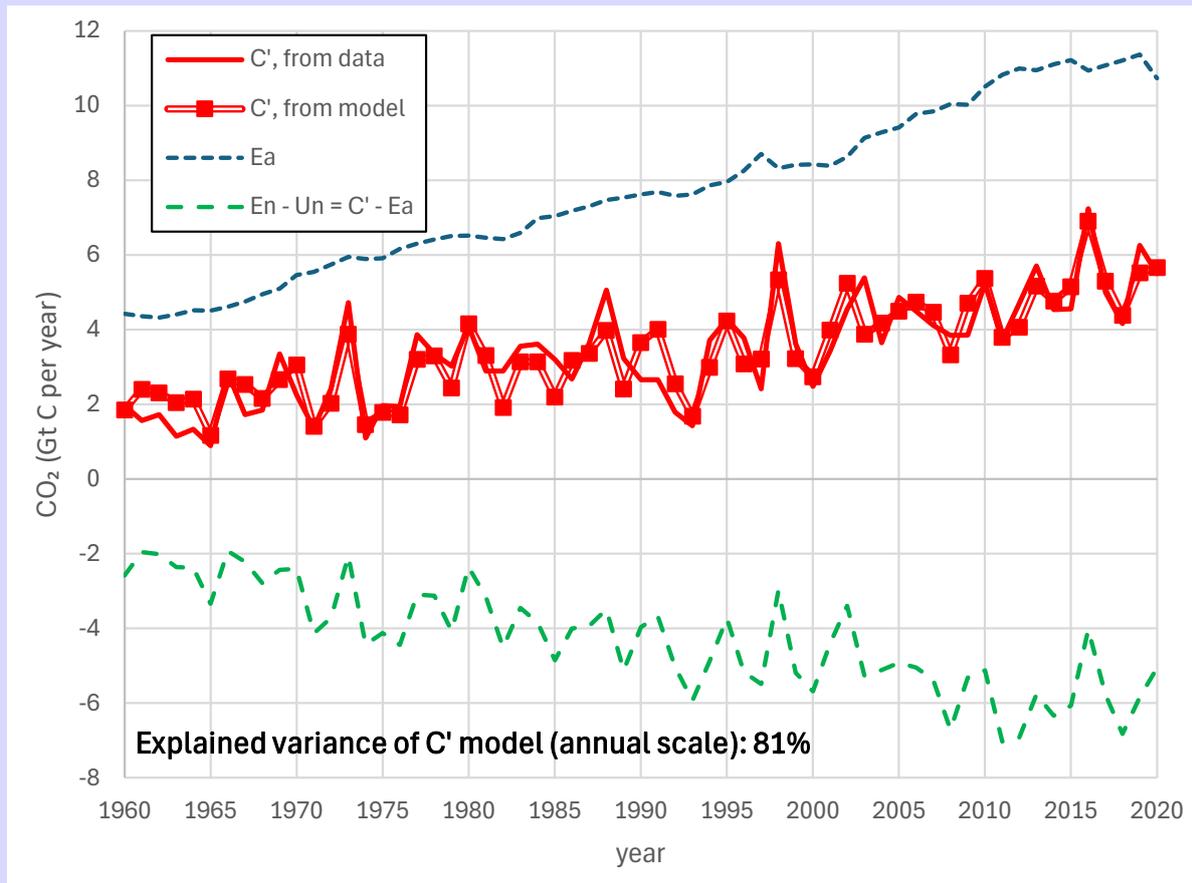


Figure R2-5.11 (new). Update of Cawley's [66,69] and Reviewer's #5 graph (seen in Figure R2-5.7) using the most recent CO₂ emission and concentration time series, and expansion up to year 2020. The results of model of Equation (10) are also shown.

As seen in Koutsoyiannis et al. [3], at the monthly scale the performance is even better, with an explained variance of 99.9%. Despite that, I wish to repeat again the following extract from Koutsoyiannis et al. [3]:

We emphasize, however, that we do not exploit the impressive result of explained variance of 99.9% to assert that we have built a decent model [...]. We prefer to highlight the fact that the hugely complex climate system entails high uncertainty, and its study needs reliable data that provide the basis for the modeling and testing of hypotheses.

After all this analysis, I wish to address an invitation to the reviewer to use my updated graph in Figure R2-5.11 for the next time she/he wants to present this stuff. That would indeed be preferable because Figure R2-5.11 is updated and expanded, as well as for the additional reason that it also includes our model results, which agree very well with the observational data.

R2-5.6. This is not the only line of evidence that contradicts the finding of the current paper. There is also the timing of the increase in atmospheric CO₂ matching the increase in anthropogenic emissions; the ratio of the atmospheric rise and anthropogenic emissions being approximately constant; declining ¹⁴C ratio; declining ¹³C ratio; declining; increasing oceanic CO₂ etc. This is all well known - see e.g. IPCC WG1 reports.

Apparently, the reviewer is not aware of my recent publication [5] entitled “*Net isotopic signature of atmospheric CO₂ sources and sinks: No change since the Little Ice Age*”. I am quoting here the abstract and the graphical abstract, and invite the reviewer to see the entire paper, which refutes her/his above claims.

Abstract Recent studies have provided evidence, based on analyses of instrumental measurements of the last seven decades, for a unidirectional, potentially causal link between temperature as the cause and carbon dioxide concentration ([CO₂]) as the effect. In the most recent study, this finding was supported by analysing the carbon cycle and showing that the natural [CO₂] changes due to temperature rise are far larger (by a factor > 3) than human emissions, while the latter are no larger than 4% of the total. Here, we provide additional support for these findings by examining the signatures of the stable carbon isotopes, 12 and 13. Examining isotopic data in four important observation sites, we show that the standard metric $\delta^{13}\text{C}$ is consistent with an input isotopic signature that is stable over the entire period of observations (>40 years), i.e., not affected by increases in human CO₂ emissions. In addition, proxy data covering the period after 1500 AD also show stable behaviour. These findings confirm the major role of the biosphere in the carbon cycle and a non-discernible signature of humans.

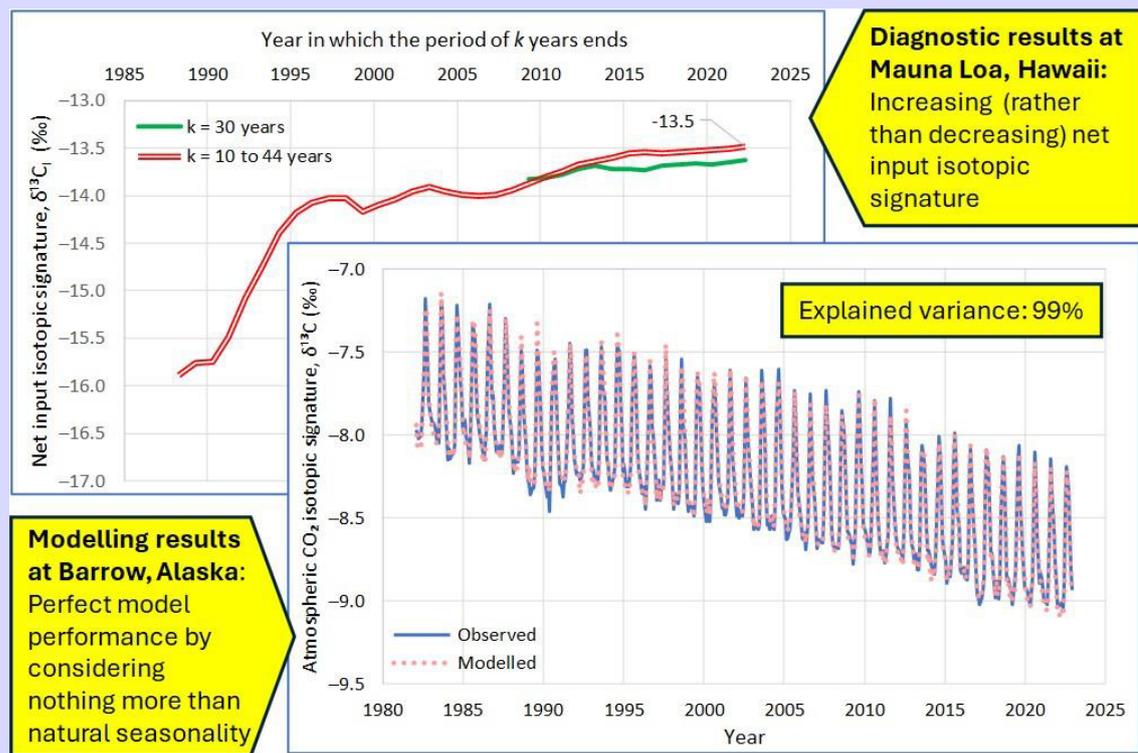


Figure R2-5.12. Graphical abstract of Koutsoyiannis [5].

R2-5.7. Returning to the abstract once more, the next claim is:

"Several proxy series, extending over the entire Phanerozoic or parts of it, ... , are compiled, paired and analyzed. The extensive analyses made converge to the single inference that change in temperature leads, and that in carbon dioxide concentration lags."

This more moderate claim is essentially correct, but **entirely** uncontraversial. Temperatures leading and CO₂ lagging does not imply that the causal relationship is exclusively from

temperature to CO₂, because the same thing can result from a system in dynamic equilibrium with causal relationships in both directions. Throughout most of the Phanerozoic, the carbon cycle has mostly acted as a feedback mechanism, that on some timescales amplifies changes in temperature and on other timescales damping them down. For instance carbon cycle feedback amplified the very small changes in solar forcing from Milankovic cycles, giving rise to large temperature swings between glacial and interglacial periods in over the last 800,000 years seen in the Vostok ice core. The change in solar forcing is much too small to explain the temperature change in isolation. In this case, as the carbon cycle is acting as a positive feedback, it is not surprising that CO₂ lags temperature (it takes time for thermal inertia of the oceans to be gradually overcome and so there is a delay in the outgassing) - see [[4]] for a basic explanation. On longer timescales, the "weathering thermostat" kept planetary temperature fairly stable - the chemical weathering of silicate rocks is temperature dependent. If temperatures are high, this causes greater weathering and more CO₂ is taken out of the atmosphere. This reduces the GHE, which tends to lower temperatures again, over many thousands of years, and the dynamic equilibrium is restored. Again, temperature leads and CO₂ lags. None of this is obscure knowledge - it can easily be found in public understanding of science texts, such as Archer [[2]] or Volk [[3]]. As we know this from existing studies, informed by physics, a statistical model sheds no real light on the issue and does not justify publication.

However, on geological timescales, the carbon cycle can also act as a forcing. For example, the most plausible explanation for the emergence from the "Snowball Earth" conditions during the Cryogenian, Late Ordovician and Silurian is that the weathering thermostat was "switched off" by the ice coverage, which allowed GHG emissions from volcanic activity to gradually accumulate in the atmosphere, until the EGHE was sufficient for temperatures to rise to the point where the ice melted. In that case, CO₂ led and temperatures lagged. Another example would be changes in the weathering thermostat due to changes in the position of the continents (see e.g. [[5]]) or the creation of mountain ranges (see e.g. [[6]]). In those cases, CO₂ would also lead and temperatures lag because there the CO₂ is acting as a forcing rather than a feedback. Again, this information has been discussed in the public debate on the science of climate.

The entire comment is irrelevant. The entire abstract, from which the reviewer quotes a couple of sentences is:

As a result of recent research, a new stochastic methodology of assessing causality was developed. Its application to instrumental measurements of temperature (T) and atmospheric carbon dioxide concentration ($[CO_2]$) over the last seven decades provided evidence for a unidirectional, potentially causal link between T as the cause and $[CO_2]$ as the effect. Here we refine and extend this methodology, and apply it to both paleoclimatic proxy data and instrumental data of T and $[CO_2]$. Several proxy series, extending over the entire Phanerozoic or parts of it, gradually improving in accuracy and temporal resolution up to the modern period of accurate records, are compiled, paired and analyzed. The extensive analyses made converge to the single inference that change in temperature leads, and that in carbon dioxide concentration lags. This conclusion is valid for both proxy and instrumental data in all time scales and time spans. The time scales examined start from annual and decadal for the modern period (instrumental data) and the last two millennia (proxy data), and reach one million years

for the most sparse time series for the entire Phanerozoic. The type of causality appears to be unidirectional, $T \rightarrow [\text{CO}_2]$, as in earlier studies. The time lags found depend on the time span and time scale and are of the same order of magnitude as the latter. These results contradict the conventional wisdom, according to which the temperature rise is caused by $[\text{CO}_2]$ increase.

This abstract summarizes the content of the paper and is fully consistent with that content. It does not reflect the other studies which the reviewer likes and refers to. It reflects the analyses contained in my paper. In this respect, the sentences quoted by the reviewer are 100% accurate and no change is required.

R2-5.8. In summary, the abstract contains nothing that is both correct and novel. The conclusion is naive, ignores a substantial amount of well established scientific work, reflecting a lack of adequate scholarship.

As I already stated, the abstract contains a summary of the paper. The analyses and results presented in the paper are correct and novel, as also recognized by the three original reviewers.

I did not invite the reviewer to assess whether my scholarship is adequate or not. Of course he has the right to do that without my invitation, but she/he may keep his assessment for her/himself.

R2-5.9. Introduction

Crime begins with propaganda, even if such propaganda is for a good cause
Hans Fritzsche {1}

This sort of rhetoric is simply unacceptable in a scientific paper and only undermines the authors work. It seems to imply that the mainstream scientific position on CO₂ is (well intentioned) "propaganda" rather than the result of dispassionate scientific research, which is simply not the case.

I put this as a motto relevant to the paper. I could argue about its relevance, but the discussion would be too long and would distract the interest from the focus of the paper. Therefore, I preferred to delete the motto in the re-revised manuscript.

R2-5.10. One of the most controversial issues of our time is the causal relationship between atmospheric temperature (T) and carbon dioxide (CO₂) concentration ($[\text{CO}_2]$) in Earth's climate.

There is no genuine *scientific* controversy on the existence and mechanism of the EGHE, which has been well understood for over a century, nor on the basic science of the carbon cycle or the evidence that demonstrates that the rise in atmospheric CO₂ is due to anthropogenic emissions (the IPCC WG1 reports have long had an explicit section on that issue). There is controversy in the public debate on climate, but it is largely due to misinformation promulgated on climate blogs and media. There are occasional failures of peer review that result in the publication of fundamentally flawed papers, but in the long run science is self-correcting and robust to these problems.

What is *misinformation, propaganda, censoring* and *silencing* is a tough issue to discuss and, again, the discussion would be a distraction from the focus of the paper. I invite the reviewer to see

another paper of mine, [44], and in particular its section 6, where I discuss the political origin of the climate change agenda.

To address this comment, I changed the quoted phrase, which now reads:

Some of the most controversial issues of our time are related to Earth's climate, not excluding the causal relationship between atmospheric temperature (T) and carbon dioxide (CO_2) concentration ($[\text{CO}_2]$).

R2-5.11. It is also accepted that the cosmic ray flux has a large effect on the climate and this flux had variations, including a cycle with a period of about 145 million years, corresponding to the passage of the solar system through one of the two sets of spiral arms of the Milky Way [16].

While time constraints means that I can't check on all of the references cited, this is something that most certainly is not accepted by the scientific community (although it has been widely discussed on climate skeptic blogs). From the IPCC AR6 WG1 report (page 958)

AR5 concluded that the GCR effect on CCN is too weak to have any detectable effect on climate and no robust association was found between GCR and cloudiness (Boucher et al., 2013). Published literature since AR5 robustly supports these conclusions with key laboratory, theoretical and observational evidence. There is high confidence that GCRs contribute a negligible ERF over the period 1750–2019.

The section makes reference to the CLOUD project at CERN that was set up to test the the proposed causal mechanism (an increase in cloud condensation nuclei) and found that the effect was very small. An effect on geological timescales has not been completely ruled out as far as I am aware, but there is very little beyond a statistical correlation as evidence.

I prefer not to include this information in the paper, as it is another political issue. We should not forget that IPCC is a political organization. Perhaps the reviewer is not aware of the developments related to the political aspects of the CLOUD project at CERN. Here I quote a part from an interview from 2011 of the then CERN's Director General Rolf-Dieter Heuer in the German newspaper Die Welt [73] using machine translation from German to English (my emphasis):

Heuer: This is actually about understanding cloud formation better. There are many parameters in nature that influence this - including temperature, humidity, impurities and cosmic radiation. The "Cloud" experiment is about investigating the influence of cosmic radiation on cloud formation. The radiation used for this comes from the accelerator. And in an experimental chamber, it is possible to research under controlled conditions how droplet formation depends on radiation and suspended particles. The results will be published shortly. **I asked my colleagues to make the results clear but not to interpret them. This would immediately enter the highly political arena of the climate change discussion.** It must be clear that cosmic radiation is only one of many parameters.

What I write is consistent with the last sentence in the above quotation, that that cosmic radiation is only one of many parameters.

To address the reviewer's comment, in the revised manuscript and in the phrase quoted by the her/him, I have changed the verb "accepted" to "asserted" (which was actually what I meant but perhaps I made a typing error and the automatic speller changed it).

R2-5.12. "According to the established narrative, which is simplistic and negligent of the huge complexity of the climatic system, ..."

Again this is rhetoric unbecoming of a scientific paper and devalues the authors argument. It isn't an "established narrative", it is a century or more of diligent scientific research and is not simplistic (demonstrated by the page count of the IPCC report) not does it neglect complexity. I will not comment on further uses of rhetoric in this review, as it would be an egregious waste of my time as a reviewer. It is not to the authors advantage in communicating his findings. That ought to be sufficient.

I did not invite the reviewer to advise me regarding my style of writing. I am 69, I have published more than 250 journal articles and more than 1100 documents in total, including editorial notes and books and I feel I have the right to express myself according to the experience I developed through the years.

That said, in the phrase quoted by the reviewer, I changed "established" to "this".

R2-5.13. It is concerning that the author cites one of their previous papers on this topic (reference 22 in the paper) but does not appear to mention the critical comment paper that was published explaining why the findings of that paper were incorrect. Unfortunately the same error is made in the current paper. Also cited is the paper by Humlum et al. (reference 34), which makes essentially the same mistake as this paper (differencing of the time series data deletes the contribution of anthropogenic emissions), but none of the papers that explained the error. This is not an acceptable practice.

I do not understand which error and which "critical comment paper that was published" the reviewer refers. There is no error. I do not understand why it is concerning to the reviewer that I cite my previous works. Should I repeat in the present work what I have already published?

See additional information in my reply to the next comment.

R2-5.14. Methodology

One important issue that should be kept in mind is related to the very high autocorrelations, which appear particularly in [CO₂] series. As high autocorrelation increases uncertainty in the long term, this is a major case leading to false identification of potential causality. This problem was discussed in {22} and illustrated in the electronic supplementary material of this publication, along with the technique to handle such situations and avoid false conclusions. Specifically, the appropriate technique is to difference the time series, so as to investigate the changes of the related processes, rather than the processes per se. Differencing reduces the autocorrelations substantially and thus avoids spurious results but has the disadvantage of reducing the explained variance.

This, as pointed out by Asbrink [[7]] and in the pubpeer discussion of the Author's previous work [[8]], is a fundamental error. Differencing the time series causes the long term linear trend component of the time series to become an additive constant in the differenced time series. If the method of analysis is insensitive to additive constants, then there is no mathematical link between the findings of the analysis and the long term increase. This is problematic in the case of the carbon cycle because the cause of the long term increase in both series (fossil fuel emissions and the EGHE) is not the same as the causes of the short term variability around the trend namely seasonality and the effects of ENSO (as noted by Bacastow, see [[7]]). Seasonality can be dealt with via the running mean used in the paper, or use of anomalies, which would be a more usual approach. In the case of ENSO, it affects temperature directly, through changes in ocean surface temperature, but the effect on CO₂ is not via temperature, but via changes in precipitation leading to variation in growth and decay in the terrestrial biosphere. It is a confounding variable that is missing from the analysis. Unfortunately, this means the analysis will incorrectly attribute the cause of the long term change to the cause of the short term variability, which is essentially unrelated. This is the same error made by Humlum et al., as well as several others, including Murry Salby and the error has been repeatedly pointed out. This primarily affects the instrumental data, but similar problems may also be present in the paleoclimate studies.

First off, we should distinguish Åsbrink's [74] Commentary from the "*pubpeer discussion*". The former is a scientific work, formally published in a journal. The latter is a failed action to force retraction of our paper. As the "*pubpeer discussion*" is already discussed above (comment R2-5.5), I am replying here to the part of the reviewer's comment that is related to Åsbrink's Comment. I admit that it was my omission not to cite Åsbrink, which I now corrected.

Åsbrink did not challenge our methodology nor our results. His comment does not contain anything related to our mathematical part. He simply expressed disagreements with some of our formulations and interpretations. These disagreements allowed us to confirm our results in the original papers [1,2] and to shed further light on why our original formulations stand. As our new analyses exceeded the length of a typical reply to a Commentary, we published them in a stand-alone new paper [3]. In this, we responded to all Åsbrink's disagreements and we further developed our method. We let both the Royal Society and Åsbrink know that they can find our complete set of replies to our newer paper [3]. This triggered a constructive email exchange with Åsbrink.

In particular, with respect to Åsbrink claim that

'Common perception' is different for different timescales. It is a well-known fact that the fluctuations of [CO₂] in a timescale of a couple of years are caused by temperature variations.

and

Hence, the common perception that increasing [CO₂] causes increased T seems likely.

in our paper [3] we proved that this common perception cannot stand. Already in our paper [2], our Figure 15, whose time lags span 200 months (≈ 17 years) confirms the validity of our results for timescales larger than decadal. In addition to what is reported in our paper [2], in [3] we tested larger timescales, using longer temperature series along with the Mauna Loa [CO₂] measurements, which began in 1958. These analyses gave essentially the same results as

in the case study presented in [2], suggesting a potentially causal system with the directionality being $\Delta T \rightarrow \Delta \ln[\text{CO}_2]$, with even better characteristics (higher cross correlation and explained variance, and time lags greater than or equal to those in [2]). In addition, we increased the time interval of differencing from one year to more than a decade. The results were again essentially the same. As per the Åsbrink's suggestion that "*For the annual cycle, one should actually look at the two hemispheres separately*", in our paper [3] we conducted an additional analysis with the South Pole $[\text{CO}_2]$ measurements, with the results being very similar: potentially causal system with causality direction $\Delta T \rightarrow \Delta \ln[\text{CO}_2]$, and lags close to 10 months. In all our case studies the possible causality $\Delta \ln[\text{CO}_2] \rightarrow \Delta T$ has been excluded as not satisfying the necessary condition of time precedence.

Concerning the reviewer's remark about the effect that differencing the time series may have on the long-term trend, our analysis in Section 9 of our paper [3] shows that clearly this is not the case. My reproduction of the reviewer's graph in her/his comment R2-5.5 clearly confirms that after aggregating the differenced results, our model, fully based on causality direction $T \rightarrow [\text{CO}_2]$, has even better performance than in the differenced case: explained variance 81% at annual scale and 99.9% on monthly scale.

Other issues mentioned by the reviewer such as ENSO and the role of the biosphere are fully analyzed in our paper [3]—see Section 9 and Appendices A.1 and A.3.

R2-5.15. Conclusions

Is there a mechanism that, at the recent period has, due to human or presumed 'unnatural' actions, reversed directionality, as the popular claim is? Perhaps, but no analysis based on observational data has shown that.

Yes. Extracting fossil carbon from the lithosphere and injecting it into the atmosphere is a mechanism that will increase atmospheric CO_2 ; the extended greenhouse effect means that this will result in an increase in global mean surface temperature (all things being otherwise equal. The carbon budget/mass balance analysis given earlier (see also [[1]]) establishes the former; a variety of evidence confirms the latter, e.g. [[9]] but see also the IPCC WG1 report

I have changed the quoted phrase, which now reads:

Did human actions, such as fossil fuel combustion and other presumed 'unnatural' actions, reverse directionality, as the popular claim is?

R2-5.16. Rather, such claims are based on imagination and climatic models full of assumptions. All models are full of assumptions, including the statistical models used in the paper. Again, this is rhetoric that does not belong in a balanced, objective scientific paper.

Indeed, models are based on assumptions, but not all models are full of assumptions. The stochastic methodology on which the paper is based is extraordinarily parsimonious in terms of assumptions, as seen in Section 3.2.

I have many papers showing the poor performance of climate models in representing reality, such as [75-80], while, most importantly, our recent paper, Koutsoyiannis et. al. [3] shows that the causality direction in climate models, identified by the same methodology, is opposite to

that identified from real-world data. However, I prefer not to expand the paper about this, as it would be a distraction.

In terms of my style of writing (the “*rhetoric*” according to the reviewer), please see my reply to comment R2-5.12.

R2-5.17 References [in Reviewer’s #5 review]

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