Report on review comments of “Reservoir Routing and its Application to Atmospheric Carbon Dioxide Balance”

by Demetris Koutsoyiannis

Introductory notes

The manuscript with the above title was submitted to *Water* on 13 May 2024 (Manuscript ID water-3021933), following an invitation from the journal’s Editorial Office. It received three reviews with several constructive comments. The editor’s decision was minor revision. As can be seen below, where all the review material is reproduced, I have addressed all the constructive review comments in the way I explain in full detail.

Key:

> Review comment.

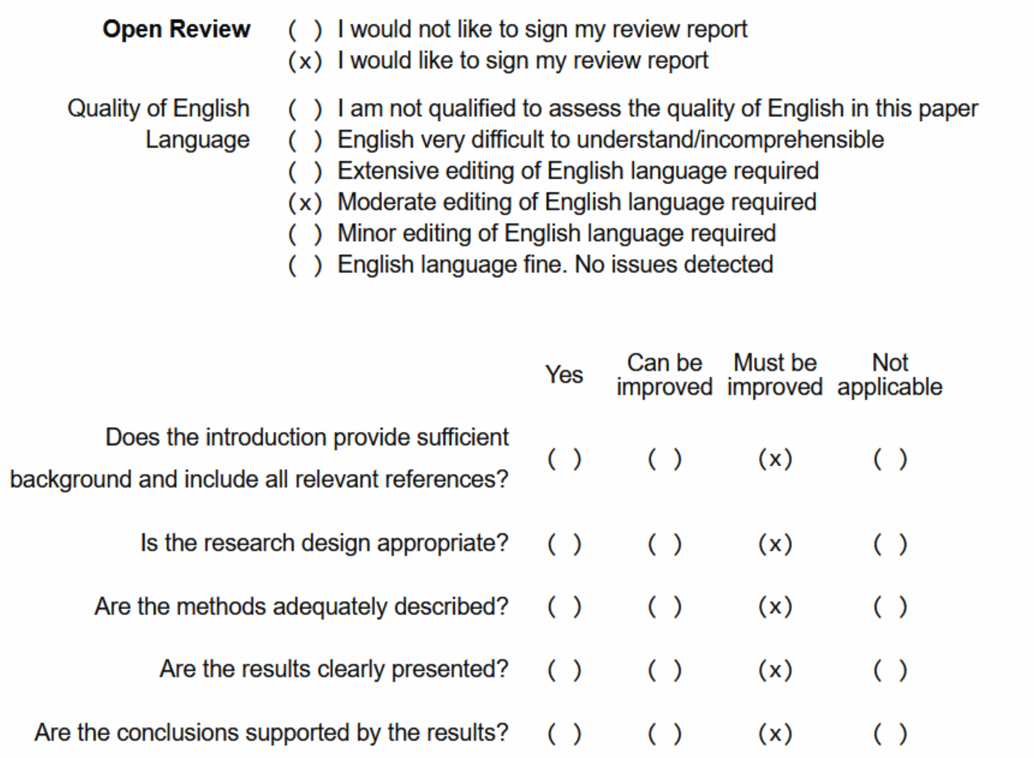
Response.

Quotation from manuscript.

Note: The list of references contained at the bottom of this Report is for the Report per se and its numbering does not coincide with that in the paper.

Reviewer 1

> **R1.1.**



Open Review
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	Yes	Can be improved	Must be improved	Not applicable
Does the introduction provide sufficient background and include all relevant references?	( )	( )	(x)	( )
Is the research design appropriate?	( )	( )	(x)	( )
Are the methods adequately described?	( )	( )	(x)	( )
Are the results clearly presented?	( )	( )	(x)	( )
Are the conclusions supported by the results?	( )	( )	(x)	( )

Apparently, Reviewer #1 opined that the manuscript must be improved in all aspects. I trust that, thanks to all reviewers’ comments, the revised manuscript is much improved.

R1.2. This manuscript entitled Reservoir Routing and its Application to Atmospheric Carbon Dioxide Balance analyzes the reservoir routing framework, extends it to find approximate solutions for nonlinear cases, and then applied it to describe the mass balance of atmospheric carbon dioxide and determining characteristic residence times. Although the framework is very novel, the experiment design and organization look problematic. I provide some detailed comments below and recommend Rejection of it.

I am very glad that the reviewer recognizes that the framework is very novel. I appreciate her/his detailed comments. I trust that the paper's organization is improved in its revised version.

R1.3. (1) The abstract and the main text lack quantitative conclusions to answer the questions proposed around carbon dioxide characteristic residence times.

I cannot understand this Comment. Both the Abstract and the Conclusions provide quantitative information "*around carbon dioxide characteristic residence times*". Perhaps the reviewer was confused by my phrase "no more than", which I changed now to "about". Specifically, the Abstract ends with a statement that now reads:

> The mean residence time of atmospheric carbon dioxide turns out to be about four years and the response time is smaller than that, thus opposing the much longer mainstream estimates.

The Conclusions section contains this paragraph, which now reads:

> The application of the reservoir routing framework to the atmospheric CO_2 gives useful insights, in terms of residence and response times, which have been an issue of controversy. The theoretical framework results in excellent agreement with real-world data on carbon dioxide concentration. The atmosphere appears to behave as a linear reservoir in terms of the atmospheric CO_2, whose exchange is clearly dominated by the biosphere processes, with human emissions playing a minor role. The quantification of the atmospheric CO_2 exchange with the RRR framework yields reliable and intuitive results, complying with observations, in contrast to the results of complex climate models, which are shown to be inconsistent with reality. The mean residence time of atmospheric CO_2 is about four years and the response time is smaller than that, thus contradicting the mainstream estimates which suggest times of hundreds or thousands of years, or even longer.

R1.4. (2) The topic is inconsistent, which includes both reservoir water balance and atmospheric carbon dioxide mass balance. One paper should have only one topic. Please focus on one topic to express the whole study, especially for the introduction part.

I am aware that it is fashionable to break up studies into pieces and publish them separately. This tactic is seemingly rewarding for individuals (in inflating their CVs with numerous publications). However, it is my principle not to follow the current fashion. Rather I have promoted, by several means, including coauthoring several Editorials and Joint Editorials [1-18], the opposite idea, that the scientific community should take a position against it. I try not to contribute to the so-called "salami publishing", but I work for the idea of longer, more informative and more thorough papers, which cover several facets of a scientific question more holistically.

> **R1.5.** (3) In addition, the introduction reads very casual. Please use scientific and professional language to organize the manuscript.

I believe that my language is scientific and professional. I kindly request the reviewer to tolerate my style of writing which I developed after an over 40-year research and teaching experience. Of course, I understand that the reviewer might have a different style, but this does not mean that everybody should follow a particular style.

> **R1.6.** (4) The proposed method which studies the carbon dioxide characteristic residence times, outflow and inflow lacks validation and assessment of its suitability. This is major deficiency of the experiment design.

Thankfully for the comment, I have now added a new Section which reads as follows:

> *4.7 RRR validation*
>
> A first thought when proposing a new method is to compare it with an existing method. As discussed in Section 3, the topic of the CO_2 balance is heavily studied and also officially reported in IPCC Assessment Reports. However, possible agreement of the RRR framework results with those of IPCC would not validate the former, because of the severity of problems in the latter, which are discussed in Section 3 and in Appendices A.2 and A.3. In particular, Appendix A.2 offers an indirect (not formal) validation of the RRR results by enrolling additional data, namely isotopic data of atmospheric ^{14}C. These data reflect an accidental real-world experiment, not designed as such but related to nuclear weapons testing, in the 1950s and 1960s, which stopped afterwards. The injection of a series of ^{14}C impulses in the atmosphere made a real-world situation close to an ideal to estimate an IRF of the $^{14}CO_2$ dynamics. The analysis in Appendix A.3 shows that the observed $^{14}CO_2$ dynamics are compatible with the RRR results and blatantly incompatible with the IPCC results.
>
> For a formal validation of the RRR method, we use the split-sample scheme (Klemeš, 1986, [19]) which has been the standard methodology in hydrology. Specifically, we split the data into two periods, where the first, 1958 – 2002, representing about 2/3 of the dataset length, is used for model fitting, and the second, 2003 – 2023, is used for validation. The resulting model fits are shown graphically in Figure 20 and Figure 21, the fitted parameters by the same method as in Section 4.3 are shown in Table 2, and the performance indices are shown in Table 3, also in comparison to those of the fit on the entire observation period.

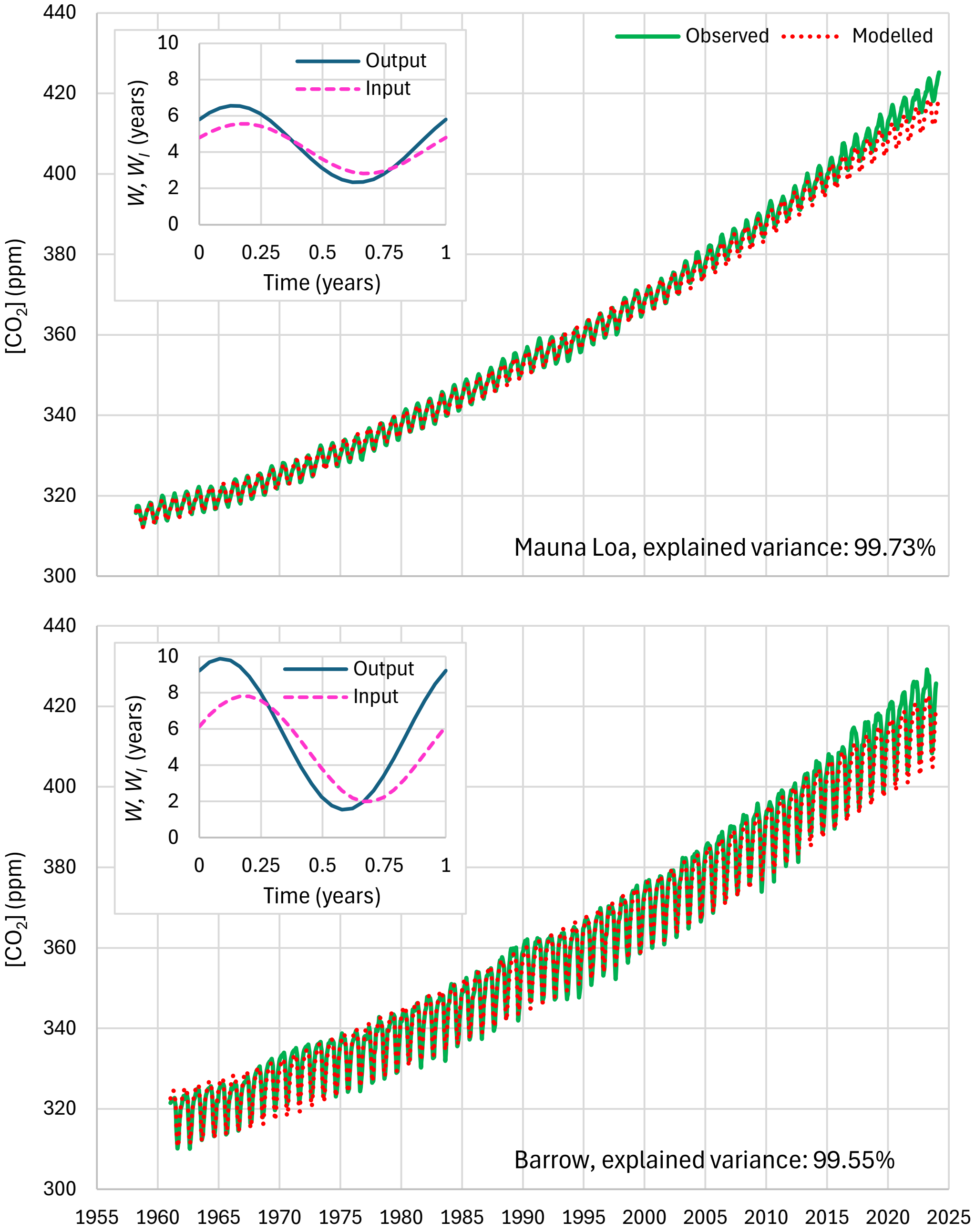


Figure 20. Comparison of observed and simulated storage, $S(t) \equiv [\mathrm{CO_2}]$, as in Figure 13 but for the calibration period 1958 – 2002: (**upper**) Mauna Loa and (**lower**) Barrow. The insets show the seasonal variation of the characteristic times W, W_I.

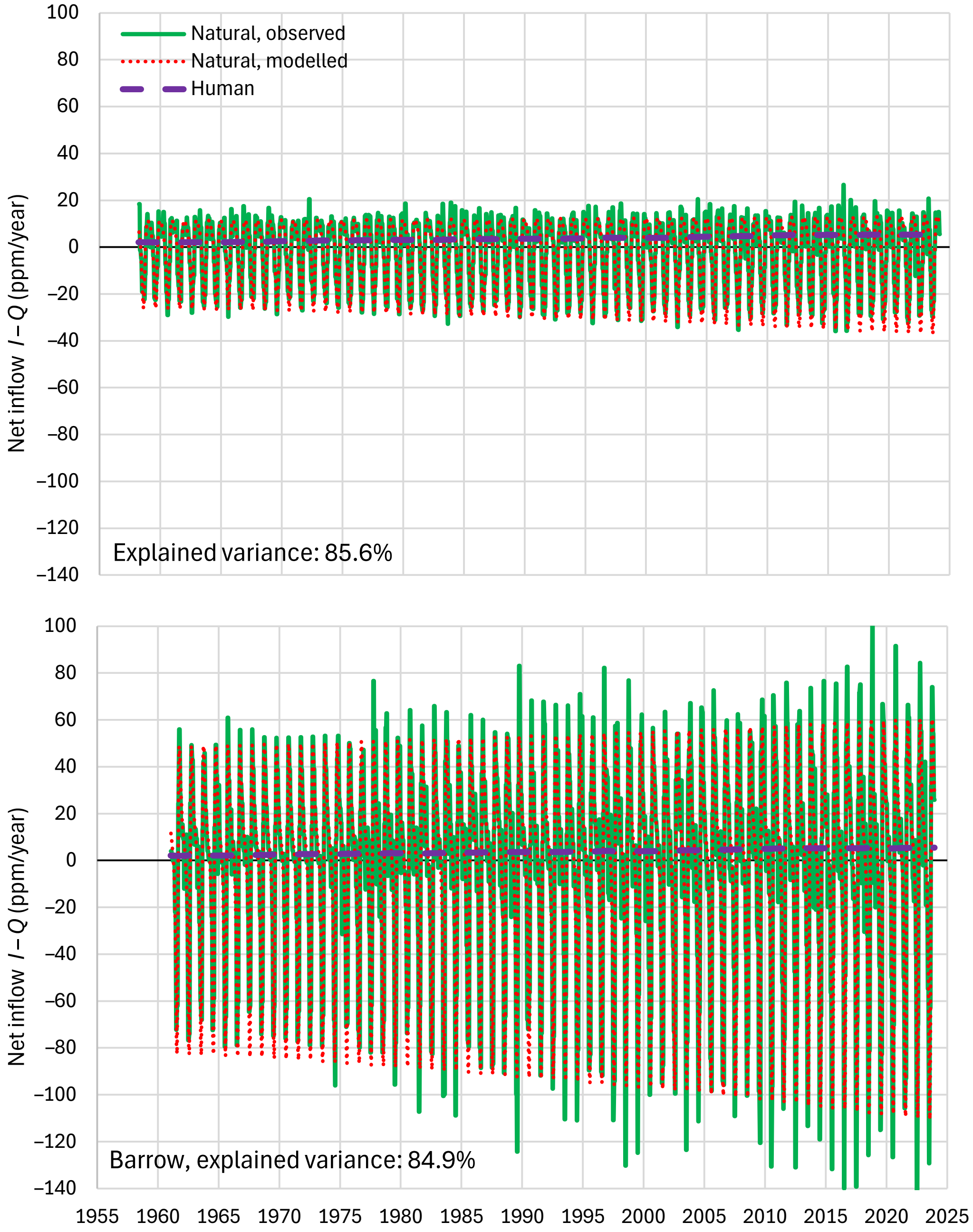


Figure 21. Comparison of observed and simulated net inflows as in Figure 15 but for the calibration period 1958 – 2002: (**upper**) Mauna Loa and (**lower**) Barrow.

Table 2. Fitted parameters of Equations (55) and (56) as in Table 1 but for calibration period 1958 – 2002. For comparison, the parameters of Table 1 are also shown in parentheses.

Site	b	φ	A (years)	ψ	b_I	φ_I	A_I (years)	ψ_I
Mauna Loa	1	5.399	2.126	2.092	0.935	5.164	1.578	2.858
		(5.445)	(1.973)	(2.115)	(0.953)	(5.247)	(1.462)	(2.855)
Barrow	1	5.710	4.174	1.368	0.935	5.134	2.207	1.594
		(5.757)	(4.181)	(1.370)	(0.953)	(5.149)	(3.104)	(1.633)

Table 1. Explained variances (%) as performance indices of the RRR method for the indicated applications.

↓Site	**Storage $S \equiv [CO_2]$ (ppm)**			**Net inflow, $I - Q$ (ppm/year)**		
Period→	**All**	**1958-2002**	**2003-2023**	**All**	**1958-2002**	**2003-2023**
Calibration over the entire period						
Mauna Loa	99.94	99.82	99.64	85.81	87.25	83.30
Barrow	99.77	99.25	99.16	85.30	85.82	84.64
Calibration over period 1958 – 2002						
Mauna Loa	99.73	99.90	96.24	85.57	87.46	82.25
Barrow	99.55	99.44	95.88	84.85	86.18	83.13

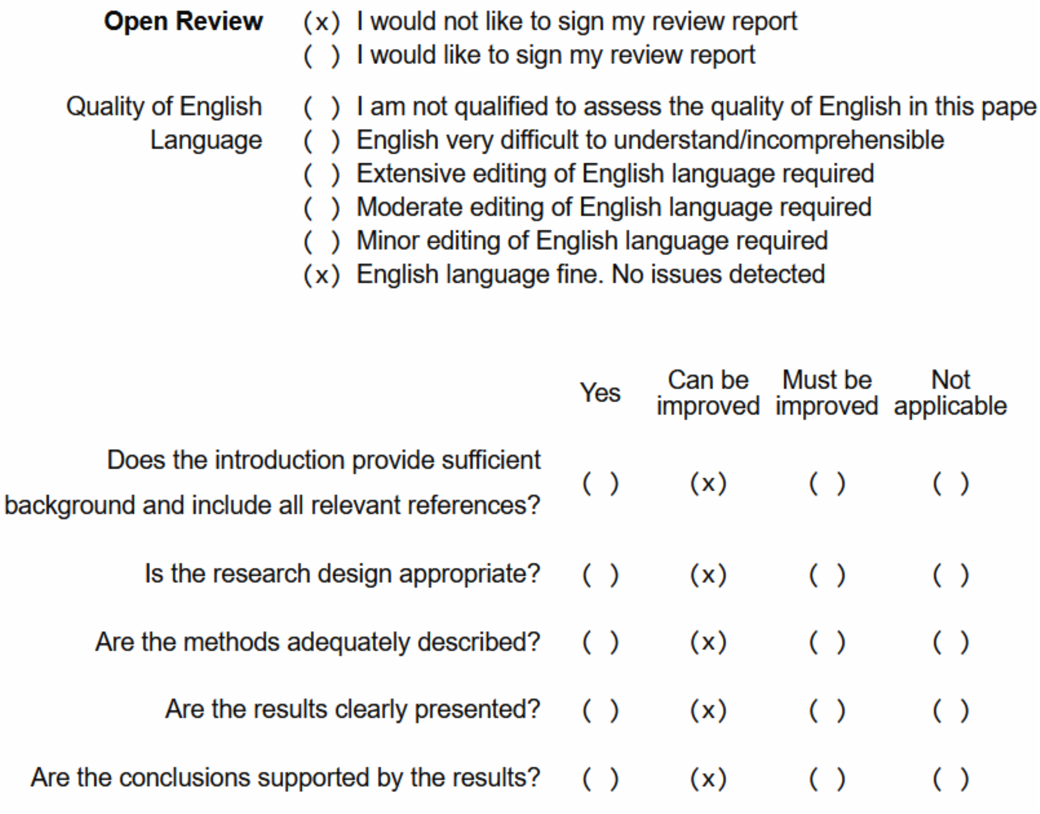
Figure 20 shows that the model, when fitted in 1958 – 2002, somewhat underestimates the $[CO_2]$ in the last few years. Figure 21 does not have any discernible visual difference in net inflow, $I - Q$, from Figure 15, in which the calibration was for the entire observation period. Table 2 shows that the parameter values changed only slightly with the change of the calibration period. Finally, Table 3, shows slight decreases of the performance indices in the period 2003 – 2023 when the fitting is made in the period 1958 – 2002. The decrease is about 3.5% in $[CO_2]$ and 1-1.5% in $I - Q$, when compared to the values of the fitting on the entire observation period. Overall, the validation results are deemed satisfactory.

R1.7. (5) The results part of Section 3 contains both method (e.g., Equations (50-65)) and background (e.g., 3.1.1) introduction. These do not belong to the results, even is not closely linked to this study topic. Please refine this part thoroughly.

I have followed this suggestion. In the revised manuscript there is the new Section 3, entitled "*Carbon Cycle: A Summary of the Established Approach*", which contains the background information formerly contained in Subsection 3.1. The next subsections in Section 3 of the original manuscript now form Section 4, entitled "*RRR Application to Carbon Cycle*".

Reviewer 2

> **R2.1.**



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Are the results clearly presented?	( )	(x)	( )	( )
Are the conclusions supported by the results?	( )	(x)	( )	( )

I am thankful to Reviewer #2 for the positive assessment of the paper.

> **R2.2.** Line 9, the expression is not accurate. Some nonlinear problems can be analytically solved.

The exact phrase in the abstract is:

> If the latter is linear, then there exists an analytical solution of the resulting differential equation…

This does not preclude the possibility that a (specific) nonlinear problem can be solved analytically. It just says that this possibility is guaranteed for a linear problem. Therefore, the statement is accurate.

Nonetheless, the reviewer is right in that specific nonlinear problems can be solved analytically. In the next sections and in Appendix A.1, these special cases of nonlinear problems admitting an analytical solution are studied in detail. But to list them in the abstract would violate the word count quota for an abstract.

> **R2.3.** The present study proposes an analytical method for Reservoir Routing problem. It is suggest refining the article title with a more specific title, including the name of the proposed method.

Following the reviewer’s suggestion, I changed the title to:

Refined Reservoir Routing (RRR) and its Application to Atmospheric Carbon Dioxide Balance

R2.4. Some quotes are unnecessary (for instance lines 24-26, 55-59).

Lines 24-36 contain the epigram (motto) of the paper:

> *What is more I loved, and still do love, mathematics for itself as not allowing room for hypocrisy or vagueness, my two pet aversions.* (Stendhal [20 (p. 111)]).

I typically use epigrams in my papers because they help the reader think beyond the formal content of the paper. I do not ask the reviewer to follow my example, only to tolerate my writing style. This particular epigram conveys a strong meaning, absolutely appropriate for the present paper. The meaning becomes even stronger by the fact that Stendhal was not a mathematician but a novel writer.

Lines 24-36 contained a biblical quotation. I thought it was relevant because it highlighted the importance of storage. However, I do not insist on including it. Rather, following the reviewer's suggestion, I removed it.

R2.5. What is the mathematical fundamental on the proposed method?

The mathematical fundamentals of the proposed method are discussed in full detail in section 2, starting from the principle of conservation of mass, expressed in Equation (1).

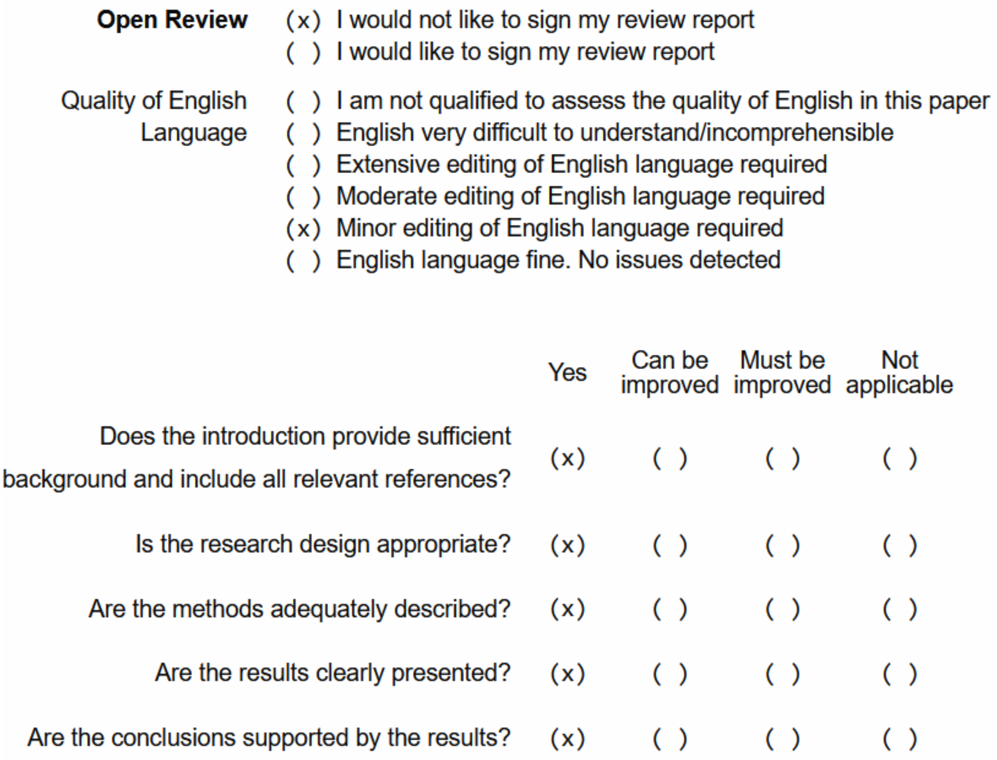
R2.6. Is the proposed method applicable to 2D or 3D problems? Please clarify it.

The following text was added below Equation (1):

> It can be seen that in the systems approach we follow, the continuity equation is unidimensional. No extensions for more dimensions are required.

Reviewer 3

R3.1.



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Are the methods adequately described?	(x)	( )	( )	( )
Are the results clearly presented?	(x)	( )	( )	( )
Are the conclusions supported by the results?	(x)	( )	( )	( )

I am grateful to the reviewer for the positive assessment of the paper.

R3.2. This manuscript proposes a simplified and extended reservoir operation framework and applies it to the mass balance study of atmospheric carbon dioxide. The application results align well with actual data, allowing for easy quantification of atmospheric carbon exchanges and providing reliable and intuitive results without resorting to complex climate models. However, the current version requires improvements before publication. It is recommended to make minor modifications based on the following comments:

I am thankful for the nice summary, as well as for the suggestions for minor modifications, which helped me expand and improve the paper.

R3.3. The introduction provides a general description of the mechanisms of carbon dioxide exchange between the atmosphere and water bodies, lacking detailed discussion and explanation of how these processes are influenced by hydrological factors.

The following three paragraphs have been added at the end of the Introduction, which I believe address this comment:

> It is worth emphasizing that the atmospheric carbon dioxide balance, like the water balance, is governed by geophysical processes, despite the common perception that it is determined by human emissions from the burning of fossil fuels. The latter represent only 4% of total emissions [21] and in this respect are similar to human emissions of water vapor, whose percentage is of the same order of magnitude [22-25]. In global hydrology, we usually neglect the human emission part, although we certainly consider

it in local studies related to irrigation. The opposite is thought in climate studies, where human emissions are seen as the cornerstone of the climate edifice. However, this is not due to the importance of human emissions but has rather been dictated by non-scientific influences [25]. Geophysically-driven emissions of both carbon dioxide and water vapor are closely linked to each other and to the biosphere processes.

Specifically, during photosynthesis, plants absorb both CO_2 and H_2O producing organic matter. The water availability drives the uptake of CO_2 through stomata, creating an interconnected cycle of gas exchange. Besides, both plants and animals respire, emitting CO_2 and H_2O, while plants also transpire. These processes determine the inflow of both CO_2 and H_2O to the atmosphere. Furthermore, decomposers break down organic material, releasing CO_2, while water facilitates the breakdown of organic compounds, influencing the decomposition rates and thus CO_2 emissions. The hydrological cycle influences plant growth by providing the water needed for photosynthesis, thereby driving CO_2 absorption. Furthermore, both CO_2 and H_2O affect the climate as both are greenhouse gases, with water being the determinant one, as, in addition to its much larger absorption of longwave radiation, it is also responsible for clouds, which also absorb radiation [25].

Of these two, we opt to study the atmospheric CO_2 balance for three reasons:

1. Its "lumping" in a systems approach is direct, because its concentration varies slowly, while that of atmospheric water varies dramatically with time, geographic location and altitude;
2. As we will see below, there is controversy about the atmospheric CO_2 budget, reflecting incomplete understanding and quantification of the processes, which the simple RRR framework may shed light on, and
3. Exporting a methodological framework developed in hydrology to the study of climate may be beneficial to both hydrology and climatology and may demonstrate the potential and usefulness of hydrology in climate research.

R3.4. Although the model shows good consistency with specific datasets, comparing it with predictions from existing complex climate models could strengthen the validation of the proposed simplified model.

I have now provided a thorough comparison with the results implied by the RRR framework on the one hand and the climate models on the other hand. Given the substantial differences between the two, I have also provided comparisons with reality. The new analysis is contained in a new Appendix, also reproduced here.

Appendix A.3: Indirect validation of the RRR results using ^{14}C isotopic data

An accidental real-world experiment, not designed as an experiment but coming up as a result of the nuclear weapons testing, allows us to calculate an upper bound of the response time of atmospheric CO_2 and thereby assess whether the claimed time lags by IPPC, reaching "*several hundred thousand years*" can have any relevance to reality or, alternatively whether the reality is that these time lags are of the order of a few years, as found in this paper.

It is reminded that carbon appears in the atmosphere, the oceans, and the biosphere in the form of the stable isotopes ^{12}C and ^{13}C at percentages of 99% and 1%, respectively [26]. It also appears in the unstable isotopic form ^{14}C, known as radiocarbon, but in trace amounts (of the order of 1×10^{-12}). As detailed by Hua et al. [27], radiocarbon is naturally produced in the upper atmosphere by the interaction of the secondary neutron flux from cosmic rays with atmospheric nitrogen isotope ^{14}N. Following its production and oxidation to CO_2, ^{14}C enters the biosphere and oceans via photosynthesis and air-sea gas exchange, respectively, providing a supply that approximately compensates for the decay of the existing ^{14}C in terrestrial and marine reservoirs.

In the 1950s and 1960s, the presence of ^{14}C was dramatically increased due to nuclear weapons testing. This produced large fluxes of thermal neutrons, which reacted with atmospheric ^{14}N to form ^{14}C. These were mostly injected into the stratosphere and subsequently transported to the troposphere. Since about 1965, the ^{14}C concentration in the atmosphere has been dropping rapidly. Given that the half line of ^{14}C is about 5700 years [28], this drop was not due to the radioactive decay but due to the CO_2 absorption by other reservoirs. Hence, the radioactive decay during these few decades can be neglected.

The use of ^{14}C data to estimate the atmospheric CO_2 residence time is not new, as it appears that it has been pioneered by Starr (1993) [29], who noted:

This study explores the plausibility of this concept, which results in much shorter atmospheric residence times, 4-5 years, than the magnitude larger outcomes of the usual global carbon cycle models which are adjusted to fit the assumption that anthropogenic emissions are primarily the cause of the observed rise in atmospheric CO_2. *The continuum concept is consistent with the record of the seasonal photosynthesis swing of atmospheric* CO_2 *which supports a residence time of about 5 years, as also does the bomb* C^{14} *decay history. The short residence time suggests that anthropogenic emissions contribute only a fraction of the observed atmospheric rise, and that other sources need be sought.*

More recently, several studies have corroborated Starr's [29] results by independent analyses. These have been produced by Berry [30,31,32], Harde, either alone [33] or in collaboration with Salby [34-36], Poyet [[37] and Stallinga [38]. On the other hand, Andrews [39], disputed these studies claiming that they are mistaken and that his analysis "*confirms the prediction of a conventional model of the carbon cycle*", but without providing any calculation to show that.

Here we perform an analysis independent of all the above, by using the rich data sets compiled by Hua et al. [27]. These include zonal, hemispheric, and global summer Δ^{14}C data sets for the period 1950–2019, as well as compiled monthly F^{14}C (and Δ^{14}C) data sets for 5 different geographical zones. All data are openly provided in spreadsheets in the Supplementary Information of the Hua et al. study. The symbols F^{14}C and Δ^{14}C denote the so-called "fraction modern" and "the per mil difference of the normalized sample / modern-carbon ratio from unity", respectively, and are defined in [40-42]. As we consistently refer to atmospheric CO_2 and since both quantities express ratios, the symbols $\Delta^{14}C$ and $\Delta^{14}CO_2$ are used here interchangeably (and likewise for $F^{14}C$).

We wish to investigate the time evolution of the radiocarbon fraction in the atmosphere, say, $F^{14}C$, and, in particular how fast this fraction converges to the pre-bomb testing minimum value $(F^{14}C)_{min}$, which can be assumed to be the naturally occurring one. We clarify that this differs from examining the concentration $[^{14}CO_2]$ per se, because the latter depends also on the total $[CO_2]$ in the atmosphere, which has been increasing for more than a century. Here the question we deal with is how fast the excess ^{14}C was removed by the biosphere, and therefore, we should isolate the study of that question from the modern increase of the total $[CO_2]$. To see that this is the reasonable approach, let us consider the imaginary case that throughout the examined period, the concentration of $[^{14}CO_2]$ was constant, while the fraction $F^{14}C$ was decreasing, e.g. at the observed rate. This would happen if the $^{14}CO_2$ absorbed by the biosphere, $[^{14}CO_2]_{ABS}$, would be replaced by that added through the total CO_2 inflow, $[CO_2]_{IN}$, that is, if $[^{14}CO_2]_{ABS} = [F^{14}CO_2]_{IN} \times [CO_2]_{IN}$, where the $[F^{14}CO_2]_{IN}$ is the isotope-14 fraction in the input CO_2. Clearly, if in this imaginary case we considered the concentration $[^{14}CO_2]$ in our calculations, we would conclude that the residence time of $^{14}CO_2$ would be infinite, because $[^{14}CO_2]$ would be constant. This is absurd because the biosphere in fact removes $^{14}CO_2$ as shown by the decrease of $F^{14}C$.

The impulses produced by the "bomb experiment" and in particular its stop at about 1965 makes a real-world situation close to an ideal to estimate an IRF of the $^{14}CO_2$ dynamics. It is reminded that, by definition, an IRF assumes zero input after the impulse, and this is precisely consistent with the above explanation as to why we should not consider the $[CO_2]_{IN}$ and hence the $[^{14}CO_2]$ in our estimation.

Based on these observations, we may form an IRF of the atmospheric $^{14}CO_2$ dynamics, by considering either of the quantities (relative differences):

$$D[F^{14}C] := \frac{F^{14}C - (F^{14}C)_{min}}{(F^{14}C)_{max} - (F^{14}C)_{min}}, \qquad D[\Delta^{14}C] := \frac{\Delta^{14}C - (\Delta^{14}C)_{min}}{(\Delta^{14}C)_{max} - (\Delta^{14}C)_{min}} \qquad \text{(A19)}$$

\r 19where $(F^{14}C)_{max}$ and $(\Delta^{14}C)_{max}$ are the maximum observations of the respective quantities, which occurred close to the year 1965, while the respective minimum values occurred in 1955 or before (depending on the geographical zone). By their definitions, both $D[F^{14}C]$ and $D[\Delta^{14}C]$ range between 0 and 1.

Figure A1 compares the temporal evolution of $D[F^{14}C]$ and $D[\Delta^{14}C]$ for the North Hemisphere (NH) zone 1, and for the period after the occurrence of the maximum until 2019, as derived from the Hua et al. [27] data. It is seen that the differences between the two are negligible and therefore the results are expected to be the same regardless of which of the two we choose to analyze.

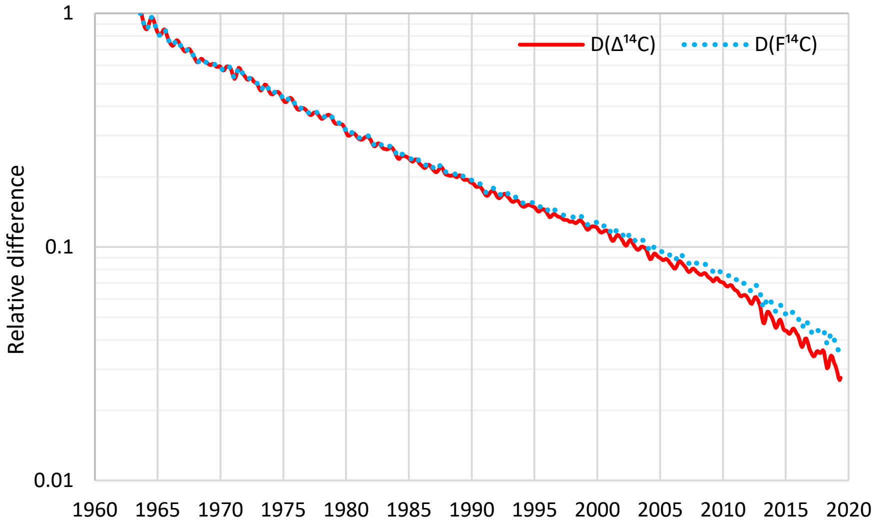

Figure A1. Comparison of $\mathrm{D}[\mathrm{F}^{14}\mathrm{C}]$ and $\mathrm{D}[\Delta^{14}\mathrm{C}]$ derived from the Hua et al. [27] data for NH zone 1.

Eventually, we choose to analyze the $\mathrm{D}[\Delta^{14}\mathrm{C}]$ series because Hua et al. [27] provide a time series also on a global basis for $\Delta^{14}\mathrm{C}$, which is depicted in Figure A2. Before we proceed to analyze this time series, it is useful to stress the following two quotations from their paper:

Decreases in atmospheric $\Delta^{14}C$ from the mid-1960s to mid-1980s are mainly due to rapid exchange between the atmosphere and the biosphere and oceans […], *while combustion of fossil fuels free of ^{14}C is the main causal factor for the $\Delta^{14}C$ decline since the late 1980s and early 1990s* […]. *Since the early and late 2000s, the atmospheric $\Delta^{14}C$ values have been lower than those of the surface waters in the North and South Pacific Gyres, respectively, indicating the oceans might become a net ^{14}C source (instead of a net ^{14}C sink) of the atmosphere* […]

The last data points in our compiled monthly data at 2019.375 have respective $F^{14}C$ values of 1.0084 and 1.0195 for the NH and SH (see Supplementary Tables 2a–e), which are very close to the pre-bomb $F^{14}C$ value of slightly lower than 1. This indicates that clean-air $F^{14}C$ is likely to reach the pre-bomb value in the early 2020s […].

The first quotation guides us to focus our model and its fitting on the period 1965 – 1985, because (a) it most faithfully reflects the system dynamics sought, i.e., the exchange between the atmosphere and the biosphere and oceans, and (b) the IRF values are higher, as are their changes in time, and hence they are more appropriate for model fitting.

The second quotation expresses a blatant disagreement with IPCC claims of time lags reaching "*several hundred thousand years*", given that the entire perturbation of ^{14}C by bomb testing disappeared in about 55 years.

If we assume that the reservoir dynamics is linear, which is the simplest and most parsimonious case, then, according to Corollary 6, the IRF will be exponential, i.e.

$$\mathrm{D}[\Delta^{14}\mathrm{C}](h) = \mathrm{e}^{-h/\mu_h} \tag{A20}$$

where μ_h is the mean response time. Hence

$$\Delta^{14}\mathrm{C} = (\Delta^{14}\mathrm{C})_{\min} + ((\Delta^{14}\mathrm{C})_{\max} - (\Delta^{14}\mathrm{C})_{\min})e^{-h/\mu_h} \tag{A21}$$

This model, with fitted μ_h = 17.2 years, perfectly describes the data for the period 1965 – 1985, as seen in Figure A2. In addition, its extrapolation for the subsequent period (without changing the fitted parameters) agrees very well with the data. Hence the simple linear reservoir is a good model for the system examined.

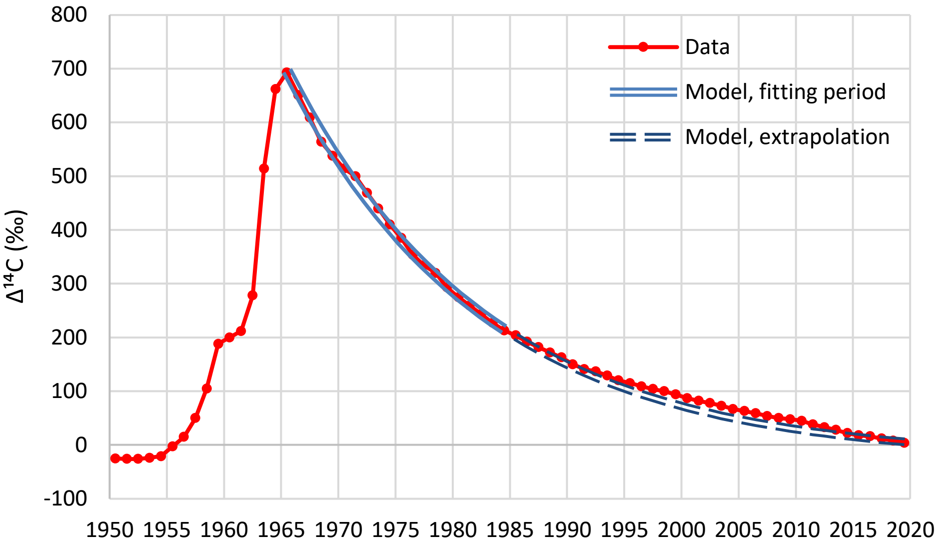

Figure A2. Global $\Delta^{14}\mathrm{C}$ time series, as provided by Hua et al. [27], and fitted linear reservoir model (Equation (A21)).

The two IRFs for the $\Delta^{14}\mathrm{C}$ case, empirical (from data) and modelled (from Equation (A20) with μ_h = 17.2 years) are compared with two IRFs that refer to the total [CO_2]. These are the IPCC model (Equation (50) with the coefficients shown in Table A1) and the linear reservoir model of the present study with μ_h = 4 years. The absolute incompatibility of the IPCC model with reality, as demonstrated through the Δ^{14}C data (and the model fitted to them, which is in perfect agreement with the data) is obvious.

The relevant question is whether or not Δ^{14}C data and model are compatible with the linear reservoir model of the present study. The answer is affirmative and the longer mean response time in the Δ^{14}C case (μ_h = 17.2 years) compared to the total [CO_2] case (μ_h = 4 years) is expected. There are three very strong reasons for this increase in response time of Δ^{14}C:

1. The absorption of the heavier isotope ^{14}C is subject to a function known as *fractionation*, that is, isotope discrimination. In particular, photosynthesis, during the exchange of O_2 and CO_2, discriminates against the heavier isotopes and, as a result, ^{14}C remains in the atmosphere for longer periods.

2. As already noted above, most of the ^{14}C produced by nuclear weapons testing was injected into the stratosphere, and the transport from the stratosphere to the troposphere is a slow process, substantially increasing the time lags.

3. While, by its definition, the IRF presupposes zero inflows after the impulse, in reality, there were additional ^{14}C inflows due to anomalous neutron flux (corresponding to a systematic increase of 5%-10% over the last 30 years according to Harde and Salby [36]). The fact that these ^{14}C inflows were not considered in the model led to an artificial increase in the actual response time.

The precise quantification of these factors is not easy and does not belong to the scope of this paper. Nonetheless, the ^{14}C analysis offers an indirect validation of the RRR results by determining an upper bound of the response time, which the RRR model respects, while the IPCC model blatantly violates.

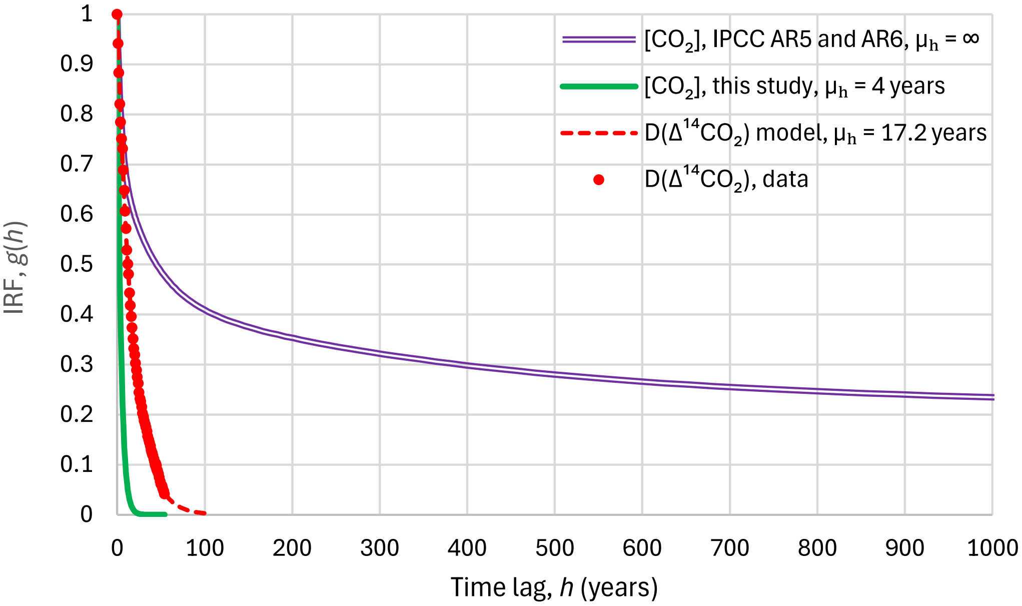

Figure A1. Comparison of two total [CO$_2$] IRFs, i.e. (a) the IPCC model (Equation (50) with the coefficients shown in Table A1), (b) the resulting from the present study (a linear reservoir model with $\mu_h = 4$ years), and two $\mathrm{D}[\Delta^{14}\mathrm{CO}_2]$ IRFs, i.e., (c) empirical (from data) and (d) modeled (from Equation (A20) with $\mu_h = 17.2$ years).

> **R3.5.** 3.The theoretical analysis section is dense, potentially challenging for readers unfamiliar with the concepts in this field. Simplifying explanations or providing visual aids could enhance understanding.

To address this comment, I reread the paper carefully and tried to improve the points that sounded difficult. In addition, I compiled a glossary, which I put as a new Appendix, reproduced below.

Appendix A.4: Glossary

Continuity equation: The equation expressing the conservation of mass, which for a reservoir with storage $S(t)$, inflow $I(t)$ and outflow $Q(t)$ is written in differential form as: $dS(t)/dt + Q(t) = I(t)$.

Impulse response function (IRF, $g_h(h)$): A system's output at a time distance (lag) h from the time in which the system is perturbed by an input that is an (instantaneous) impulse of unit mass (a Dirac delta function). It is also expressed in dimensionless form, $g(\eta) = g_h(\eta W_0)W_0$. An interesting property (Proposition 4) is that the IRF is identical to the probability density function of the residence time for the case that the input is an impulse function.

Reservoir, linear: A reservoir in which the outflow is proportional to storage. Any other type of storage–outflow relationship defines a *nonlinear reservoir*.

Reservoir, sublinear: A reservoir in which the outflow is proportional to storage raised to a power $b < 1$.

Reservoir, superlinear: A reservoir in which the outflow is proportional to storage raised to a power $b > 1$.

Residence time ($\underline{W}$): The time duration that a particle (molecule) spends in the reservoir from its entry to its exit. Excepting the (unrealistic) case of a perfectly regular (laminar) flow, the residence time is different for different molecules and is therefore represented as a stochastic variable (hence the underscore in the notation).

Residence time, characteristic (W_0): The time that is defined as the ratio $W_0 := S_0/Q_0$, where S_0 and Q_0 represent the initial conditions of storage and outflow, respectively, at time $t = 0$. In general, W_0 depends on the initial conditions. In a linear reservoir it is equal to the mean residence time, μ_W.

Residence time, mean (μ_W): The mean of the stochastic variable $\underline{W}$, which represents the residence time. It may also be expressed in dimensionless form, $\mu_w = \mu_W/W_0$. In a linear reservoir, the mean residence time is equal to the characteristic residence time $\mu_W = W_0$ and the dimensionless mean residence time is $\mu_w = 1$. In a sublinear or superlinear reservoir, a simple approximation of the mean residence time is given by Equation (41)).

Residence time, median ($W_{1/2}$): The median of the stochastic variable $\underline{W}$, which represents the residence time. It may also be expressed in dimensionless form, $w_{1/2} = W_{1/2}/W_0$. In a linear reservoir, the median residence time is smaller than the mean residence time by the factor ln 2 = 0.69. In a sublinear or superlinear reservoir, a simple approximation of the median residence time is given by Equation (41)).

Response time, mean: The mean of the IRF, in dimensional form (μ_h) or dimensionless form ($\mu_\eta = \mu_h/W_0$). In a linear reservoir, the mean response time is equal to the mean residence time and to the characteristic residence time, $\mu_h = \mu_W = W_0$, and the dimensionless ones are $\mu_\eta = \mu_w = 1$. In a sublinear reservoir, the mean response time is

> generally smaller than the mean residence time. In a sublinear or superlinear reservoir, the mean response time is determined from the exact Equation (44).
>
> *Response time, median*: The median of the IRF, in dimensional form ($h_{1/2}$) or dimensionless form ($\eta_{1/2} = h_{1/2}/W_0$). In a linear reservoir, the median response time is smaller than the mean response time by the factor ln 2 = 0.69. In a sublinear reservoir, the median response time is generally smaller than the median residence time. In a sublinear or superlinear reservoir, the median response time is determined from the exact Equation (44).
>
> *System*: A set of independent interacting elements, characterized by (a) a boundary that determines whether an element belongs to the system or the environment, (b) interactions with the environment (inputs and outputs), and (c) relationships between its elements and inputs and outputs. In its simplest form, a system transforms an input signal into an output signal.
>
> *Systems approach*: A holistic way of describing complex structures and solving complex problems, using the concept of a system, thereby simplifying the representation of a structure or a problem without requiring a detailed description of every element and process.

R3.6. 4.The abstract and conclusion are relatively simple and lack a systematic summary. For example, the practical significance of this innovative study in estimating carbon emissions or influencing policy formulation could be elaborated.

I try to be as far from influencing policy formulation as possible. In contrast, I try to be as close as possible to the classical ideal of science as the pursuit of truth. Unfortunately, policy formulation is greatly based on lies and this may have affected what it purports to be science.

As per the summary suggested by the reviewer, in the revised manuscript I have included the following text in the beginning of the Conclusions section:

> The study offers a comprehensive framework to refine reservoir routing (RRR) which is of some usefulness for several problems in hydrology, hydraulics and water management. Additionally, it offers some insights into the application of mass balance (continuity equation) with linear or nonlinear dynamics in hydrological processes and beyond, most notably in processes of the climatic system. The RRR framework includes the following features, obtained by theoretical analyses and also useful for practical problems:
>
> - It defines and clarifies the relevant quantities, including the characteristic time lags, such as residence and response times which are often confused in the literature. (The glossary presented in Appendix A.4 summarizes the related concepts and their definitions.)
> - It refines the case of a reservoir with linear dynamics, which admits analytical solutions for all related variables, and rederives and streamlines these analytical solutions.

- It classifies the cases of a reservoir with nonlinear dynamics, studies some special cases that admit analytical solutions, and provides working approximations of the outflow and the residence time, including its probability distribution and statistical characteristics.
- It provides an exact solution for the instantaneous response function and the response time, whether for the linear or nonlinear case.
- It proposes a framework for model fitting, based on observed data, for several cases, whether with linear or nonlinear dynamics.

References

1. Kundzewicz, Z.W., and Koutsoyiannis, D.: Editorial - The peer-review system: prospects and challenges, *Hydrological Sciences Journal* **2005**, 50 (4), 577–590. doi: 10.1623/hysj.2005.50.4.577
2. Koutsoyiannis, D., Blöschl, G., Bárdossy, A., Cudennec, C., Hughes, D., Montanari, A., Neuweiler, I., and Savenije, H.: Joint Editorial: Fostering innovation and improving impact assessment for journal publications in hydrology. *Hydrol. Earth Syst. Sci.* **2016**, 20, 1081–1084, doi: hess-20-1081-2016.
3. ——— *Hydrology and Earth System Sciences*, doi: 10.5194/hess-20-1081-2016.
4. ——— *Journal of Hydrology*, doi: 10.1016/j.jhydrol.2016.03.013.
5. ——— *Journal of Hydrology: Regional studies*, doi: 10.1016/j.ejrh.2016.03.002.
6. ——— *Vadose Zone Journal*, doi: 10.2136/vzj2016.01.0002ed
7. ——— *Water Resources Research*, doi: 10.1002/2016WR018895.
8. Quinn, N., Blöschl, G., Bárdossy, A., Castellarin, A., Clark, M., Cudennec, C., Koutsoyiannis, D., Lall, U., Lichner, L., Parajka, J., Peters-Lidard, C. D., Sander, G., Savenije, H., Smettem, K., Vereecken, H., Viglione, A., Willems, P., Wood, A., Woods, R., Xu, C.-Y., and Zehe, E.: Joint editorial: Invigorating hydrological research through journal publications, *Hydrol. Earth Syst. Sci.* **2018**, 22, 5735–5739. doi: 10.5194/hess-22-5735-2018
9. ——— *Ecohydrology*, doi: 0.1002/eco.2016.
10. ——— *Hydrological Sciences Journal*, doi: 10.1080/02626667.2018.1496632.
11. ——— *Hydrology Research*, doi: 10.2166/nh.2018.124.
12. ——— *Journal of Hydrology*, doi: 10.1016/j.jhydrol.2018.08.079.
13. ——— *Journal of Hydrology and Hydromechanics*, doi: 110.2478/johh-2018-0030.
14. ——— *Journal of Hydrology: Regional studies*, doi: 10.1016/j.ejrh.2018.07.003.
15. ——— *Journal of Hydrometeorology*, doi: 10.1175/JHM-D-18-0208.1.
16. ——— *Proceedings of the International Association of Hydrological Sciences*, doi: 10.5194/piahs-380-3-2018.
17. ——— *Vadose Zone Journal*, doi: 10.2136/vzj2018.06.0001ed.
18. ——— *Water Resources Research*, doi: 10.1029/2018WR024053.
19. Klemeš, V. Operational testing of hydrological simulation models. *Hydrological Sciences Journal* **1986**, 31(1), 13-24.
20. Stendhal [penname of Henri Marie Beyle]. *The Life of Henry Brulard* [Vie de Henry Brulard, translated to English by J. Sturrock], ISBN 978-1-681371-22-1, The New York Review of Books, New York, NY, USA, 1995. Available online, https://archive.org/details/stendhal-life-of-henry-brulard/ (Accessed 30 April 2024).
21. Koutsoyiannis, D.; Onof, C.; Kundzewicz, Z.W.; Christofides, A. On Hens, Eggs, Temperatures and CO_2: Causal Links in Earth's Atmosphere. *Sci* **2023**, *5*, 35. (doi: /10.3390/sci5030035)
22. Peachey, B. Mitigating human enhanced water emission impacts on climate change. In *2006 IEEE EIC Climate Change Conference*; 2006. doi:10.1109/EICCCC.2006.277221.
23. Sherwood, S.C.; Dixit, V.; Salomez, C. The global warming potential of near-surface emitted water vapor. *Environmental Research Letters* **2018**, 13 (10), 104006.
24. Li, X.; Peachey, B.; Maeda, N. Global warming and anthropogenic emissions of water vapor. *Langmuir* **2024**, 40 (14), 7701-7709.
25. Koutsoyiannis D. Relative importance of carbon dioxide and water in the greenhouse effect: Does the tail wag the dog? (Preprint – version 2), *ResearchGate*, June 2024, doi: 10.13140/RG.2.2.18964.92809

26. Graven, H.; Keeling, R.F.; Rogelj, J. Changes to carbon isotopes in atmospheric CO2 over the industrial era and into the future. *Global Biogeochemical Cycles* **2020**, 34 (11), e2019GB006170 (doi: 10.1029/2019GB006170)
27. Hua, Q.; Turnbull, J.C.; Santos, G.M.; Rakowski, A.Z.; Ancapichún, S.; De Pol-Holz, R.; Hammer, S.; Lehman, S.J.; Levin, I.; Miller, J.B.; Palmer, J.G. Atmospheric radiocarbon for the period 1950–2019. *Radiocarbon* **2022**, 64 (4), 723-745.
28. Kondev, F.G.; Wang, M.; Huang, W.J.; Naimi, S.; Audi, G. The NUBASE2020 evaluation of nuclear physics properties. *Chinese Physics C* **2021**, 45 (3), 030001.
29. Starr, C. Atmospheric CO2 residence time and the carbon cycle. *Energy* **1993**, 18 (12), 1297-1310.
30. Berry, E.X. Human CO2 emissions have little effect on atmospheric CO2. *International Journal of Atmospheric and Oceanic Sciences* **2019**, 3 (1), 13-26.
31. Berry, E.X. The impact of human CO2 on atmospheric CO2. *Science of Climate Change* **2021**, 1 (2), 1-46.
32. Berry, E.X., Nature Controls the CO2 Increase, *Science of Climate Change* **2023**, 3 (1), 68-91.
33. Harde, H. What humans contribute to atmospheric CO2: Comparison of carbon cycle models with observations. *Earth Sciences* **2019**, 8 (3), 139-159.
34. Salby, M.; Harde, H. Control of atmospheric CO2-Part I: Relation of carbon 14 to the removal of CO2. *Science Climate Change* **2021**, 1 (2), 177-195.
35. Salby, M.; Harde, H. Control of atmospheric CO2- Part II: Influence of Tropical Warming. *Science Climate Change* **2021**, 1 (2), 197-213.
36. Harde, H.; Salby, M. What controls the atmospheric CO2 level. *Science Climate Change* **2021**, 1 (1), 54-69.
37. Poyet, P. *The Rational Climate e-Book*. 2nd Edition, ISBN: 978-99957-1-929-62022, Available online, https://www.researchgate.net/publication/347150306 (Accessed 30 April 2024).
38. Stallinga, P. Residence time vs. adjustment time of carbon dioxide in the atmosphere. *Entropy* **2023**, 25, 384. (doi: 10.3390/e25020384)
39. Andrews, D.E. Correcting an error in some interpretations of atmospheric 14C data. *Earth Sciences* **2020**, 9 (4), 126-129.
40. Reimer, P.J.; Brown, T.A.; Reimer, R.W. Discussion: reporting and calibration of postbomb 14C data. *Radiocarbon* **2004** 46 (3), 1299–1304.
41. Currie, L.A.; Stafford, T.W.; Sheffield, A.E.; Klouda, G.A.; Wise, S.A.; Fletcher, R.A.; Donahue, D.J.; Jull, A.T.; Linick, T.W. Microchemical and molecular dating. *Radiocarbon* **1989**, 31 (3), 448-463.
42. Stenström, K.; Skog, G.; Georgiadou, E.; Genberg, J.; Mellström, A. A guide to radiocarbon units and calculations. Lund University, Department of Physics, Internal Report LUNFD6(NFFR-3111)/1-17/(2011), 2011. Available online, https://portal.research.lu.se/files/5555659/2173661.pdf (Accessed 30 July 2024)