Relative importance of carbon dioxide and water in the greenhouse effect: Does the tail wag the dog?

Supplementary Information: Earlier reviews and rejections of the paper

by Demetris Koutsoyiannis, National Technical University of Athens

Contents

1 Introductory notes

The Supplementary Information of the paper contains interesting material as it demonstrates the current practices of silencing voices that disagree with mainstream opinions, which are purported to be science.

The contained materials include the rejection files from three journals, namely *Hydrological Sciences Journal*, *MDPI Hydrology* and *Ecohydrology and Engineering*. The document contains all reviews and replies to them, as well as key exchanges with the journal's Editorial Offices. Replies to reviews are contained in the case that the Editor accepted the request to rebut them—otherwise no replies were prepared. In one case a reviewer of *MDPI Hydrology* was also one of *Ecohydrology and Engineering*. The replies to her/his comments are contained in Section 4.

2 Information file for the rejection by *Hydrological Sciences Journal*

2.1 Decision email

Subject: Hydrological Sciences Journal - Decision on Manuscript ID HSJ-2024-0111
Date: Tue, 27 Feb 2024 11:21:21 +0000
From: Hydrological Sciences Journal <onbehalfof@manuscriptcentral.com>
Reply-To: xxxx
To: dk@itia.ntua.gr

27-Feb-2024

Prof. Demetris Koutsoyiannis,

Thank you very much for submitting your manuscript, "Relative importance of carbon dioxide and water in the greenhouse effect: Does the tail wag the dog?" to Hydrological Sciences Journal (HSJ). We appreciate you considering our journal for your research.

All Co-editors of the journal have carefully conducted an independent assessment of your manuscript. While we recognize the effort and quality of your work, we regret to inform you that we cannot offer publication in HSJ at this time.

Our primary reasons for this decision are as follows:

Focus outside the scope of HSJ: the central theme of your study appears to lie primarily within the field of atmospheric physics rather than focusing specifically on hydrology, or its applications. While we appreciate the interdisciplinary nature of your research, HSJ's editorial board and peer-review process are best suited for manuscripts with a strong hydrological focus.

Substantial overlap with previously published work: after careful examination, it appears that the core elements and findings of your manuscript significantly overlap with your recent publication in Hydrological Sciences Journal (Koutsoyiannis and Vournas, 2024). We feel that the most relevant information for the hydrological community has already been effectively presented in your earlier work.

Given these reasons, we believe that publishing your manuscript in HSJ would not serve the best interests of either the journal or your research. However, we encourage you to submit your work to a journal with a stronger focus on atmospheric physics and sciences, where your findings are likely to receive appropriate attention and wider dissemination within the relevant field.

We apologize for any inconvenience this may cause, and wish you all the best in your future research endeavours.

Sincerely,

The Editors

Attilio Castellarin, Stacey Archfield, Aldo Fiori, Riddhi Singh, Konstantinos Soulis

Hydrological Sciences Journal

(sent by HSJ Editorial Office on behalf of the Co-editors)

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Hydrological Sciences Journal Editorial Office

IAHS UK Centre for Ecology and Hydrology
Wallingford Oxon
OX10 8BB
UK
Editor-in-chief:
A. Castellarin

Co-editors:
S. Archfield
A. Fiori
K. Soulis
R. Singh

2022 Journal Impact Factor: 3.5; 5-year JIF 3.7 (Journal Citation Reports released in 2023)

HSJ is online at: https://www.tandfonline.com/loi/thsj Volumes up to vol. 66 are free to view

https://iahs.info/Publications-News/Hydrological-Sciences-Journal.do

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** Reviewers of HSJ papers published in the previous year are acknowledged in a list published in the following year **

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2.2 Reply to decision email

Subject: Re: your recent most manuscript
Date: Tue, 27 Feb 2024 10:58:12 +0200
From: Demetris Koutsoyiannis <dk@itia.ntua.gr>
To: attilio.castellarin

Dear Attilio,

What an honour to get an unanimous rejection by the entire team of HSJ editors!

Not a surprise, but I thought I had to try HSJ, because: (a) my main theme is the importance of water, (b) this paper backs the earlier one published in HSJ, and (c) I wanted to give HSJ a chance to consider my work, which I regard of some importance.

I am very much acquainted with rejections, as well as with the fact that my rejected papers prove to be my best. So, I am hopeful for the importance of this paper. On the other hand, I am aware of the difficulties to publish it because of the prevailing of the fascist practices of eliminating diversity of opinion among publishers, editors and reviewers (while praising diversity of sexes, races, etc.).

Cheers,

Demetris
Demetris Koutsoyiannis
National Technical University of Athens

Recent publications:

Book: Stochastics of Hydroclimatic Extremes: A Cool Look at Risk, 3rd Edition
Articles: Revisiting causality using stochastics: 1. Theory 2. Applications
On hens, eggs, temperatures and CO_2: Causal links in Earth's atmosphere
Revisiting the greenhouse effect—a hydrological perspective

3 Information file for the rejection by *MDPI Hydrology*

Note 1: The decision email contained the following text:

Disclaimer: The information and files contained in this message are confidential and intended solely for the use of the individual or entity to whom they are addressed.

For this reason, I do not include it in this information file. However, its content could be inferred from my reply (and its addendum), which I include below. I also include the reviews, which I downloaded from the MDPI site containing the reviews of my paper (not copied from the decision email).

Note 2: MDPI offers the option to post the reviews online, along with the paper. I always choose this option, because I support open transactions. But it works only if the paper is accepted. This I do not like, and that is why I publish them here.

3.1 Review Report Form (Reviewer 1)

Open Review

(x) I would not like to sign my review report
( ) I would like to sign my review report
Quality of English Language
( ) I am not qualified to assess the quality of English in this paper
( ) English very difficult to understand/incomprehensible
( ) Extensive editing of English language required
( ) Moderate editing of English language required
( ) Minor editing of English language required
(x) English language fine. No issues detected

	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)	( )

Comments and Suggestions for Authors

see file [Contained in following pages]

peer-review-36288550.v2.pdf

Submission Date 27 March 2024

Date of this review 20 Apr 2024 19:58:00

Review of Manuscript ID: hydrology-2939218
"Relative importance of carbon dioxide and water in the greenhouse effect: Does the tail wag the dog?"
By Demetris Koutsoyiannis

My recommendation is that the paper must not be published. Mainly the introduction and conclusions are not supported by the results presented. The general claims of the paper are controversial and need a much better argumentation if the author wants to insist on his thesis. In such a case, I believe that Hydrology is not the right journal. For those claims, the author should seek to publish in a high-top journal on climate where there will be higher impact and more rigorous scrutiny.

I believe that the important claim about the major role of water on climate is demonstrated, along with the suggestion that hydrology should have a more prominent and more active role in climate research. Nevertheless, I believe that this is not original, there are many authors that from various perspectives have previously stressed the role of water. For instance, Newell et al (1979), IPCC, AR1 (1990), IPCC, AR5 (2013), Held and Soden (2000), Maurelis (2003), and Lu (2023) consider in detail the role of water vapor feedback on global warming. The importance of clouds and latent heating is widely recognized too. For instance, the IPCC, AR1, 1990, cap 2 says:

> *"Of the atmospheric gases the dominant greenhouse gas is water vapor, If H2O was the only greenhouse gas present then the greenhouse effect of a clear sky mid- latitude atmosphere, as measured by the difference between the emitted thermal infrared flux at the surface and the top of the atmosphere, would be about 60-70% of the value with all gases included, by contrast, it CO2 alone was present the corresponding value would be about 25% (but note that because of overlap between the absorption bands of different gases, such percentages are not strictly additive)"*
>
> *In addition, the relative strength of greenhouse gases will depend on the period over which the effects of the gases are to be considered For example, a short-lived gas that has a strong (on a kg-per-kg basis) greenhouse effect may, in the short term be more effective at changing the radiative forcing than a weaker but longer-lived gas, over long periods however the integrated effect of the weaker gas may be greater as a result of its persistence in the atmospheric.*

The thesis expressed in this paper is that none of the anthropogenic additions to the hydrological and carbon cycle drives climate. For that, he uses the (known) fact that the 9.4 Gt C/year human emissions are a small part (4%) of the total CO2 inflows to the atmosphere. But it is the change in atmospheric concentration, not the change in the fluxes that has been argued to drive climate change (no one argues that CO2 drives climate).

In the IPCC AR6 report the increase of stratospheric-temperature-adjusted radiative forcing due to doubling CO2 is estimated to be 3.9 W m^{-2}. Whereas in the paper the figure is 3.36 W m^{-2}. I believe that the difference does not support the conclusions of the paper.

The idiom in lines 838-842 and the last phrase of the title is not defensible. No one has considered that water is a minor player in climate, or that water vapor is not the most important greenhouse gas. The terminology of forcing and feedback comes from the analysis of the response to a perturbation.

Another argument used in the conclusions refers to a previous paper of the author that reportedly shows that temperature changes in the modern record precede CO2 changes. Independently of the correctness of this claim, it is not a part of the present paper. The conclusions of this paper should be supported by its results.

Some comments about the suitability of MODTRAN for the questions of the paper. As stated in lines 709- 714, MODTRAN has a fixed temperature profile based on radiative-convective equilibrium assumptions that may be appropriate in another context but not here. The role of clouds is very simplified, and other significant players are not considered (at least in the paper) like chlorofluorocarbons and aerosols.

A list of some references that I believe are important is the following:

I. M. Held and B. J. Soden. Water vapor feedback and global warming. Annual review of energy and the environment, 25(1):441–475, 2000.

Forster, P., T. Storelvmo, K. Armour, W. Collins, J.-L. Dufresne, D. Frame, D.J. Lunt, T. Mauritsen, M.D. Palmer, M. Watanabe, M. Wild, and H. Zhang, 2021: The Earth's Energy Budget, Climate Feedbacks, and Climate Sensitivity. In Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [Masson-Delmotte, V., P. Zhai, A. Pirani, S.L. Connors, C. Péan, S. Berger, N. Caud, Y. Chen, L. Goldfarb, M.I. Gomis, M. Huang, K. Leitzell, E. Lonnoy, J.B.R. Matthews, T.K. Maycock, T. Waterfield, O. Yelekçi, R. Yu, and B. Zhou (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 923–1054, doi:10.1017/9781009157896.009.

IPCC. AR5 Climate Change 2013: The Physical Science Basis; Cambridge University Press: Cambridge, UK, 2013.

Newell, R.E.; Dopplick, T.G. Questions Concerning the Possible Influence of Anthropogenic CO2 on Atmospheric Temperature. J. Appl. Meteorol. Climatol. 1979, 18, 822–825.

J.T. Houghton, G.J. Jenkins and J.J. Ephraums (eds.). Climate Change: The IPCC Scientific Assessment (1990), Cambridge University Press, Cambridge, Great Britain, New York, NY, USA and Melbourne, Australia
410 pp.

Happer, W. Why has global warming paused? Int. J. Mod. Phys. A 2014, 29, 1460003.

Maurellis, A.; Tennyson, J. The climatic effects of water vapour. Phys. World 2003, 16, 29

Colman, R. A Comparison of Climate Feedbacks in General Circulation Models. Clim. Dyn. 2003, 20, 865–873.

Colman, R.A. Climate Radiative Feedbacks and Adjustments at the Earth's Surface. J. Geophys. Res. Atmos. 2015, 120, 3173–3182.

3.2 Review Report Form (Reviewer 2)

Open Review
(x) I would not like to sign my review report
( ) I would like to sign my review report
Quality of English Language
( ) I am not qualified to assess the quality of English in this paper
( ) English very difficult to understand/incomprehensible
( ) Extensive editing of English language required
( ) Moderate editing of English language required
( ) Minor editing of English language required
(x) English language fine. No issues detected

	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)	( )

Comments and Suggestions for Authors

In this work, the author aims to demonstrate the relative importance of water in climate in comparison to carbon dioxide in order to give prominence to hydrology. This topic is very interesting, worthy of investigation and of appropriate in-depth analysis. However, I have some doubts about whether the manuscript can be accepted for publication. My first concern is on the appropriateness of this manuscript for the journal Hydrology. In fact, the main focus of the manuscript is about the relative importance of carbon dioxide and water vapor in the greenhouse effects, which is not among the scopes of Hydrology. So, in my opinion this manuscript would suit well in other kinds of MDPI journals, such as Atmosphere or Climate.

Apart from this, I found the article very confusing and difficult to be read. For this reason, I recommend a general revision of the manuscript in order to make it more readable. The last sentence in the title is funny, but sincerely I would avoid using it in a scientific paper (it would be suitable in a magazine for general audience). In the Introduction, the author questions that the classical value of science is gradually being abandoned: the author is wondering if articles published in scientific journals promote the truth or political aims. I don't want to argue whether he is right or not, but I don't think this journal is the right place for this type of discussion. In my opinion, a potential reader of the journal Hydrology is expecting to find new ideas about aspects of hydrology, e.g. groundwater, surface water, soil water, and atmospheric water, and not such kind of discussions. Similarly, I think that questions about the appearance of artificial intelligence do not have anything to do with Hydrology. In general, the introduction is full of self-quotations of the author's articles that reduce the scientific value of the manuscript. Moreover, there are many useless obvious sentences (e.g. “by UNESCO [25], hydrology is the science which deals with the waters of the Earth”). On the other side, a clear framework of the problem to be investigated is missing, as well as a clear description of the methodology proposed by the author.

The theoretical background proposed in section 2 is too much detailed and contains information and formulae that can be easily found in books for undergraduate. I assume that most of the readers of scientific journals already know these concepts. All this theory unnecessarily weighs down the manuscript and can be removed, or at least drastically shortened.

There are dozens of specific corrections that must be done, I am reporting just some of them as examples:

L 25-26: this sentence is very strong and needs further argumentation. The author provides only a self-citation, which of course is not sufficient.

L 27: similarly, I do not think this self-citation can be used.

L 69: for the generical reader, it is useless to report the ideas of an anonymous reviewer, which can be interesting only for the author of the reviewed paper.

L 212: This sentence is inappropriate... almost all people knows what π is.

I have some doubts about the scientific soundness of the results, but they will be better evaluated after a general revision of the manuscript and a resubmission to another journal. In particular, I would be more cautious in drawing such drastic conclusions (that could have a relevant impact on decision makers) only on the basis of calculations provided by a single model. As written in the manuscript, there are several codes that perform detailed modelling of radiation in the atmosphere, so it would be interesting to make a comparison among them.

Submission Date 27 March 2024
Date of this review 15 Apr 2024 14:42:36

3.3 Review Report Form (Reviewer 3)

Open Review
(x) I would not like to sign my review report
( ) I would like to sign my review report
Quality of English Language
( ) I am not qualified to assess the quality of English in this paper
( ) English very difficult to understand/incomprehensible
( ) Extensive editing of English language required
( ) Moderate editing of English language required
(x) Minor editing of English language required
( ) English language fine. No issues detected

	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)	( )

Comments and Suggestions for Authors

This paper purports to reexamine the role of water vapor and CO2 in the radiative balance of the Earth, but is overburdened with the author's editorial opinions about everything from the role of AI, the political opinions of Nature editors, the nature of truth seeking and bizarre rankings of different disciplines, along with irrelevant textbook derivations that are not used and conclusions that are unjustified by any analysis. In the meantime, the author sprinkles ad hominem arguments about other scientists throughout the text (the epigram is for what exactly?). The text has many examples of misleading statements and fallacious reasoning even before we get to meat of the work. Sections 3 and 4 are fine, until section 4.2 when the effects of clouds are discussed. The cloud modeling is extremely simplistic and unsatisfactory, but the clear-sky approximations might well be a useful contribution in a greatly slimmed down paper. The attribution calculations with MODTRAN in section 6 in the clear-sky case are fine (but limited to the specific profile chosen, not the global mean) and not inconsistent with prior work, but the purported attribution using the macro formula (Eq. 21) is fundamentally mistaken and the conclusions drawn are invalid. I therefore do not recommend publication.

Detailed comments:

l. 14 assumption of what the author is trying to prove. Also, late Holocene values of CO2 were ~280 and so the rise to 420 ppm is a 50% increase. Human activities have contributed a third of the total amount of CO2. Hardly 'small'.

l. 15 0.5% of 240 W/m2 (OLR) is 1.2 W/m2 - but this should be 1.8W/m2 (IPCC AR6, Etminan et al 2016) and so this immediately suggests a problem (see below - this appears to be because of the tropical profile used in the construction of Equation 21.

l.25-35. This belongs in an op-ed not a science article. Though it's interesting that the author appears to think that people pretending to have no political opinions (which is of course not true) is better for science than people being honest about their values and upfront about the reasons for whatever advocacy they pursue.

l.36-50. This is not an article about the accuracy (or not) of generative AI (or if it is, it does not belong in Hydrology!). All of this is a waste of space. If the author wishes to critique Lacis et al, they should just focus on what those authors said, not how it is interpreted by a bot. If that were to become the standard for science articles, knowledge production and truth seeking will collapse as scientists spend more and more time dealing with various hallucinations from large language models. The details of the author's pet peeves are really not of great interest to a science audience.

l 63. It is simply incorrect to state that increases in non-condensing GHGs have not altered the radiative fluxes in the atmosphere. High resolution spectrographic analysis has shown clearly that the spectral signatures of changes in the GHGs are visible in time series of satellite data - either across missions (e.g. Harries et al, 2001, doi:10.1038/35066553) or within a single instrument over time (Kramer et al, 2021, doi:10.1029/2020GL091585). Even at the surface, while K+V (2024) argue that the direct impact of CO2 is not detectable in noisy datasets, they don't consider the indirect impact of CO2 on the water vapor itself (the WV feedback), which is expected to have 2 to 3 times the impact on LW than CO2 alone. The inference made in KV24 and here (on line 78/79) that because a direct effect of CO2 on surface fluxes is not detectable once WV changes have been factored in means that 'the effect of CO2 is negligible' is fallacious reasoning.

l. 76. The first part of this question is a valid thing to study. The second question is nonsense.

l. 80. It's very unclear to me how any greenhouse gas is supposed to know whether LW radiation is upwelling or downwelling before affecting it. This question is therefore also nonsensical. Maybe a rewrite to state that the aim is attribute the role of H2O in both outgoing LW at the TOA and downwelling LW at the surface would be clearer. It's not obvious that the attribution would be the identical, but the notion that one could be zero and not the other is silly.

l. 82. All attribution exercises involve counterfactuals, and thus must be model based. This is neither profound nor problematic.

l. 83-85. This is totally irrelevant.

l 91. These references are to work that, if true, would undermine every calculation in this paper. Fortunately for the author they are not correct and unless one was to delve into their (obvious) flaws, I'd recommend ignoring them completely.

l. 92. Human CO2 emissions may only be 4% of total emissions, but they are effectively 100% of the long-term imbalance in CO2 fluxes.

l. 93-101. This is a significant overestimation. Anthropogenic sources of WV (directly via combustion, or indirectly via irrigation and increased evaporation) are tiny compared to the ~3mm/day global flux of evaporation. There is no validity in pretending that the affected terrestrial evaporative flux is important when this is a very small fraction of the total evaporation. First there is a typo in the text, the FAO estimates 4,300 km3/yr in withdrawals, not 4.3 km3/yr. But even using the correct number, 4300 km3/year is less than 1% of the global evaporative flux. And given the short residence time for atmospheric water vapor this can make only a very small proportionate impact on water vapor concentrations on land or anywhere.

l. 101-104. This is just a pure ad hominem argument that has no place in an scientific article.

l 129-135. This is a bizarre argument. There are literally thousands of papers a year written about the role of water in climate change - from cloud processes, the cryosphere, in the stratosphere and in the ocean and everything in between. And who has 'downgraded' the importance of the carbon cycle? Similarly, there are thousands of papers discussing this.

l. 149-203. This is standard atmospheric science textbook stuff. Not sure why it's here. Just reference a standard text, Curry and Webster (1998) for instance.

l. 211. Why is eq. (11) here? None of it is used.

l 266. There is no evidence of 'public perception' in any part of this paper (and I'm not sure of it's relevance in any case), but given that CO2 is well-mixed and WV is not, it is hardly surprising that WV variations dominate the spatial and temporal variations in downward LW. I am unclear as to why the author seems to think that Lacis et al, or anyone else thinks otherwise.

l 270. If the preceding exposition related to Penman-Monteith 'does not help' (and I agree that it doesn't). Why is it in the paper?

l. 351. Only the absolute values have the offset, however the temporal changes in the CERES fluxes are robust (and match the interannual variations in the ocean heat content (Loeb et al, 2021).

l. 470+ As the author is aware, the clouds in MODTRAN are not representative of real clouds, nor their distribution across optical depth and height. However, the clouds seen in CERES don't provide any height information, and the idea that a realistic model of the impacts of clouds on LW radiation can be formulated based on a linear function of CERES-derived cloud fraction is hopelessly naive. Indeed, it is obvious from Fig 13 that any such attempt is going to fail. [There is a more subtle point about what is meant by cloud fraction since this is - in practice - dependent on the limits of the observing platform. An argument can be made that in reality cloud cover is always 100%, but with a large range of optical depths that can go below what either our optical sensors (eyes!) or the satellite instruments can detect]. The main issue is that clouds are different heights with different temperatures and different background specific humidity and so the same cloud fraction at 1km will have a very different impact on the LW than that same fraction at 10km. This will also be a big effect on the overlaps between clouds and water vapor and clouds and CO2 that come up later in the paper.

l 644. The decrease in total outgoing radiation in the CERES period has been the subject of multiple papers Loeb et al, 2021; Raghuraman et al, 2022 ; Hodnebrog et al, 2024 and it is clear that there are multiple causes, including GHG-driven cloud and water vapor feedbacks, surface albedo changes and aerosol effects (direct and indirect via clouds). The author is not presenting an attribution study of this, and so the conclusion here is unsupported (and is in fact wrong).

l. 645 This is hopelessly naive, and ignores the role of water and cloud feedbacks on CO2-driven warming. It is also unsupported by any analysis. Delete.

l 654. I agree with the author with respect to the clear sky results, the macroscopic formulas seem to work well over their calibrated range. However, I don't know that they will extend well to the broader range required by the attribution exercise in Section 6. This requires reasonable accuracy down to zero water vapor and zero CO2, and it is not at all clear that logarithmic response to CO2 will extend that far. This should be checked by the authors.

l. 656. The clouds-related part is much less convincing and I don't think will be valid in the next section.

l. 661. This is a bizarre statement. All attribution work must be model based and revolves around counter-factuals, and this might only 'seem pointless' to someone who has never thought about it.

l 679-680. This is nonsense and based on fallacious reasoning. Sure, if you could increase WV by 30% at the same time you decrease CO2 to zero, temperatures on average would remain the same, but that would greatly increase relative humidity levels and there is no proposed mechanism for how that could be sustained on a global level. The author seems to posit that the water vapor feedback is negative - which contradicts all credible studies on the topic - and which I think would be a surprise to many!

Table 5. This is a nice summary, but the case is a little odd. These are results for a tropical profile but with a global mean surface temperature (which is 11.7 deg C cooler), and with fixed water vapor pressure, so will have a much greater amount of water vapor than the global average would have. I would suggest using a consistent profile (i.e. tropical temperatures for a tropical profile, and, midlatitude temperatures with mid-latitude profiles etc.). The impact it makes (I think) is substantial in the LW fluxes. However, the best metric to use here is G=LW_up_surf - LW_up_TOA (which should be zero in the absence of any greenhouse substances), and that is little less sensitive to the absolute temperatures. If the clear-sky attribution between CO_2 and water vapor is done in this case, it's 27% for CO_2 (28% with tropical temperature), but 31% for the mid-latitude summer case. (Percentage calculated assuming that the overlaps are split 50:50, following Lunt et al, 2021; doi:10.5194/gmd-14-4307-2021). This is very similar to the results seen in Lacis et al, 2010; Schmidt et al, 2010, where the clear-sky attribution to CO_2 in the global mean was 27% (ignoring other greenhouse substances). One minor note of caution is the fact that G in MODTRAN is not zero in the case with no greenhouse substances at all (instead it's around 1.3 W/m2) which I think is likely to be due to an issue with the MODTRAN approximations in that extreme situation.

Figure 23. This is only for the full tropical profile which (as stated above) has far more water vapor than the global average. The change assuming constant relative humidity (as opposed to specific humidity) would be nolticeably larger.

l. 747. The 'Freon' scale in MODTRAN is for all CFCs, not just Freon.

l. 769 onwards. The calculations leading to equation 27 and table 6 seem fine, but the author seems to be a little confused as to what they mean. He has calculated the gradient - with present conditions (400 ppm, etc.) of any fractional perturbation in water vapor or CO_2 or cloud, which is interesting enough, and akin to a standard 'radiative forcing' calculation (Etminan et al, 2016), but it is not the same as the contribution of each of those substances to the greenhouse effect as a whole (which is what he claims to be comparing to with Schmidt et al, 2010). Indeed, as noted above the clear sky results inferred from Table 5 are totally compatible with the Schmidt et al attribution. Additionally, Equation 21 is not defined at CO_2=0, and so cannot be used to infer the total contribution in any case (and also, it is calibrated to the tropical water vapor profile, not the global mean).

l. 774-777. This is an apples-to-oranges comparison and is simply not valid. Additionally, the 1:8 ratio for clear sky explicitly contradicts the attribution exercise in Table 5.

Figure 24. This figure is grossly misleading. The values shown are the radiative forcing ratios at CO_2=400ppm for the tropical profile, not the 'contribution of greenhouse drivers to LW fluxes'.

l 784. The values given in Table 7 are just estimates of the historical radiative forcing - but they are not very accurate becuase of the poor representation of clouds and the use of a tropical profile. Note that the IPCC (following Etminan et al 2016) has the global mean forcing from CO_2 from 300 to 420ppm as roughly 1.8 W/m2 (at the top-of-atmosphere) which is not that far from the estimates here and compatible once the tropical water vapor profile is taken into account. Note too that van Wijngaarden and Happer are also compatible with the standard estimate of the radiative forcing for doubled CO_2 (3.4 W/m2 +/- 0.6 W/m2 (95% CI)).

l. 812-823. The author should probably be aware that no-one is proposing the actual removal of all CO_2 from the atmosphere, and thus we don't need to be too concerned with the impacts that this hypothetical might have on the carbon cycle.

l. 824-837 I don't understand this argument - possibly some editing is needed?

l. 841. The author misunderstands what the forcing/feedback distinction refers to. The anthropogenic increase in CO_2 emissions (and hence concentrations) is an external perturbation to the system, to which many aspects of the climate react, including water vapor and clouds. Were the anthropogenic perturbation to water vapor larger, then it would be be treated similarly. But as it is, humans have directly caused CO_2 concentrations to increase by 50%, but have only directly caused a <1% increase in water vapor (as shown above).

l. 875. The total of human energy production is irrelevant in this context.

l. 877. It's not clear who the author is arguing with. Does anyone think that water is not important for the climate system?

l. 904-906. This is incorrect. What was demonstrate was that the interannual variations in the CO_2 rate of growth are affected by temperature variations (dominated by ENSO). The papers cited did not provide an alternative explanation for the 50% rise in CO_2 concentration since the mid-19th Century.

l. 907. These 'facts' are not supported by either data nor theory, and so it is not at all stunning that they don't impact the broader climate debate.

l. 911. This is not a valid conclusion from the work presented. Indeed, this is complete nonsense. Even if the calculations in Fig 24 were correct, they are related to CO_2 concentrations, not emissions. Thus the percentage of emissions due to humans is not the relevant metric - rather it should be the percentage of CO_2 concentrations that are attributable to human activity (which is over one third and growing). But even that isn't correct because the impact of increasing CO_2 also leads to increases in water vapor and so the ratio of the greenhouse contribution will scale with the human contribution.

l. 919. This is all nonsense as well.

Comments on the Quality of English Language

Some very minor edits needed.

Submission Date 27 March 2024
Date of this review 22 Apr 2024 21:33:59

Review Report Form (Reviewer 4)
Open Review
( ) I would not like to sign my review report
(x) I would like to sign my review report
Quality of English Language
( ) I am not qualified to assess the quality of English in this paper
( ) English very difficult to understand/incomprehensible
( ) Extensive editing of English language required
( ) Moderate editing of English language required
( ) Minor editing of English language required
(x) English language fine. No issues detected

	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)	( )

Comments and Suggestions for Authors

Please see the attachment. [Contained in following pages]

peer-review-36536530.v1.pdf

Submission Date 27 March 2024
Date of this review 29 Apr 2024 08:42:46

Review of
"Relative importance of carbon dioxide and water in the greenhouse effect: Does the tail wag the dog?"

by Demetris Koutsoyiannis

Recommendation: While many interesting calculations have been done with a well-established model, I cannot recommend publication of the article in this form because the author has set up an incomplete way of comparing the relative importance of water vapor and CO_2, totally leaving out how the atmosphere moves, vertically to maintain the profile that is presumed in all the calculations and then in three dimensions. To understand the atmosphere, one has to consider its energy balance in all four dimensions and not just the fluxes of radiation. There is wide scientific agreement that water plays a very important role in the atmosphere and in determining the extent of climate change, but by presenting an analysis that is not comprehensive, the paper fails in recognizing the role that CO_2 and greenhouse gases other than water vapor play in controlling the climate. Thus, the second phrase of the title is just not correct—the concentrations of the greenhouse gases other than H_2O control the spigot and thus controls the water vapor concentration and water vapor's feedback effect that amplifies the direct influences of the changes in CO_2 on the climate.

General Comments:

1. One of the claims I used to make of students who had been through geometry was that I could prove to them that all triangles are isosceles. I'd draw a triangle that was clearly not isosceles, draw the bisector of the angle between the two sides and then the bisector of the base of the triangle. And by getting the intersection of the two lines in what seemed a reasonable spot, carry through the proof. Of course, they'd object this could not be true, but as much as they looked at the steps in the proof, all seemed correct. What they had to figure out was that it was my original drawing, good as my approximation looked, that was at fault. This is the problem with this paper, once one accepts the premise that one can just look at the relative roles of water vapor and CO_2 in the IR fluxes to judge their relative significance, all the rest plays out (and the author even sneaks in a closer regarding the carbon cycle that is very misleading as well.

2. The global climate models that project that the rise in the CO_2 concentration is causing a significant warming have atmospheric radiation parameterizations that perform similarly to the reference codes with high spectral resolution that are used in this paper, so that is not where to look for the very different conclusion that the author comes to. The difference is that the global climate models include all the terms contributing to and affecting the global energy balance, in particular, all the terms that contribute to the time changing energy balance at each vertical and horizontal location in the atmosphere. In contrast, the author looks at only the instantaneous state of the atmosphere while specifying the vertical lapse rate and not calculating the effects of energy imbalances at each layer and location.

3. The calculations correctly show that water vapor is very important in determining the infrared fluxes of energy—there is no question about that. And fine to tout the role of hydrology and hydrologists in considering climate change and its impacts, although this paper does not at all discuss the hydrologic cycle (so precipitation, flow, evaporation, water in its various forms, and so on), so not a complete picture of water's role at all, and that is one part of the problem.

4. Regarding the calculations of atmospheric longwave radiation, which is all that is presented, there is a real failure to understand and investigate the particular, but different, roles of water vapor and CO_2 (and other of the greenhouse gases—that is, gases composed of three or more atoms) in contributing to the net energy absorption and release at various levels of the atmosphere, much less the roles of other factors such as Boyle's Law and convection that determine the vertical profile of temperature in the atmosphere—indeed, the energy balance and what maintains the lapse rate is not looked at at all—it is just imposed on the profiles that are analyzed. As it turns out, the role of water vapor in the greenhouse effect is concentrated in the lowest two or so kilometers. If there were no other factors, the heat being trapped by water vapor radiates to the surface but would also radiate to space, costing significant energy and really making leading to a cold surface temperature. In describing Figure 21 and the calculation when CO_2 is removed, but keeping the water vapor mixing ratio and lapse rate as in the profile being used, the outgoing radiation would be much greater were there no CO_2 there. So, where would that energy come from except by the surface cooling significantly. The CO_2 that is present has its effect mainly in the upper troposphere, reducing the amount of energy lost to space by the water vapor and so allowing the temperature to rise. As such, the CO_2 is acting as a spigot, controlling the loss of energy to space from water vapor and surface emissions—the more CO_2 that is there, the less the loss and so the warmer the surface temperature. Without the CO_2, the increased loss of energy by water vapor and the surface cools and then this leads to less water vapor holding capacity in the atmosphere, which reduces the water vapor greenhouse effect, which cools more, which reduces atmospheric water vapor more. And conversely, the more CO_2, the warmer it gets, the more water vapor the atmosphere can hold, the greater the trapping if energy and the warmer the surface, etc.—this is the water vapor feedback. Yes, the largest contributor in absolute amount is due to water vapor, but it is the CO_2 concentration that controls it. The work by the author simply does not consider all of this—it is the time-changing energy balance that controls the temperature, and this issue is not addressed at all, and cannot be by just including the infrared energy calculations. So, incomplete model, very misleading results.

5. A further consequence of the CO_2 blocking outgoing radiation and thus enabling additional global warming is that it reduces the amount of energy that would be drained by convection to maintain the lapse rate is greatly reduced by the role of CO_2. With just water vapor in the atmosphere, the layers above the water vapor would be very cold were the CO_2 not there and the moist adiabatic lapse rate could only be sustained by

large amounts of heat being transported up, so again, absence of CO_2 would lead to cooling and presence enables warming, in another way serving as a spigot for the role of water vapor.

6. The author then introduces another misleading point on lines 910-913 relating to the carbon cycle and the importance of CO_2 emissions. During the Holocene prior to the industrial period, there were significant CO_2 emissions from both the land and the ocean of order 100 GtC/year from each, and also a roughly equivalent return flux, keeping the atmospheric CO2 concentration near constant at about 280 ppm. Then along came the industrial revolution and humans started taking C that had been sequestered underground for many millions of years and upon combustion, such that current emissions are of order 10 GtC/year. Yes, so 10 is a small percentage of 100+100, but that is the wrong comparison. Imagine being a bank teller (being the atmosphere); a person (representing the ocean) comes in with a $100 bill and asks for change and the teller gives back ten $10 bill—same amount as in and no one has lost money; that was what was going on during the Holocene—the ocean would emit CO_2 and take back the same amount in different locations, and the atmospheric concentration remained constant. Then along comes a second customer (representing fossil fuel combustion) who has printed up an excellent quality $10 counterfeit bill and he gives it to the teller and does not take anything out. It is a small amount compared to the $100 transaction the teller just did, but in this case the teller got to keep the money. He might decide to give a quarter of it to his customer, the ocean, and a quarter to his customer, the land and so keeps half to himself. By the time the teller has been doing this for 150+ years, he's got a good bit more money—indeed now something like 40% more than when he started; even though the gain is much smaller than the amount of transactions in and out, he has gained a lot of money. Indeed that is how the business world works—or a casino—one can have lots of transactions going in and out, but what one wants is the small profit that comes when new money keeps being introduced. The whole paragraph simply does not indicate and understanding of the carbon cycle. As a result, the conclusions on lines 919-933 are simply wrong regarding the importance of the human caused changes in the atmospheric CO_2 concentration.

7. If one is going to make the case that some established understanding is wrong, one has to not only offer a contrary perspective, but also explain why the prevailing perspective is incorrect. There is really no attempt to explain this. Instead here, rather than base an analysis on a much more comprehensive representation than the perspective that the author is seeking to demean, the author greatly simplifies the representation and then asserts (without any evidence) that his view is sufficiently comprehensive to draw his conclusions. Well, this is just not the case—the perspective in the paper just assumes nothing else matters, etc. Totally inadequate justification.

Specific Comments:

Lines 10-13: As the General Comments note, regarding the role of CO2, what matters is not just its fraction of the fluxes, but where in the atmosphere the CO2 is active and how it plays an important role in determining how effective the water vapor greenhouse effect will be.

Without CO2 there, as he actually describes later, there is a quite significant temperature change, and it would be even more if he did not arbitrarily impose/assume that the lapse rate would be sustained in some way that did not further detract from the water vapor greenhouse effect.

Lines 13-15: Again, the author has failed to construct a full representation of the process involved in determining the atmosphere's temperature structure and strength of the greenhouse effect. In reality, the CO_2 concentration has control of the spigot that determines how strong the water vapor greenhouse effect is. Perhaps a metaphor would help explain—the CO_2 level is equivalent to number of ticket scanners at a concert with a large crowd (representing the water vapor greenhouse effect) wanting to get in to the concert hall. The number in the crowd is far larger than the number of ticket scanners, but the number of scanners determines the rate of people getting in. Talking percentages instead of the role played makes no sense in this situation. Increasing the number of ticket scanners by an amount analogous to increasing the CO2 concentration from 300 to 420 would make a huge difference on the throughput of concert attendees into the arena.

Line 22: A really inappropriate quote for an article asserting it is an authoritative view of a scientist on an issue.

Lines 25-35: More appropriate for a letter to the editor or an issue on problems in science than in an article that is seeking to present factual information on science. And far too much self-citation. If this is to be an article about correcting something that is wrong with some aspect on science, it should be focused on presenting solid science and not a personal perspective.

Lines 36-62: It is really not clear that any authority thinks that AI is to be trusted—strange to include.

Lines 63-74: A bit argumentative

Lines 76-81: Nice to pose questions to be addressed, but it would be much more appropriate to phrase the questions in a less biased way. Fine to investigate the relative roles of CO_2 versus and in concert with water vapor. Suggesting that the view of the importance of CO_2 is limited to one author versus the author of this paper is totally inappropriate—it is the understanding of the operation of the scientific community for over a century that is being questioned and being argued against by the author of this paper and few others. If arguing that a prevailing scientific view/paradigm should be overturned, it is generally expected that there needs to be a very clear strong case to be made. Making this personal is just not appropriate—indeed, the argument needs to be based on the science and it is this that needs to be explained.

Reviewer Comment: The prevailing scientific view makes very clear that the role of water vapor is very important, as are the various states of water—gaseous, liquid, and solid. In addition, the prevailing scientific view makes clear that atmospheric radiation plays a very important role. Where the scientific view seems to be different is that it insists that all processes of the Earth system need to be considered quantitatively, not just radiation and not just water vapor, and this needs to be done at all locations in latitude, longitude, and altitude in an integrated way. The introduction to this paper, at least to this point, has not made this clear nor really gotten to where the differences in view might be (e.g., the second question is about one particular aspect—and one that is drawn from one single comment of what is an un-named reviewer—any text of atmospheric radiation would not support the statement that water vapor also affects the outgoing LW flux, so this question is really setting up a straw argument than getting to the essence of the question—the relative and interacting roles of water vapor and other gases (especially CO2).

Lines 88-91: Glad to see the authors agree that models need to be used to understand because they provide the ability to understand observations and what changes to various conditions like atmospheric composition can do to affect the system.

Line 92: This statement represents an out of context and very incomplete representation of the carbon cycle. Throughout the Holocene prior to the initiation of widespread combustion of fossil fuels, various observations (e.g., ice core records—and thank you to solid water for keeping this record of the CO_2 concentration) indicate that the atmospheric CO_2 concentration was relatively constant with natural emissions being matched by natural uptake. What ice core and other proxy and instrumental records indicate is that, since the start of combustion of fossil fuels, the atmospheric concentration has been rising by an amount that is only about half of each year's expected increment to the atmospheric concentration resulting from human-generated fossil fuel emissions (strictly speaking, as is appropriate to scientific discussions, they are not emissions from humans that are being considered), suggesting that the rate of uptake by natural responses has been rising in response to the higher atmospheric concentration. What would better be said is that emissions of CO_2 from combustion of fossil fuels has been gradually rising since the start of the industrial revolution and now represents about 4% of the amounts of natural release and uptake of CO_2 by the ocean and terrestrial biosphere. One has to say 4% of what, and say it accurately.

Lines 92-93: It is totally unexplained why such a comparison should be made, and why it should be that equal amounts or percentages should be the basis of the comparison.

Lines 101-105: Yes, one "could speculate" about this, but again, still no real basis for the comparison, which is needed to figure out what the appropriate metric for the comparison should be. It is suggested that the metric should be a "stronger greenhouse gas" but what is meant by that is not yet explained, nor who they interact, what conditions apply, etc. Perhaps we'll get to it, but just because water vapor is said to have the strongest greenhouse effect as measured at the surface does not mean that this is all that matters in affecting the global climate. Basically, the author is presenting a perspective that seems very narrow (to this point, focused solely on certain elements just at the surface, etc.) when considering behavior of the global Earth system.

Lines 119-126: Well, finally, what the focus of this paper is to be about. Where the assertion on lines 124-126 comes from seems purely a personal view with no documentation nor investigation yet as to why the supposed relative level of attention may seem to be the case.

Lines 132-136: More assertions, making the issue seem like a competition between practioners, a jealousy about the number of articles, etc. And no indication that water has been the story for a very long time with very significant human modification over time and been much studied and so quite well understood, whereas humans adding substantial and growing amounts of CO_2 to the system has only been intensively looked at for several decades and so the is much to be studied, including concerns (substantiated by, for example, the different climates of Mars and Venus and the potential for very large changes with ongoing CO_2 emissions affecting its atmospheric concentration).

Lines 137-140: More indication that this is more a clamoring for attention by scientists in a particular understanding rather than a real search for understanding how human activities are affecting the current and future climate. I might just note that there is now much attention to what is happening and will be happening to the Antarctic and Greenland ice sheets—frozen water—does that count as hydrology or glaciology, etc.? Of course, climate science focuses on the importance of water vapor—is the problem that they are not called hydrologists? This is getting to sound pretty petty.

Lines 150-203: Section 2.1, it is not at all clear why this matters. It is based on standard conditions, but conditions over the surface of the Earth vary widely and by altitude vary greatly.

Line 218: What is meant by saying the temperature of the surface of the Earth is well-determined? It varies widely by location and varies through the day, from day-to-day, by season, from year-to-year, and has been changing by decade—and over Earth's history has varied greatly in most locations compared to what is experienced at present. And the reported observations of temperature are in a shaded shelter about 2-m above the surface. Given there is a fourth power dependence on temperature, even relatively small variations can lead to quite a different value of the upward directed flux.

Lines 219-221: Similarly, conditions, including temperature, water vapor loading, cloudiness, etc.) in the atmosphere vary greatly by longitude, latitude, and altitude. And the downward flux varies by altitude (as does the upward flux) due to absorption along the way, etc. The notion that one can determine the climate by such idealized conditions alone is really naïve if that is where this discussion is headed.

Lines 265-267: This conclusion is simply not justified. First, while the LW flux from water vapor might give an indication of water vapor's influence on downward radiation under standard conditions, conditions vary all over the Earth. Second, the Earth's climate depends on more than the IR flux at the surface—what happens to the climate as a whole depends on what is going on throughout the atmosphere and requires full consideration of the energy balance of the Earth system, which is not determined by just what is happening at the surface. Third, there is no allowance for accounting for changes in atmospheric composition or other changes—what about how the climate responds to volcanic eruptions, what about effects of air pollution on fluxes in some regions and not others, what about explaining glacial-interglacial cycling, what about explaining the Cretaceous, what about changes in sea level or ocean acidification, etc.? The rules of thumb that are mentioned in this section are fine for the purposes they are intended—they are not sufficient to describe the full behavior of the Earth system.

3.4 Reply to decision email

Subject: Re: [Hydrology] Manuscript ID: hydrology-2939218 - Update
Date: Wed, 29 May 2024 12:49:45 +0300
From: Demetris Koutsoyiannis <dk@itia.ntua.gr>
To: xxxxxxxx
CC: Hydrology Editorial Office <hydrology@mdpi.com>

Dear Ms. xxxxxxxx,

Thank you for your message. Here is a brief reply.

-- You state that you "can guarantee for the high qualification of all of them" [the reviewers]. It is pointless to do that. I can judge myself about their qualification, given that they are all anonymous, repeatedly use offending characterizations such as "nonsense" in their assessments of my writings, and also use expressions, the meaning of which they do not understand (for example calling "ad hominem arguments" the arguments that refer to positions of persons, and are not against the persons per se). Their critique on my first paragraph precisely justifies why that paragraph is necessary. In fact, their reactions offer confirmation of the validity of the first paragraph, and I am disappointed that you and the Editor did not fully grasp this.

-- You state "for the subject matter covered, is not suitable for publication in Hydrology". That would be fine at an initial stage, in which the Editor would had found it unsuitable for the journal. But he didn't, and using the reviewers' arguments for asserting unsuitability at this phase, is something I deeply regret as I cannot think of a valid justification for it.

-- You state that, according to the reviewers, my paper "contains a series of inaccuracies in the application of the methodology and in the interpretation of the results." You and the editor did not wonder whether it is not my paper that contains inaccuracies but the reviews. I can infer that with safety, given that you did not offer me the option to rebut the reviews.

-- At the beginning of the review phase, you conveyed me the Editor's recommendation to publish a preprint in MDPI's /Preprints/ platforms. I immediately accepted his recommendation and published it. The platform offers the possibility to post review comments. Until now, no negative comment has been posted. Please convey to the "high qualification" reviewers my willingness to reply to their comments, in case they post these publicly on the /Preprints/ platform.

I appreciate our collaboration in /Hydrology/ all these years. As I now feel that the editorial standards of the journal do not correspond to the scientific standards that I believe necessary, please remove my name from the journal's Editorial Board.

Kind regards,

Demetris Koutsoyiannis
National Technical University of Athens
Recent publications:
Book: Stochastics of Hydroclimatic Extremes: A Cool Look at Risk, 3rd Edition
Articles: Revisiting causality using stochastics: 1. Theory 2. Applications
On hens, eggs, temperatures and CO2: Causal links in Earth's atmosphere
Revisiting the greenhouse effect—a hydrological perspective
Net isotopic signature of atmospheric CO_2 sources and sinks: No change since the Little Ice Age

3.5 Addendum to reply to decision email

Subject: Re: [Hydrology] Manuscript ID: hydrology-2939218 - Update
Date: Fri, 31 May 2024 09:13:00 +0300
From: Demetris Koutsoyiannis <dk@itia.ntua.gr>
To: xxxxxxxx
CC: Hydrology Editorial Office <hydrology@mdpi.com>

Dear Ms. xxxxxxxx,

You did not respond to my earlier email.

I asked you to remove my name from the Editorial Board of /Hydrology/ but you did not.

Please do remove it immediately. I explained my reasons in my earlier email.

In addition, please update the status of my paper:

It should state "Rejected", not "Under review", because I want to submit it somewhere else.

Kind regards,

Demetris

PS. I have OCRed the reviews you sent me, and I am attaching the four reviews in searchable format. You and the Editors may wish to search for either of the words "partial" or "derivative" to see if they appear. My search result is that neither of the two words appears in none of the four reviews. Nb., the use of the partial derivatives is the most novel part of my paper, and the most (if not the only) scientific approach to the problem I researched. I hope the Editors can understand that, and perhaps ask themselves why the "high qualification" reviewers avoided to discuss the most novel idea of my paper.

4 Information file for Review Round 1 (Major revision) in *Ecohydrology and Engineering*

4.1 Introductory notes

The manuscript "*Relative importance of carbon dioxide and water in the greenhouse effect: Does the tail wag the dog?*" was submitted on 07 June 2024 to *Hydroecology and Engineering* (Manuscript ID hee00317), after an invitation by the Editorial Office of the journal. It received three reviews, one with constructive comments aiming at its improvement, and two with strongly negative comments aiming at forcing rejection of the paper. The editor's decision was a major revision. As seen below, where all review material is reproduced, I have addressed all constructive review comments and rebutted all negative comments in full detail.

It may be easily seen that the strongly negative reviewers did not spot any error in the mathematical part. What is more, they did not locate at all the substantial mathematical advance contained in my paper, which is the construction of generalized equations describing the effect of the different constituents of the greenhouse effect, and the use of partial derivatives. The latter part of the proposed methodology is the key approach to assessing the relative importance of different factors and has not been used before in relevant studies.

It is easy for the Editor to confirm this claim of mine. A simple search of each of the words "partial" or "derivative" reveals that these reviewers never used either.

As stated in the Acknowledgments section of both the original and revised manuscript, it was rejected before by two journals, the first time without review and the second time after review. I have posted a preprint of the paper on two platforms, Preprints and ResearchGate. In the latter, I also posted all rejecting review material (see links below, in the replies to Reviewer #1). As can be seen below, Reviewer #1 of the current submission is the same as Reviewer #3 of the earlier submission.

Key:

Review comment.

Response.

> Quotation from manuscript.
>
> Quotation from other documents.

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.

4.2 Reviewer 1

R1.1.

Reviewer review report

Overall Recommendation	( ) Accept in present form ( ) Accept after minor revision(I do not need to see the revised version) ( ) Reconsider after major revision(I want to see the revised version) (✓) Reject
English Language and Style	( ) Extensive editing of English language and style required ( ) Minor spell check required (✓) English language and style is fine ( ) I don't feel qualified to judge about the English Language and Style
Recommendations for Authors	★★☆☆☆ Is the manuscript novelty enough? ★☆☆☆☆ Is the manuscript scientifically sound and not misleading? ★★☆☆☆ Does the manuscript give a concise and comprehensive view of the topic? ★★☆☆☆ Is the manuscript well organized and of proper length? ★★☆☆☆ Is an adequate number of references investigated and appropriately quoted?

Interestingly, Reviewer #1 finds the manuscript neither novel enough nor scientifically sound. Yet it is readily understood, without any effort, that the paper is novel. For example, just viewing the figures of the paper, one can see that out of 26 figures in the body of the paper, 24 are new, having not appeared anywhere else before. One can also see that the concept of partial derivatives, which is proposed as the key approach to assessing the relative importance of different factors, has never been used before in relevant studies.

It is also interesting that Reviewer #3 (see below) concurs with Reviewer #1 in terms of the novelty and scientific soundness of the paper, while Reviewer #2 has the exact opposite opinion: five stars for novelty and five stars for scientific soundness.

R1.2. This is essentially the same report that I gave for the previously to another journal for the same submission. Since absolutely nothing has changed - not even the typos - I feel justified in repeating the critique in full. I suggest that the author actually engage with the critique to avoid wasting everyone's time in future.

Indeed, it is the same paper that was rejected by two other journals. Actually, as I always try to practise transparency, which I believe is essential for scientific progress, I have taken the following steps: (a) I notified the editor of *Hydroecology and Engineering* about the previous rejections of the paper, which I submitted precisely in the form it was rejected before in *Hydrology*; (b) I have posted a preprint in ResearchGate [1] (in which I corrected the typos mentioned by the reviewer); and (c) I have posted the rejecting reviews and my replies to the Editorial Office of *Hydrology* [2]. The latter post includes the original review by Reviewer #1, as well as my view on this, as conveyed by me to the Editorial Office of *Hydrology*.

Naturally, I had no reason before to deal with an attacking, non-constructive review, which repeatedly uses offending characterizations such as "nonsense"[1], and which clearly aims at blocking my paper, by persuading the Editor that it should be rejected. Apparently, the reviewer was successful as indeed the Editor of *Hydrology* was persuaded, and my paper was rejected.

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[1] I reported that to the Editor of *Hydrology*. I believe he ought to react to this, but he did not.

But I do not give up and that is why I submitted it to *Hydroecology and Engineering*. Actually, I have published very many papers on climate[2] criticizing the mainstream "climate science" dogmas, and I struggled to publish each one of them (see also my reply to Reviewer #2, Comment R2.3). Some of them are cited in the present paper, which irritated Reviewer #3. Till now no error has been spotted in any of my papers.

Since the Editor of *Hydroecology and Engineering* tentatively approved the paper pending major revisions, I feel I must now put the effort to rebut this offensive and non-constructive review, which, notably, did not find any error in the mathematical part. My aim is not to persuade the reviewer, as I believe this is hopeless, but to persuade the Editor that my paper is important, novel and correct. In this respect, I rebut below the reviewer's comments, one by one and in full detail. I hope the Editor will understand and excuse my defensive tone, given the offensive style of the review.

> **R1.3.** ----
> This paper purports to reexamine the role of water vapor and CO2 in the radiative balance of the Earth, but is overburdened with the author's editorial opinions about everything from the role of AI, the political opinions of Nature editors, the nature of truth seeking and bizarre rankings of different disciplines, along with irrelevant textbook derivations that are not used and conclusions that are unjustified by any analysis. In the meantime, the author sprinkles ad hominem arguments about other scientists throughout the text (the epigram is for what exactly?). The text has many examples of misleading statements and fallacious reasoning even before we get to meat of the work. Sections 3 and 4 are fine, until section 4.2 when the effects of clouds are discussed. The cloud modeling is extremely simplistic and unsatisfactory, but the clear-sky approximations might well be a useful contribution in a greatly slimmed down paper. The attribution calculations with MODTRAN in section 6 in the clear-sky case are fine (but limited to the specific profile chosen, not the global mean) and not inconsistent with prior work, but the purported attribution using the macro formula (Eq. 21) is fundamentally mistaken and the conclusions drawn are invalid. I therefore do not recommend publication.

There is no ad hominem argument in my paper. The reviewer may consult any dictionary[3] to see that an "ad hominem" argument is one that is directed against a person rather than the position that that person is maintaining. The term and the concept are originally Greek, "προς τον ἄνθρωπον" and is posited as opposite to "πρὸς τὸν λόγον", and we owe it to Aristotle[4] (see additional information in [3]).

I reread my paper and did not find any case in which I direct arguments against any person. I criticize their work and positions, not their characters and lives. Interestingly, the reviewer does not specify which of my statements she/he regards as "ad hominem arguments". In her/his original review for

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[2] 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.

[3] E.g., starting from https://www.google.com/search?client=firefox-b-d&q=ad+hominem

[4] Aristotle, "Σοφιστικοί Έλεγχοι", ("On Sophistical Refutations"), Section 22; Greek text in https://el.wikisource.org/wik/Σοφιστικοί_Έλεγχοι/2; English translation by W. A. Pickard-Cambridge in http://classics.mit.edu/Aristotle/sophist_refut.3.3.html. The exact phrase is: οὗτοι πάντες οὐ πρὸς τὸν λόγον ἀλλὰ πρὸς τὸν ἄνθρωπον λύουσιν" ("all these persons direct their solutions against the man, not against his argument").

Hydrology, she/he mentioned one particular instance, which she/he deleted in the current version of her/his review. That particular statement in my paper is:

> One could speculate that each of these 4% additions might influence the climate to a degree comparable to that percentage, but the reasons that only the influence of CO_2 is investigated and highlighted by the scientific community, being regarded as a control knob of climate (even though H_2O is much stronger as a greenhouse gas) are not scientific.

The comment that she/he wrote (which appears in [2], posted online by me) and is now deleted, was:

> This is just a pure ad hominem argument that has no place in an scientific article.

I really wondered who the particular “ἄνθρωπος/homo” was and what my particular “πρoς τον ἄνθρωπον/ad hominem” argument was, specifically. I have not been able to spot either. I welcome her/his deletion now, but I note that she/he kept her/his general accusation that I sprinkle ad hominem arguments about other scientists throughout the text, without specifying any instance where I did that. This very point is suggestive of the overall quality of the review and that is why I have discussed it in detail.

Furthermore, the reviewer wonders, “the epigram is for what exactly?”. I often use epigrams, borrowed from intellectuals. My purpose is to make readers think about them. Hence, I am inviting the reviewer to think about it.

Following Reviewer #2’s suggestion (her/his Comment R2.3), I have now changed the Introduction. However, I insist that the original Introduction was appropriate and with high explanatory power—in particular, in explaining the hostile reactions to the present paper. Therefore, I include the original Introduction in the present Report as an Appendix.

Interestingly, the reviewer found that some parts of the paper are fine, but (to use her/his expression) the “meat of the work” is what she/he found “misleading statements”, “fallacious reasoning”, “extremely simplistic and unsatisfactory”, “fundamentally mistaken” and “inconsistent with prior work”. Indeed, many of my results are “inconsistent with prior work” and here exactly lies the importance and usefulness of the paper.

> **R1.4.** Detailed comments:
> Abstract. assumption of what the author is trying to prove. Also, late Holocene values of CO2 were ~280 and so the rise to 420 ppm is a 50% increase. Human activities have contributed a third of the total amount of CO2. Hardly 'small'.

The abstract is correct. The exact phrase is

> The minor effect of carbon dioxide is also confirmed by the small, non-discernible effect of the recent escalation of atmospheric CO_2 concentration from 300 to 420 ppm.

Recent is meant to be a century-long, in accord with what was found in the study by Koutsoyiannis and Vournas [4], focused on the subject referred to in this statement.

The abstract does not refer to human activities. Nonetheless, the reviewer’s statement referring to the contribution of human activities is an estimate or opinion and not a fact. This opinion has been disputed in the last five years in a number of papers by Koutsoyiannis and Kundzewicz [5], Koutsoyiannis et al. [6,7,8], Koutsoyiannis and Vournas [4] and Koutsoyiannis [9,10].

> **R1.5.** 0.5% of 240 W/m2 (OLR) is 1.2 W/m2 - but this should be 1.8W/m2 (IPCC AR6, Etminan et al 2016) and so this immediately suggests a problem (see below - this appears to be because of the tropical profile used in the construction of Equation 21.

I do not refer to the IPCC estimate, but to the results of the paper, which may not be consistent with the IPCC. I am doing an independent study.

> **R1.6.** Introduction: This belongs in an op-ed not a science article. Though it's interesting that the author appears to think that people pretending to have no political opinions (which is of course not true) is better for science than people being honest about their values and upfront about the reasons for whatever advocacy they pursue.

Everybody has political positions, but advocacy for them does not belong to the domain of science. I try to adhere to the classical ideal of science as the pursuit of the truth, independent of political and economic interests. Nb., as mentioned in the Funding Information section, the research of this paper was conducted out of scientific curiosity and received no funding. Therefore, I believe I am close to this ideal.

> **R1.7.** p2. This is not an article about the accuracy (or not) of generative AI (or if it is, it does not belong in Hydroecology and Engineering). All of this is a waste of space. If the author wishes to critique Lacis et al, they should just focus on what those authors said, not how it is interpreted by a bot. If that were to become the standard for science articles, knowledge production and truth seeking will collapse as scientists spend more and more time dealing with various hallucinations from large language models. The details of the author's pet peeves are really not of great interest to a science audience.

Following Reviewer #2's suggestion (her/his Comment R2.3), I have now changed the Introduction and removed the information about the bot. However, I insist that the original Introduction was appropriate and with high explanatory power—in particular in explaining the hostile reactions to the present paper. Therefore, I have included the original Introduction to the present Report as an Appendix.

> **R1.8.** It is simply incorrect to state that increases in non-condensing GHGs have not altered the radiative fluxes in the atmosphere. High resolution spectrographic analysis has shown clearly that the spectral signatures of changes in the GHGs are visible in time series of satellite data - either across missions (e.g. Harries et al, 2001, doi:10.1038/35066553) or within a single instrument over time (Kramer et al, 2021, doi:10.1029/2020GL091585).

As stated in Koutsoyiannis and Vournas [4]) (Appendix B):

> Harris et al. (2001) analysed the difference between the spectra of the outgoing longwave radiation of the Earth as measured by orbiting spacecraft in 1970 and 1997 and found differences in the spectra that point to long-term changes in atmospheric CO_2 and other gases related to the greenhouse effect. Their study considered the profiles of atmospheric temperature and water vapour, but did not give any hint about the relative contribution (and importance) of water vapour in comparison to these gases. In a macroscopic approach, as the

> one followed in this paper, it is the total longwave radiation flux, rather than the changes in the spectrum for particular frequencies, that counts most.
>
> […]
>
> [A]ll indications suggest that at the macroscopic level (global fluxes) the changes observed are related to water, while nothing related to CO_2 is discerned. This suggests a minor role of CO_2, as substantiated by van Wijngaarden and Happer (2020) and de Lange et al. (2022), and also expressed by Smirnov and Zhilyaev (2021, p. 197) in their statement “water molecules in the atmosphere may be responsible for the observed heating of the Earth”, which is in “contradiction with the results of climatological models in the analysis of the Earth’s greenhouse effect”. The difference in the conclusions of Harris et al. (2001) on the one hand and the last three studies mentioned just above on the other hand, must lie in the fact the latter investigated the entire range of longwave frequencies from 0 to 2500 – 2600 cm^{-1}, while the former examined frequencies in the range of 700 to 1400 cm^{-1}. Interestingly, the frequencies left out in Harris et al. (2001) (i.e. < 700 cm^{-1} and > 1400 cm^{-1}) are precisely those fully dominated by water molecules.

Furthermore, both Harries et al (2001) and Kramer et al (2021) used clear-sky-only data to arrive at their conclusions that the greenhouse effect has been altered by CO_2. Kramer et al. purported to find results for all-sky conditions but they did that by converting the clear-sky data by their equation (8), not by using all-sky observed data. Subsequently, they admitted that:

> This conversion to all-sky conditions accounts for the presence of clouds but not cloud changes.

Therefore, their results do not apply to real-world conditions, as clouds exist and are present about 70% of the time (see Figure B2 in Koutsoyiannis and Vournas [4]). When all-sky data are used to find results for the real world, the greenhouse effect trend goes from increasing (clear-sky-only) to declining/flat (all-sky).

Specifically, Koutsoyiannis and Vournas [4] (Appendix B) note:

> The CERES TOA time series of longwave radiation fluxes for clear sky and all sky conditions are shown in Figure B1 [Figure R1]. The graphs also show the linear trends in the two cases, which were estimated for full years only (i.e., 23 years for the Terra platform and 20 years for the Aqua platform, leaving out a number of values exceeding a multiple of 12). The linear trends are very small. What is more interesting is that, while they are slightly negative for clear sky, they become slightly positive for all sky. This does not suggest that they are linked to CO_2 changes. Rather a link to atmospheric water is more likely, as it can be conjectured that they reflect changes in the temperature and water vapour profiles in the atmosphere and hence in the formation and vertical profiles of clouds.

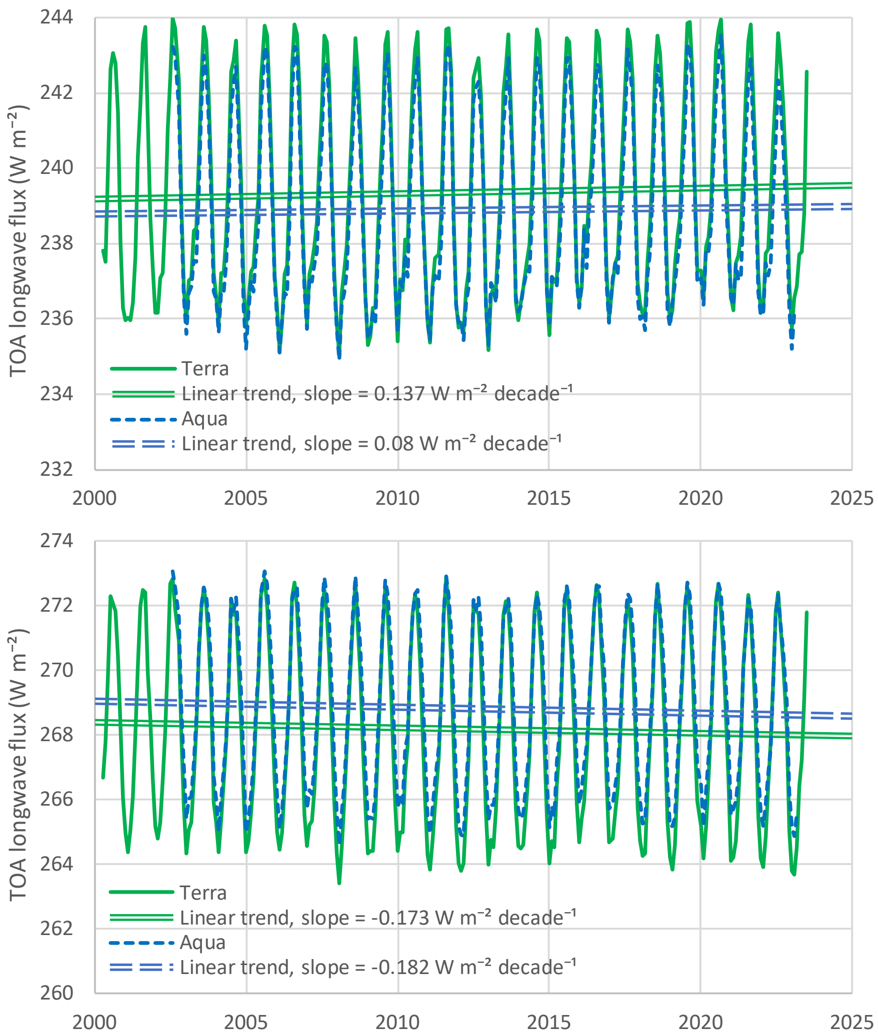

Figure R1 [TOA means top of atmosphere; original caption in paper follows]
Figure B1 TOA time series of longwave radiation fluxes, as provided by NASA's CERES, along with linear trend, for (upper) all sky and (lower) clear sky.

Similar are the results by Song et al. [11], who note:

> The total column precipitable water (TCPW) anomaly significantly increases at a rate of 0.44 cm yr^{-1}. However, the cloud area fraction (CAF) anomaly is reduced by −0.60% yr^{-1} […]. Therefore, although the greenhouse effect can be enhanced by increasing GHGs and water vapor in the atmosphere, it can be weakened by decreasing clouds. If these two actions offset each other, a hiatus of the global greenhouse effect will result. To confirm this, the variations of G_{aa} and G_{sa} in all-sky conditions are compared with those in clear-sky conditions in [Figure R2]. The clear-sky atmospheric and surface greenhouse effect parameters increase significantly at rates of 0.22 W m^{-2} yr^{-1} and 0.19 W m^{-2} yr^{-1}, respectively. However, the atmospheric and surface greenhouse effect parameters both become trendless when clouds are considered. […] Overall, the downward tendency of clouds is the dominant contributor to the greenhouse effect hiatus. […] Consequently, a zero-trend greenhouse effect is achieved under the balance of its primary contributors (e.g. water vapor, clouds, and GHGs). Finally, the hiatus of the greenhouse effect-driven warming leads to the recent global warming slowdown, in which the atmosphere traps (emits) near constant heat from (to) the surface.

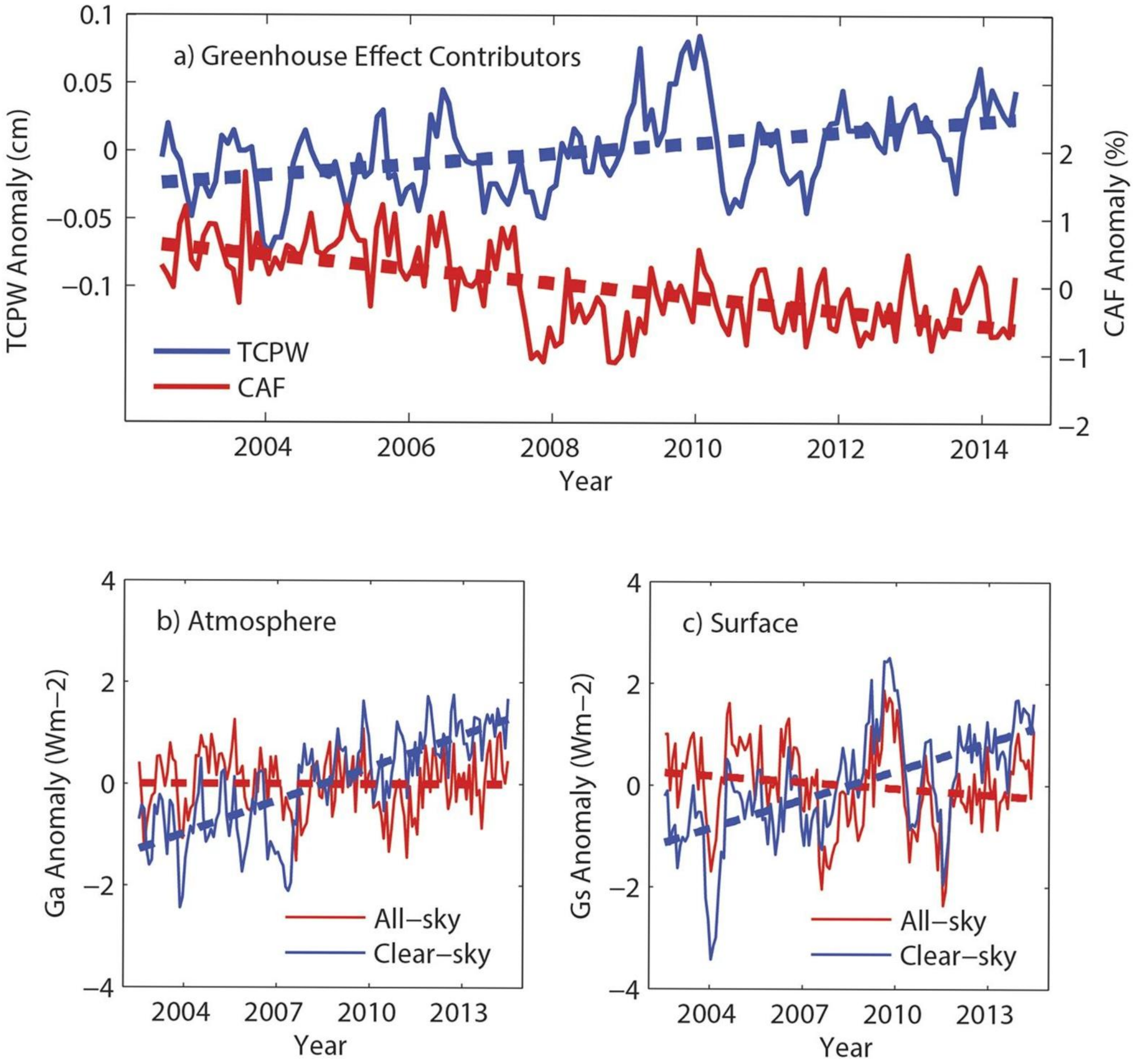

Figure R2 [CPW: total column precipitable water; CAF: cloud area fraction**;** original caption in paper follows]
Figure 4 (a) Monthly variations of the global averaged TCPW (unit: cm) and CAF (unit: %) anomalies between 2003 and 2014. Dashed lines are the linear trend lines obtained by the least squares method. (b) Monthly variations of the atmospheric greenhouse effect parameter anomaly (G_{aa}; unit: W m^{-2}) from 2003 to 2014 for all-sky (red lines) and clear-sky (blue lines) conditions. Dashed lines are the linear trend lines obtained by the least squares method. (c) Same as (b) but for the surface greenhouse effect parameter anomaly (G_{sa}; unit: W m^{-2}). The figure was plotted using MATLAB software.

R1.9. Even at the surface, while K+V (2024) argue that the direct impact of CO2 is not detectable in noisy datasets, they don't consider the indirect impact of CO2 on the water vapor itself (the WV feedback), which is expected to have 2 to 3 times the impact on LW than CO2 alone.

The reviewer seems to assume that water vapor is only a feedback to CO_2. Koutsoyiannis and Vournas [4] did not make such an assumption, of course. They analyzed data. Data do not comply with what "is expected" to happen according to the conventional wisdom and to Reviewer #1. This means that that expectation is wrong. Not that Nature is wrong.

R1.10. The inference made in KV24 and here (paragraph 4) that because a direct effect of CO2 on surface fluxes is not detectable once WV changes have been factored in means that 'the effect of CO2 is negligible' is fallacious reasoning.

I am unable to discern what specific reasoning employed in Koutsoyiannis and Vournas [4] could be characterized as fallacious. The present paper confirms this result using models and, additionally, expands it to the outgoing radiation.

R1.11. Research questions: The first part of this question is a valid thing to study. With respect to the second question, It's very unclear to me how any greenhouse gas is supposed to know whether LW radiation is upwelling or downwelling before affecting it. The 2nd question is therefore nonsensical. Maybe a rewrite to state that the aim is attribute the role of H2O in both outgoing LW at the TOA and downwelling LW at the surface would be clearer. It's not obvious that the attribution would be the identical, but the notion that one could be zero and not the other is silly.

As I already noted (reply to comment R1.2), I view the expressions "nonsensical" and "nonsense" inappropriate in scientific publishing and in peer review exchanges—particularly anonymous ones. During my long (twelve years) experience as Editor of the oldest hydrological journal (*Hydrological Sciences Journal*), I have been interested in positive and negative ethics of editorial practice. I tended to ignore those reviews that were offensive. Though I may have failed once or twice, I would hope that the Editor of *Hydroecology and Engineering* shares similar values.

R1.12. p2 and also p26. All attribution exercises involve counterfactuals, and thus must be model based. This is neither profound nor problematic.

I assume the reviewer refers to this phrase:

> Also, the second question cannot be studied on a purely empirical basis, as no long-term data exist (systematic satellite measurements of outgoing LW flux have only been made in the 21st century). Therefore, to study these questions we need to resort to theoretical arguments and analyses.

I personally have no doubt that investigating research questions based on observations is preferable. Models can be and usually are wrong, while Nature is always right. In several decades from now, when datasets of sufficient size will be available, the same research questions should be reexamined using data.

Resorting to models is not necessarily the only option. That is why I also enrolled data in my analyses, to check the reliability of the models I used. Here I provide a relevant quotation from Koutsoyiannis et al. [5], which found a way to fully replace climate models (and actually dispute them) based on data:

> In complex systems such as Earth's climatic system, experimentation is impossible. Yet it is a widespread belief that the climate models are faithful representations of the climatic system and hence offer the possibility of the so-called *in silico* experimentation (Hannart et al. [12]). Furthermore, it has been claimed (Hannart and Naveau [13]) that "in silico *experimentation* [is] *the only option*" and that "*the increasing realism of climate system models renders such an* in silico *approach plausible*". Such claims are epistemologically problematic. A hypothetical "causality" that is incorporated in any model, particularly of a complex system, is not

necessarily identical to the natural causality. In addition, the agreement of climate model outputs with reality has been questioned (e.g. [14-18]).

Our methodology can help with this epistemological problem in two ways. First, it provides a different option to test causality, showing that the so called *in silico* experimentation is not the only option as claimed. Second, it can additionally test whether there is indeed realism in representation of causality of the climatic system by the climate models. As already stated in the Introduction, our methodology, regardless of the detection of causality per se, can define a type of data analysis, which could shed light on modelling performance by comparing observational data with model results. This is particularly useful in the case of climate modelling. In other words, it could help in verifying or falsifying the commonly accepted theory, which is incorporated in the climate models.

R1.13. Bottom of p2. These references [19,20] are to work that, if true, would undermine every calculation in this paper. Fortunately for the author they are not correct and unless one was to delve into their (obvious) flaws, I'd recommend ignoring them completely.

The exact statement is:

We will do this by applying the established greenhouse theory and by enrolling standard models, without considering doubts that have been cast on the validity of the theory or alternative hypotheses (e.g. [19,20]).

I think it is clear from this statement that I do not use these alternative hypotheses. However, for the completeness of the presentation, I think it appropriate not to hide that alternative hypotheses have been proposed.

R1.14. Human CO2 emissions may only be 4% of total emissions, but they are effectively 100% of the long-term imbalance in CO2 fluxes.

I am aware of this very common opinion, but I am confident that it is wrong. Nature does not distinguish between the CO_2 molecules emitted by humans and those emitted by other processes.

I have studied the atmospheric CO_2 balance/imbalance in another paper, which I am also struggling to publish: "*Reservoir routing and its application to atmospheric carbon dioxide balance*" [21]. As regards the present paper, this issue is outside of its scope and I invite the reviewer to see my other paper.

R1.15. p2 last paragrpah. This is a significant overestimation. Anthropogenic sources of WV (directly via combustion, or indirectly via irrigation and increased evaporation) are tiny compared to the ~3mm/day global flux of evaporation. There is no validity in pretending that the affected terrestrial evaporative flux is important when this is a very small fraction of the total evaporation. First there is a typo in the text, the FAO estimates 4,300 km3/yr in withdrawals, not 4.3 km3/yr. But even using the correct number, 4300 km3/year is less than 1% of the global evaporative flux. And given the short residence time for atmospheric water vapor this can make only a very small proportionate impact on water vapor concentrations on land or anywhere.

Yes, there were a couple of typing errors which have been corrected. The exact text is this:

Human CO_2 emissions are 4% of the total [8] but there are also human H_2O emissions over the terrestrial part of Earth of a comparable percentage [22,23,24]. Specifically, according to Koutsoyiannis [25] the quantity of evaporation and transpiration over land is 91 400 km^3/year. According to Food and Agriculture Organization of the United Nations [26], the human water withdrawal in 2010, including the evaporation from reservoirs, was 4 300 km^3/year, of which 69% and 19% were for agricultural and industrial use, respectively. Considering the fact that almost all of agricultural and a large part of industrial water are evaporated, as well as the increasing trend in water withdrawal, with simple calculations we may conclude that the current human addition to the natural water cycle over land is about 4%. One could speculate that each of these 4% additions might influence the climate to a degree comparable to that percentage, but the reasons that only the influence of CO_2 is investigated and highlighted by the scientific community, being regarded as a control knob of climate (even though H_2O is much stronger as a greenhouse gas) are not scientific.

We may note, though, that the percentage of human H_2O emissions becomes much lower, of the order of 1%, if we also consider the evaporation over oceans, where there is no human intervention. However, it appears reasonable for this estimate of human water emissions to consider only the terrestrial part of Earth, because of the high local variability and the small residence time of atmospheric water. Indeed, the mean residence time of atmospheric water is a few days (specifically, 12 250 km^3 / (522 700 km^3/year) = 0.023 years = 8.6 d, where the total volume of liquid water in the atmosphere, 12 250 km^3, was taken as the average atmospheric water content, 24.0 kg/m^3, as estimated from the ERA5 Reanalysis over the globe for the period 1950-2023, while the atmospheric inflow volume of water, 522 700 km^3/year, was taken from [25]). In contrast, CO_2 is well mixed as it has a much higher residence time, of a few years (specifically, 870 Gt / (226.9 Gt/year) = 3.8 years, where the total mass of atmospheric CO_2, 870 Gt was taken from the Intergovernmental Panel on Climate Change (IPCC) report [27], namely its figure 5.12, while the atmospheric inflow mass, 226.9 Gt/year, was taken from [8]). Detailed analysis of the CO_2 residence time (which differs substantially from the official IPCC's one) can be found in [21].

In other words, the percentage of 1%, which is mentioned by the reviewer, is clearly included in my text. This corresponds to the global flux, while 4% refers to the terrestrial flux. The text also provides a justification why 4% may be more representative than 1% when we deal with water vapor. For this reason, no changes are needed.

R1.16. p3, paragraph 4. This is a bizarre argument. There are literally thousands of papers a year written about the role of water in climate change - from cloud processes, the cryosphere, in the stratosphere and in the ocean and everything in between. And who has 'downgraded' the importance of the carbon cycle? Similarly, there are thousands of papers discussing this.

Yes, thousands of papers discuss “the role of water in climate change”, as if this role is only to be the “feedback” that enhances the importance of CO_2, or to materialize “climate impacts” giving rise to scary scenarios that have to involve water to become scarier. This disrupts and downgrades the role of water to an element driven by CO_2. Water is the main player, not a feedback. Therefore, no change is needed to the paragraph. (See also my reply to Comment R1.28.)

R1.17. Section 2.1. This is standard atmospheric sciecnce textbook stuff. Not sure why it's here. Just reference a standard text, Curry and Webster (1998) for instance.

Curry and Webster (1998) do not contain the equations presented in Section 2.1 of the original version or Appendix A or the revised version. In particular, they do not include Equations (A6) (produced by myself in my paper [28]) and (A7) that were used in the calculations of the paper and are more accurate than commonly used equations of the literature. Equation (A7) is not contained anywhere else, because it is new, produced in my present paper.

Anyhow, Section 2 is now reduced and most of it is moved to an Appendix; see also my reply to Comment R2.4.

R1.18. p5 Why is eq. (11) here? None of it is used.

Yes, the former Equation (11)—now moved to Appendix A as Equation (A10)—is used in the calculations because this is the one that defines the Stefan–Boltzmann constant σ, as used in the paper. Perhaps the reviewer is not aware of the fact that, before the establishment of that equation, different researchers used different values of σ. Koutsoyiannis and Vournas [4] have given the details and also converted old data sets for the correct value of σ.

R1.19. p7 para2. There is no evidence of 'public perception' in any part of this paper (and I'm not sure of it's relevance in any case), but given that CO2 is well-mixed and WV is not, it is hardly surprising that WV variations dominate the spatial and temporal variations in downward LW. I am unclear as to why the author seems to think that Lacis et al, or anyone else thinks otherwise.

Yes, there is evidence, given in the Introduction, which, unfortunately, I had to change in the revised version. In particular, the answers by the bot, which the reviewer found irrelevant, exactly reflect that public perception.

R1.20. p7. para3. If the preceding exposition related to Penman-Monteith 'does not help' (and I agree that it doesn't). Why is it in the paper?

The exact text was:

> Yet it is useful to quantify the contribution of CO_2 to the greenhouse effect and compare it to that of water vapor. In this, the above formulae do not help, and we need to enroll detailed modeling of the spectroscopic properties of the atmosphere.

In brief, these formulae do not help to quantify the contribution of CO_2 to the greenhouse effect. But they do help to quantify the contribution of H_2O to the greenhouse effect. We should not forget the fact that H_2O is a greenhouse gas That is why they are included.

The paragraph has been rephrased now as follows:

> However, for reasons explained in the Introduction, we seek to quantify the contribution of CO_2 to the greenhouse effect and compare it to that of water vapor. In this, we need to enroll detailed modeling of the spectroscopic properties of the atmosphere. Also, while the formulae of Appendix A are useful for the downwelling radiation, they do not quantify the outgoing radiation at the top of the atmosphere (TOA). For those tasks, we may use detailed spectroscopic models.

R1.21. p9. para4. Only the absolute values have the offset, however the temporal changes in the CERES fluxes are robust (and match the interannual variations in the ocean heat content (Loeb et al, 2021).

I do not understand what the problem of the reviewer is here. I use as reference the official CERES document (Updated 8/4/2023). Furthermore, I analyzed in detail the temporal changes in the CERES fluxes in Section 5.3.

R1.22. p17, Section 4.2 onwards: As the author is aware, the clouds in MODTRAN are not representative of real clouds, nor their distribution across optical depth and height. However, the clouds seen in CERES don't provide any height information, and the idea that a realistic model of the impacts of clouds on LW radiation can be formulated based on a linear function of CERES-derived cloud fraction is hopelessly naive. Indeed, it is obvious from Fig 13 that any such attempt is going to fail. [There is a more subtle point about what is meant by cloud fraction since this is - in practice - dependent on the limits of the observing platform. An argument can be made that in reality cloud cover is always 100%, but with a large range of optical depths that can go below what either our optical sensors (eyes!) or the satellite instruments can detect]. The main issue is that clouds are different heights with different temperatures and different background specific humidity and so the same cloud fraction at 1km will have a very different impact on the LW than that same fraction at 10km. This will also be a big effect on the overlaps between clouds and water vapor and clouds and CO2 that come up later in the paper.

Clouds are very important and, as the reviewer points out, MODTRAN is not representative of real clouds. That is why I used CERES data instead. Certainly, Figure 13 would not satisfy a fan of determinism, but in a macroscopic study using a stochastic framework, such as mine, it provides very useful information. The empirical relationships routinely used in engineering practice to estimate evapotranspiration are no better in terms of data scattering than Figure 13.

As mentioned in the Acknowledgments, I have shared the paper with William Happer, who I believe is the world's top expert on combining atomic physics, optics and spectroscopy. He gave me encouraging and useful feedback. His opinion on the clouds part of the paper was this: "*I think that an important part of your message is that clouds substantially decrease the effectiveness of CO_2 and other greenhouse gases.* […] *We already know enough to be sure that clouds will diminish the importance of CO_2 and other greenhouse gases. So we agree with you.*"

Anyhow, my paper provides a first approximation on the issue, and I hope others (including William Happer) will provide better approaches in the future.

R1.23. p26. para1. The decrease in total outgoing radiation in the CERES period has been the subject of multiple papers (Loeb et al, 2021; Raghuraman et al, 2022 ; Hodnebrog et al, 2024) and it is clear that there are multiple causes, including GHG-driven cloud and water vapor feedbacks, surface albedo changes and aerosol effects (direct and indirect via clouds). The author is not presenting an attribution study of this, and so the conclusion here is unsupported (and is in fact wrong).

Right, I am "not presenting an attribution study of this" for the simple reason that it is out of my paper's scope. As seen in its title, the paper's scope is to assess the relative importance of carbon dioxide and water in the greenhouse effect. It is not to examine the causes of changes in radiation in the CERES period.

R1.24. p26. para 1 (and elsewhere). This is hopelessly naive, and ignores the role of water and cloud feedbacks on CO2-driven warming. It is also unsupported by any analysis.

The paragraph in question is:

> The above results from CERES data, which are for the torrid zone only, are similar to those for global averages, presented by Koutsoyiannis and Vournas [4 Appendix B]. The latter study also examined SW radiation data and found a decrease of total outgoing radiation, which is consistent with the increased atmospheric temperature. This decrease of outgoing radiation can hardly be attributed to increased [CO_2] but it can be related to water vapor and cloud profiles. The effect of CO_2 is trumped by the effect of clouds, which is consistent with the major role of water on climate and the minor one of CO_2.

As seen above, the "hopelessly naïve" paragraph is supported by analysis in Koutsoyiannis and Vournas [4] and in the present paper. Further, as seen in my reply to Comment R1.22, it is consistent with what Happer and colleagues know about the effect of clouds, which I hope they will publish.

R1.25. p26. I agree with the author with respect to the clear sky results, the macroscopic formulas seem to work well over their calibrated range. However, I don't know that they will extend well to the broader range required by the attribution exercise attempted in Section 6. This requires reasonable accuracy down to zero water vapor and zero CO2, and it is clear that logarithmic response to CO2 will not extend that far. However, while this would be important in a reall attribution attempt, it is not used later in theis paper because the attribution is fundamentally flawed and is not being calculated correctly (see below).

I do not agree that the macroscopic formulae should include cases "down to zero water vapor and zero CO_2", because the purpose of the paper is to assess the relative importance of greenhouse gases in realistic conditions.

Nonetheless, the paper also provides such information on imaginary conditions, such as those mentioned by the reviewer. As stated in the beginning paragraph of Section *6.1, Imaginary-world Conditions*:

> Investigating imaginary-world conditions seems pointless, yet we include it for the reasons explained in the Introduction—the fact that several popular narratives are based on imaginary-world conditions. The imaginary-world conditions we examine are the most extreme ones, starting from the case that [CO_2] is totally absent in the atmosphere and ending to the case where the atmosphere is composed of merely CO_2. Extreme ranges are also examined for other greenhouse gases, including water vapor.

For the imaginary conditions, I do not use the generalized relationships, but the MODTRAN model per se.

R1.26. para 3. The clouds-related part is much less convincing.

Given Reviewer #1's general antipathy to the clouds-related part of my paper, it is unsurprising that she/he finds it "less convincing".

R1.27. para 4. This is a bizarre statement. All attribution work must be model based and revolves around counter-factuals, and this might only 'seem pointless' to someone who has never thought about it.

I assume the reviewer is referring to my use of the word "pointless" in the paragraph quoted above (in reply to her/his Comment R1.25). I insist it is pointless to examine the imaginary conditions. It is meaningful only to examine the realistic conditions. However, I did include the pointless part for the reasons I explained, i.e., because "*several popular narratives are based on imaginary-world conditions*".

She/he is wrong to claim that I am "*someone who has never thought about it*" and the proof would be found if she/he were to read my published papers on the subject and also this statement in the Acknowledgements of the present paper:

> I thank an anonymous reviewer of a predecessor paper [4], whose critical comments made it necessary to delve into the topic examined in the present paper. The brief reply to those review comments was not included in the predecessor paper in order not to distract its focus. Yet it constituted the springboard to produce this paper.

R1.28. para 5. This is nonsense and based on fallacious reasoning. Sure, if you could increase WV by 30% at the same time you decrease CO2 to zero, temperatures on average would remain the same, but that would greatly increase relative humidity levels and there is no proposed mechanism for how that could be sustained on a global level. The author seems to posit that the water vapor feedback is negative - which contradicts all credible studies on the topic - and which I think would be a surprise to many!

I invite the reviewer to see the title of the Section, above the paragraph she/he discusses. The title is "*6.1 Imaginary-world Conditions*". My point is that it is essential to distinguish between imaginary and real worlds. If these are confused, discussions about the validity of scientific papers decrease rather than clarify understanding. The reviewer here has criticized my discussion about an imaginary situation but she/he has criticized it on the grounds that it could not be—as she/he states (amongst many other hypothetical situations)—"*sustained on a global level*" all the while seeming to forget that in this section I am discussing *Imaginary-world Conditions*. She/he also forgets that the main part of the paper is about real-world conditions.

In brief, I am the wrong recipient of her/his accusations of "*nonsense*". He could address them, as well as her/his statement "*if you could increase WV by xxx% at the same time you decrease CO_2 to yyy*" to other recipients who do not distinguish what is real and what is imaginary.

I do not posit anything about "water vapor feedback" because I think that water vapor is not a feedback. I repeatedly state that in the paper, starting from the Introduction, whose penultimate paragraph is this:

> While climate has become a hot topic and its research a top priority, it is odd that hydrology has lost importance, as evident from the abundance of papers examining climate change impacts on water and applying model projections for the future based on CO_2 emission scenarios. This misses the fact that water is the key element on Earth in driving climate and that the hydrological cycle is self-ruling rather than a feedback or impact of another cycle—

> namely the carbon cycle, which has also been downgraded to an issue governed by human carbon emissions (the 4% of the total).

The reviewer does not clarify which the "credible studies on the topic" are and in which respect they contradict my studies. Therefore, to aid clarification, I list below four studies giving their titles and quoting relevant statements from them which show that increasing the near-surface water vapor (by irrigation) leads to cooling:

- *Substantial decline in atmospheric aridity due to irrigation in India* [29].

  > We found that LST [land surface temperature] has considerably (0.4–1.7 °C) declined during the growing season over the Indo-Gangetic Plain and in other parts of India […] This significant cooling of 0.8 °C […] is associated with the irrigation expansion.

- *Irrigation cooling effect on land surface temperature across China based on satellite observations* [30].

  > In the arid climate zone, nearly all the irrigated areas show a lower daytime LST than the adjacent non-irrigated areas, leading to a strong ICE [irrigation cooling effect] magnitude of >6 K in the growing season.

- *Characterizing spatial, diurnal, and seasonal patterns of agricultural irrigation expansion-induced cooling in Northwest China from 2000 to 2020* [31].

  > Specifically, more intensive cooling occurred in new irrigated areas from unused lands (-0.69 ± 0.02 K) than that from grasslands (-0.47 ± 0.05 K) and forests (-0.28 ± 0.04 K). The cooling effects were dominated by marked daytime cooling compared to negligible nighttime warming.

- *The global warming potential of near-surface emitted water vapour* [23].

  > Water vapour is the most abundant and powerful greenhouse gas in Earth's atmosphere, and is emitted by human activities. Yet the global warming potential (GWP) and radiative forcing (RF) of emitted water vapour have not been formally quantified in the literature. […] Water is introduced in vapour form at rates matching total anthropogenic emissions (mainly from irrigation) […] Increases in water vapour greenhouse effect are small because additional vapour cannot reach the upper troposphere, and greenhouse-gas warming is outweighed by increases in reflectance from humidity-induced low cloud cover, leading to a near-zero or small cooling effect. Near-surface temperature decreases over land are implied even without evaporative cooling at the surface, due to cooling by low clouds and vapour-induced changes to the moist lapse rate. These results indicate that even large increases in anthropogenic water vapour emissions would have negligible warming effects on climate, but that possible negative RF may deserve more attention.

All the above suggests that the relationship between water vapor and climate is complex, but its full modeling is out of the scope of the paper. The present paper acknowledges this complexity, criticizes the naivety of considering water as a feedback and expresses my thesis that much more research is needed to remove the misconception that water is a feedback. See the concluding paragraph in the paper:

Given these recent developments, the case of the magnified importance of CO_2, and particularly the human emissions thereof, appears to be a historical accident in scientific terms, that was exploited in non-scientific terms. If we return to science, the proper path is to improve hydrology and stochastics in order to better understand and model climate. For climate is mostly hydrology in terms of its driving physical mechanisms (as articulated here) and mostly stochastics in terms of its proper mathematical representation (as implied by its very definition; cf. Koutsoyiannis [59,32]).

R1.29. Table 5. This is a nice summary, but the case is a little odd. These are results for a tropical profile but with a global mean surface temperature (which is 11.7 deg C cooler), and with fixed water vapor pressure, so will have a much greater amount of water vapor than the global average would have. I would suggest using a consistent profile (i.e. tropical temperatures for a tropical profile, and, midlatitude temperatures with mid-latitude profiles etc.). The impact it makes (I think) is substantial in the LW fluxes. However, the best metric to use here is G=LW_up_surf - LW_up_TOA (which should be zero in the absence of any greenhouse substances), and that is little less sensitive to the absolute temperatures. If the clear-sky attribution between CO2 and water vapor is done in this case, it's 27% for CO2 (28% with tropical temperature), but 31% for the mid-latitude summer case. (Percentage calculated assuming that the overlaps are split 50:50, following Lunt et al, 2021; doi:10.5194/gmd-14-4307-2021). This is very similar to the results seen in Lacis et al, 2010; Schmidt et al, 2010, where the clear-sky attribution to CO2 in the global mean was 27% (ignoring other greenhouse substances). One minor note of caution is the fact that G in MODTRAN is not zero in the case with no greenhouse substances at all (instead it's around 1.3 W/m2) which I think is likely to be due to an issue with the MODTRAN approximations in that extreme situation.

Following the reviewer's suggestion, I added in Table 5 the values that are derived for the standard "*tropical temperatures for a tropical profile*". The modified (expanded) Table 5 is included below, as well as in the revised manuscript.

As seen in the modified Table 5 and as expected, the quantitative results do change, when temperature is increased by 11.7 K. However, we are not interested in absolute values but in comparative ones. These are the same as in the original Table. For example, the LW radiation fluxes for CO_2 concentration scale equal to 1 and zero concentration of all other greenhouse gases, is matched by zero CO_2 concentration with water vapor scale as low as 0.0015 to 0.04! (observe the values highlighted in green).

Table 1 Results of MODTRAN calculations for tropical profile and temperature at zero altitude of either 288 K (the value of current global temperature used by Brutsaert [33]) or 299.7 K (the standard value of temperature at zero altitude of the tropical profile) and for extreme (imaginary-world) cases of greenhouse gas concentrations.

$[CO_2]$ relative to the default value of 400 ppm	Water vapor scale relative to the default tropical profile	Other greenhouse gases concentration relative to default	Downward IR heat flux at surface (W/m^2)	Upward IR heat flux at surface (W/m^2)	Outgoing LW radiation flux at 100 km altitude (W/m^2)
1	1	1	325.6 (369.3)*	381.5 (446.5)	249.5 (298.5)
1	1	0	324.0 (366.8)	381.5 (446.5)	256.7 (307.1)
0	0	0	1.7 (2.2)	380.3 (445.3)	379.0 (443.7)
1	0	0	68.2 (80.9)	380.3 (445.3)	340.7 (400.4)
2500†	0	0	215.7 (259.0)	381.5 (446.8)	257.1 (302.2)
0	0.0015	0	78.8 (90.0)	380.3 (445.3)	366.4 (449.6)
0	0.04	0	157.8 (183.3)	380.6 (445.6)	340.7 (399.7)
0	0.2 (0.27)*	0	212.5 (259.7)	380.9 (445.9)	319.3 (370.2)
0	1	0	319.7 (358.9)	381.5 (446.5)	284.7 (339.1)
0	2.2 (2.6)	0	372.1 (438.3)	381.8 (447.1)	257.7 (302.6)

* The values without and with parentheses correspond to temperature at zero altitude of 288 K and 299.7 K, respectively. When there is no parenthesis in the column of water vapor scale, the same scale is assumed for both cases.

† In this case the atmosphere is composed merely by CO_2 (2500 × 400 = 1 000 000 ppm = 1).

R1.30. Figure 23. This is only for the full tropical profile which (as stated above) has far more water vapor than the global average. The change assuming constant relative humidity (as opposed to specific humidity) would be nolticeably larger.

Of course, it would be possible to add more figures similar to Figure 23, making other assumptions. But I believe this particular Figure and the analyses made to construct it are sufficient for the paper, which already contains 28 figures (including two in the Appendix). The reviewer may feel free to make her/his own analyses with alternative hypotheses, produce additional figures, and publish them in her/his own paper.

R1.31. p29. para2. The 'Freon' scale in MODTRAN is for all CFCs, not just Freon.

Since nothing would change by adding the reviewer's note, I preferred not to mention it, particularly as I have found no relevant information concerning it.

R1.32. p30 onwards. The calculations leading to equation 27 and table 6 seem fine, but the author seems to be a little confused as to what they mean. He has calculated the gradient - with present conditions (400 ppm, etc.) of any fractional perturbation in water vapor or CO2 or cloud, which is interesting enough, and akin to a standard 'radiative forcing' calculation (Etminan et al, 2016), but it is not the same as the contribution of each of those substances to the greenhouse effect as a whole (which is what he claims to be comparing to with Schmidt et al, 2010). Indeed, as noted above the clear sky results inferred from Table 5 are totally compatible with the Schmidt et al attribution.

Additionally, Equation 21 is not defined at CO2=0, and so cannot be used to infer the total contribution in any case (and also, it is calibrated to the tropical water vapor profile, not the global mean).

I respectfully disagree 100%. The calculations should be made for the present conditions and those that appeared in the past and or are plausible for the future (cf. Table 7, which provides values for [CO_2] = 800 ppm).

Imaginary world conditions are for the imaginary world of those who would live in their imagination. Even so, I have included Table 5 to prove that the entire narrative of even their imaginary world conditions is false.

My study is clearly not compatible with the Schmidt et al attribution—and should not be. It is radically different.

R1.33. p30 para3. This is an apples-to-oranges comparison and is simply not valid. Additionally, the 1:8 ratio for clear sky explicitly contradicts the attribution exercise in Table 5.

The exact paragraph is this:

> These results differ substantially from those of Lacis et al. [34] and Schmidt et al. [35], who, using a different methodology, attributed 75% to water vapor and clouds and 19% to CO_2. Our results are closer to an example given by Brooks [36], in which the contribution of the CO_2 bands is about 1:8 compared to water vapor, without considering the clouds.

I think the paragraph is perfectly correct. Even Reviewer #3 (who equally opposes my article; Comment R3.9) includes the same statements "*clouds and water vapor make up 75% in total*". I cannot understand what Reviewer #1 regards as apples and what as oranges.

The ratio 1:8 compares relatively well to the one I give, based on the values in Table 6. The related text in the paper (Section 6.2) is:

> In other words, each of the related factors, water vapor and clouds, is an order of magnitude more important than [CO_2] in terms of the greenhouse effect.

Also, with reference to the values in Table 6:

> It can be readily found using the values in Table 6, that the relative importance of water vapor over [CO_2] is 0.207/0.015 = 13.8 times for the downwelling flux, and $(-0.136)/(-0.015) = 9.1$ times for the outgoing flux.

Table 5 is irrelevant to this discussion as it refers to imaginary-world conditions.

R1.34. Figure 24. This figure is grossly misleading. The values shown are the radiative forcing ratios at CO2=400ppm for the tropical profile, not the 'contribution of greenhouse drivers to LW fluxes'.

Figure 24 is absolutely correct as it refers to the real world in contrast to other studies referring to imaginary-world conditions. It is for our Earth—not for comparisons with another planet with an atmosphere devoid of CO_2. Note that CO_2 was never absent in Earth's atmosphere; rather historically it used to be much more abundant [10].

I believe Figure 24 is the most representative and most useful figure of my study.

R1.35. Table 7. The values given in Table 7 are just estimates of the historical radiative forcing - but they are not very accurate becuase of the poor representation of clouds and the use of a tropical profile. Note that the IPCC (following Etminan et al 2016) has the global mean forcing from CO2 from 300 to 420ppm as roughly 1.8 W/m2 (at the top-of-atmosphere) which is not that far from the estimates here and compatible once the tropical water vapor profile is taken into account. Note too that van Wijngaarden and Happer are also compatible with the standard estimate of the radiative forcing for doubled CO2 (3.4 W/m2 +/- 0.6 W/m2 (95% CI)).

The values in Table 7 are as accurate as the available data allow. The comparison with van Wijngaarden and Happer is included in the paragraph immediately following Table 7:

> Our results in Table 7 are comparable to those of van Wijngaarden and Happer [37] (corroborated in de Lange et al. [38]), who using a detailed representation for high-resolution transmission molecular absorption database (HITRAN, a compilation of spectroscopic parameters that a variety of computer codes use to predict and simulate the transmission and emission of light in the atmosphere) and satellite data, concluded that a doubling of CO_2 concentration (from 400 to 800 ppm) would result in a 3 W/m^2 decrease of radiation flux in the top of the atmosphere, which translates to –1.1%.

As I already mentioned (in my reply to Comment R1.22), Happer himself has perused the paper and agreed with my estimates related to clouds.

R1.36. p32 para2. The author should probably be aware that no-one is proposing the actual removal of all CO2 from the atmosphere, and thus we don't need to be too concerned with the impacts that this hypothetical might have on the carbon cycle.

The paragraph in question reads:

> Furthermore, even the 10% of the current atmospheric value of water vapor for $T_\mathrm{S} = T_\mathrm{E}$, given in the quoted statement by Lacis et al., would produce a greenhouse effect and hence would imply the inequality $T_\mathrm{S} \neq T_\mathrm{E}$, thus leading to absurd. That greenhouse effect would not be 10% or close to it, but closer to its current magnitude. Indeed, according to Brutsaert's equation (13), for $T_\mathrm{E}/T_\mathrm{S}$ = 255/288 (with 288 K being the current average temperature used by Brutsaert) and vapor pressure ratio $e_\mathrm{E}/e_\mathrm{S}$ = 0.1, the resulting emissivity ratio $\varepsilon_\mathrm{E}/\varepsilon_\mathrm{S}$ would be $(0.1 / (255/288))^{1/7}$ = 0.73. An emissivity $e_\mathrm{E} > 0$ means that we would again have the greenhouse effect produced by water vapor. (See also Table 5 and its discussion in Section 6.1.) And even in an "*icebound Earth state*", thermodynamics implies the presence of water vapor in the atmosphere, due to sublimation. Remarkably, though, geological evidence presented by Veizer [39-41] suggests the presence of running water as far back as we have a record, up to 3.8 or even 4.2 billion years, despite the much smaller solar irradiance (the so-called faint young sun puzzle). All these imply that the argument is mistaken and so is the popular result that is being widely reproduced.

This reviewer's comment is indeed most interesting. In her/his previous comments the reviewer consistently referred to the imaginary-world conditions (such as absence of CO_2 in the atmosphere) as if these were appropriate to base any conclusions. Now, when the calculations of this paragraph show that even the imaginary-world conditions cannot produce the results promoted as the climate truth, the reviewer changes course and states *"no-one is proposing the actual removal of all CO_2 from the atmosphere*".

There is nothing to change in the paragraph, which is correct.

R1.37. p32 para 3. I don't understand this argument - possibly some editing is needed?

The paragraph has been rephrased as follows:

> The distinction between feedbacks and forcings, also appearing in the quoted statements by Lacis et al., is problematic. Both H_2O and CO_2 have always been present on Earth and both are greenhouse gases, with the difference being that the former is much more abundant in the atmosphere and determinant for the greenhouse effect, as already demonstrated. Calling CO_2 forcing and H_2O feedback is like claiming that the tail wags the dog.

R1.38. p32. para4. The author misunderstands what the forcing/feedback distinction refers to. The anthropogenic increase in CO2 emissions (and hence concentrations) is an external perturbation to the system, to which many aspects of the climate react, including water vapor and clouds. Were the anthropogenic perturbation to water vapor larger, then it would be be treated similarly. But as it is, humans have directly caused CO2 concentrations to increase by 50%, but have only directly caused a <1% increase in water vapor (as shown above).

As I already replied to Comment R1.4, the reviewer's statement referring to the contribution of human activities is an estimate or opinion and not a fact. This opinion has been disputed by several publications, including those referred to in my reply to Comment R1.4. Whether or not the reviewer agrees with these publications does not matter in this discussion. I think she/he should not present her/his opinion, however popular this may be, as a fact.

R1.39. p33. para1. The total of human energy production is irrelevant in this context.

I do not understand why the reviewer has a problem with a one-line statement which is true and accurately quantified. Engineers, who I guess are included in the audience of a journal titled *Hydroecology and Engineering*, should be interested to know that the so much cursed human energy production, which I call humanity's locomotive, is just 1/2100th of the natural locomotive, which also involves the emission of greenhouse gases—both H_2O and CO_2, with the former being the determinant.

R1.40. p33 para2/p34 para 1. It's not clear who the author is arguing with. Does anyone think that water is not important for the climate system?

Yes… Reviewer #1 (and she/he is not alone) thinks that water is just a feedback of the CO_2.

R1.41. p34 para 2 last line. This is incorrect. What was demonstrated was that the interannual variations in the CO2 rate of growth are affected by temperature variations (dominated by ENSO). The papers cited did not provide an alternative explanation for the 50% rise in CO2 concentration since the mid-19th Century.

The statement in question (slightly modified in the revised manuscript) is this:

> And as Koutsoyiannis and Kundzewicz [5] and Koutsoyiannis et al. [8] have shown, it is the relationship between temperature and biosphere that has determined the recent increase in the atmospheric [CO_2].

It is 100% accurate. The papers cited do provide an alternative explanation for the 50% rise in CO_2 concentration since the mid-19th Century. See Section 6 in the former and Section 9 and Appendix A.1 in the latter.

R1.42. p34 para3. These 'facts' are not supported by either data nor theory, and so it is not at all stunning that they don't impact the broader climate debate.

The statement in question is:

> Considering all these facts, it is stunning that the whole "climate project", including climate modeling, is based on hypotheses and scenarios about human CO_2 emissions.

The facts are supported by both data and theory and the statement is correct.

R1.43. p34. para4. This is not a valid conclusion from the work presented. Indeed, this is complete nonsense. Even if the calculations in Fig 24 were correct, they are related to CO2 concentrations, not emissions. Thus the percentage of emissions due to humans is not the relevant metric - rather it should be the percentage of CO2 concentrations that are attributable to human activity (which is over one third and growing). But even that isn't correct because the impact of increasing CO2 also leads to increases in water vapor and so the ratio of the greenhouse contribution will scale with the human contribution. This was shown in reference #66.

Of course, the calculations should, and do, refer to concentrations and not emissions. Both Figure 24 and the conclusion discussed are correct. By resorting to an insulting characterization of "complete nonsense", the reviewer demonstrates her/his inability to use logic.

In any case, I have nothing more to discuss in this inappropriate comment, except to ask the Editor to think about it.

R1.44. p34. para 5. This is all nonsense as well.

I would request the Editor to think about why the reviewer has used the terms "*complete nonsense*" and then again "*all nonsense*" in the last couple of comments and I will not comment further except to say that this closing remark reflects precisely the main characteristic of this review: offensive and insulting.

4.3 Reviewer 2

R2.1.

Reviewer review report

Overall Recommendation	( ) Accept in present form (✓) Accept after minor revision(I do not need to see the revised version) ( ) Reconsider after major revision(I want to see the revised version) ( ) Reject
English Language and Style	( ) Extensive editing of English language and style required ( ) Minor spell check required (✓) English language and style is fine ( ) I don't feel qualified to judge about the English Language and Style
Recommendations for Authors	★★★★★ Is the manuscript novelty enough? ★★★★★ Is the manuscript scientifically sound and not misleading? ★★★☆☆ Does the manuscript give a concise and comprehensive view of the topic? ★☆☆☆☆ Is the manuscript well organized and of proper length? ★★★★☆ Is an adequate number of references investigated and appropriately quoted?

I am thankful to Reviewer #2 for the positive assessment of the paper, giving it five stars for novelty and five stars for scientific soundness, as well as for the recommendation to accept it for publication after minor revision. Additionally, I am grateful for the constructive suggestions, which I addressed, as detailed below.

R2.2. The key issue raised is whether CO2 (and other noncondensing greenhouse gases) dominates over H20 in importance, and whether the terrestrial greenhouse effect would collapse without CO2 – Lacis' so called CO2 control knob theory of climate.

Right, this is a key issue of my paper.

R2.3. The Introduction begins with editorializing about the climate change debate and what AI bots have to say about the greenhouse effect. This material serves no purpose and should be deleted. The introduction should succinctly outline the two different perspectives on the terrestrial greenhouse effect, and the associated questions.

I understand why the reviewer thinks so, but I give here some information that hopefully could help Reviewer #2 and the Editor to understand that I think otherwise.

Since 2020 (in the Covid-19 period) I have produced 15 climate papers—and several others on different subjects. Out of these 15, 13 have been published, one is the present paper and one more is still in review.

In each one of the published papers, if I devoted an average period of time, say, τ to produce the paper, I had to devote an additional time 2τ to publish it. My rebuttals are often equivalent to additional papers. This rebuttal Report (including the information contained in its Introductory Notes above) provides a good example of how I usually struggle to publish each of my papers.

Do I have these difficulties because my papers contain errors? Definitely not! If errors were found, then I would either correct them or ask for the retraction of the papers. But not a single error was found. And none of the papers was retracted despite the systematic efforts of several colleagues who admittedly tried to persuade editors and publishers to retract my papers.

So, what is the reason for my difficulties? I have tried to describe the landscape of the climate debate as briefly, meaningfully and politely as I could. I could say much more—and I know much more—but will refrain from doing so, though that does not mean that I am giving up. On the contrary, as a retired Emeritus professor who does not need to improve his CV to get a better job and salary, I wish only to pursue the truth in my subject independent of political or economic interests.

For the last several decades, the climate debate has been ideological and political, not scientific. Consequently, it has been full of lies. I am confident that the search for scientific truth is the only way to reverse the cultural and social decay caused by lies. Our scientific associations have been turned into advocacy groups serving politico-economic agendas. They promote ideologies, rather than scientific ideas. This is not science but sophistry.

Having said all this, I fully understand and respect the suggestion of Reviewer #2 and, accordingly, I have rewritten the Introduction, removing the material in the original manuscript that the reviewer deemed purposeless. However, I insist that the original Introduction was appropriate and with high explanatory power—in particular regarding the reactions to my present paper (and my earlier ones). Therefore, I include the original Introduction in the present Report as an Appendix.[5]

R2.4. Section 2 on Theoretical Background presents much material on undergraduate thermodynamics, which does not seem appropriate for this journal and does not seem to be needed to make the primary arguments in this paper. Sections 2.1 should be deleted. Section 2.2 describes empirical relationships focused on surface longwave radiation flux, which are also not useful for the primary arguments in this paper; Section 2.2 should be deleted. Section 2 should begin with Section 2.3 on spectroscopic models.

I wanted to make a stand-alone, self-contained, paper. Yet I understand the reviewer's suggestion, as the detail I give may distract the reader from the main focus. On the other hand, what is contained in section 2 is not trivial. For example, Equation (A6) (produced by myself in my paper [28]) is not widely known, even though it is more accurate than other, widely used, equations. Equation (A7) is not contained anywhere else because it is new, produced for my present paper.

However, I appreciate the reviewer's suggestion. Section 2 is now reduced and most part of it is moved to the new Appendix A.

R2.5. Section 4 uses the output of spectroscopic models to develop macroscopic relationships for surface and outgoing longwave radiation. This is a valuable part of the paper.

I am grateful to the reviewer who was the only one to understand that this is a valuable part of the paper.

R2.6. Section 5 doesn't make a lot of sense to me. Yes, there is uncertainty in observations, but that does not really impact the experiments in Section 6. Section 5.2: What is the point of testing the MODTRAN results using the old empirical or semi-empirical formulae? Assuming that these formulae are more accurate than MODTRAN makes no sense. Seems like all of section 5 could be eliminated.

I understand the reviewer and I agree with her/his point that "*uncertainty in observations* […] *does not really impact the experiments in Section 6*". However, I respectfully disagree with her/his other

[5] In the subsequent submission to *Science of Climate Change*, I have reinstated the material removed from the original Introduction.

points in this comment. Definitely, there is meaning in testing the MODTRAN results using the old empirical or semi-empirical formulae.

The empirical formulae are based on data and I have the habits of (a) always testing models using data and (b) promoting this idea as much as I can.

Besides, what I am testing is not MODTRAN per se, but the macroscopic relationships for surface and outgoing longwave radiation, which I produced. Additionally, section 6 includes three different tests, in three subsections, which the reviewer might have overlooked: *5.1 Radiation Flux Profiles*, where the relationships are tested with radiosonde data; *5.2 Downwelling Radiation*, where indeed the testing is based on empirical formulae, which in turn were based on observed data (see also Koutsoyiannis and Vournas [4]); and *5.3 Outgoing Radiation*, where the testing is based on CERES data.

For these reasons, I thought it better not to eliminate Section 5.

R2.7. Section 6 is the heart of the paper, the experiments are well constructed but this whole section could use more text and explanation (plenty of space if you eliminate most of section 2 and all of section 5).

I am delighted to see the reviewer's opinion on section 6. Following her/his suggestion, I have tried to add more text and explanation. Specifically, a detailed mathematical explanation has been added now in Section 6.2 (below Figure 23), which reads:

> While the above graphs are suggestive of the low importance of [CO_2] in realistic real-world conditions, here we propose a more advanced method to quantify its relative importance in a more general and systematic manner. Let L be a quantity of interest—in our case the LW radiation flux—that depends on several factors F_i, the explanatory variables. To determine the relative importance of each of the factors F_i, we consider the relative change $\delta L / L$ produced by a relative change $\delta F_i / F_i$ in the factor F_i and we take the ratio:
>
> $$\frac{\delta L}{L} \Big/ \frac{\delta F_i}{F_i} = \frac{\delta L}{\delta F_i} \frac{F_i}{L} \tag{6}$$
>
> As δF_i becomes small, the ratio $\delta L / \delta F_i$ tends to the partial derivative $\partial L / \partial F_i$ and hence the above quantity becomes
>
> $$\frac{\partial L}{\partial F_i} \frac{F_i}{L} = \frac{L'_{F_i} F_i}{L} =: L^{\#}_{F_i} \tag{7}$$
>
> where $L'_{F_i} := \partial L / \partial F_i$ is the partial derivative of L with respect to F_i and $L^{\#}_{F_i} := \partial \ln L / \partial \ln F_i$ is the partial log-log derivative (LLD) of L with respect to F_i. Details about the properties of the LLD are given in [59] (p. 97).
>
> Considering all explanatory variables, the total differential is

$$\mathrm{d}L = \sum_i \frac{\partial L}{\partial F_i} \mathrm{d}F_i \tag{8}$$

and hence

$$\mathrm{d}(\ln L) = \frac{\mathrm{d}L}{L} = \sum_i \frac{\partial L}{\partial F_i} \frac{F_i}{L} \frac{\mathrm{d}F_i}{F_i} = \sum_i L^{\#}_{F_i} \frac{\mathrm{d}F_i}{F_i} = \sum_i L^{\#}_{F_i} \,\mathrm{d} \ln F_i \tag{9}$$

The partial LLDs, $L^{\#}_{F_i}$, reflect the relative importance of each F_i. For illustration, let us consider a quantity L affected by two factors F_1 and F_2. A small relative change $\delta F_1/F_1$ in F_1, equal to a, without any change in F_2, will result in a change of the dependent quantity L equal to $(\delta L/L)_1 = L^{\#}_{F_1}\, \delta F_1/F_1 = L^{\#}_{F_1} a$. Likewise, a small relative change $\delta F_2/F_2$, in F_2, again equal to a, without any change in F_1 will result in a change of $(\delta L/L)_2 = L^{\#}_{F_2} a$. Hence,

$$\frac{(\delta L/L)_1}{(\delta L/L)_2} = \frac{L^{\#}_{F_1}}{L^{\#}_{F_2}} \tag{10}$$

which means that the relative change in the quantity of interest due to changes in the explanatory variable is proportional to the partial LLD. Hence, Equation (9) allows the decomposition of the relative change $\mathrm{d}L/L$ due to the relative change $\mathrm{d}F_i/F_i$ of each of the different explanatory variables. Apparently, if the system studied is nonlinear (as most natural systems are), the partial LLDs are not constant. In this case, we must first specify a point of interest (with its coordinates F_i) and then calculate the partial LLDs for this point. The method is quite general and can be applied to any point of interest.

In addition, the following text was added below Figure 24:

In addition, it must be noted that the explanatory variables are not independent from each other. For example, absence of water vapor entails absence of clouds. Additionally, according to the mainstream narrative, an increase of [CO_2] results in increase of water vapor (with the mainstream regarding it to be a feedback of CO_2). But these dependencies, whether true (water vapor – clouds) or not (CO_2 – water vapor, regarded as a feedback) do not invalidate the methodology nor its results. The results in Table 6 have been produced for average conditions prevailing in the present atmosphere. If these conditions change because of the dependencies or any other reasons, the partial LLDs should be evaluated at a new point of the vector of explanatory variables.

R2.8. Sections 7 and 8 are very good.

I am delighted to see the reviewer's opinion on sections 7 and 8.

R2.9. In summary, publish with major revisions (that mostly consist of deleting a lot of unnecessary text).

I am grateful to the reviewer for her/his recommendation.

4.4 Reviewer 3

R3.1.

Reviewer review report

Overall Recommendation	( ) Accept in present form ( ) Accept after minor revision(I do not need to see the revised version) ( ) Reconsider after major revision(I want to see the revised version) (✓) Reject
English Language and Style	( ) Extensive editing of English language and style required ( ) Minor spell check required (✓) English language and style is fine ( ) I don't feel qualified to judge about the English Language and Style
Recommendations for Authors	★☆☆☆☆ Is the manuscript novelty enough? ★☆☆☆☆ Is the manuscript scientifically sound and not misleading? ★☆☆☆☆ Does the manuscript give a concise and comprehensive view of the topic? ★★★☆☆ Is the manuscript well organized and of proper length? ★☆☆☆☆ Is an adequate number of references investigated and appropriately quoted?

Interestingly, Reviewer #3 finds the manuscript neither novel enough nor scientifically sound. However, it is actually easy enough to see that the paper is novel. For example, just viewing the figures of the paper, one can see that out of 26 figures in the body of the paper, 24 are new, having not appeared anywhere else before. One can also see that the concept of partial derivatives, which is proposed as the key approach to assessing the relative importance of different factors, was not used before in relevant studies.

It is also interesting that Reviewer #1 concurs with Reviewer #3 in terms of the novelty and scientific soundness of the paper, while Reviewer #2 has the exact opposite opinion: five stars for novelty and five stars for scientific soundness.

R3.2. Major comments
It would help to have line numbers. This limits the comments.

Perhaps it is my omission but I did not receive such an instruction from the editorial office to include line numbers. Some publishers do not want line numbers in the submitted document as they add them automatically in the pdf they produce for the review phase.

R3.3. The paper's goal is to explore the greenhouse effect of water vapor vs carbon dioxide but it uses a radiative transfer model without a comprehensive framework of the full climate system, and, in particular, the energy and water cycles. Rather, the paper rejects all the scientific papers that deal with complex earth system models and has a number of jumps of logic to reach what appear to be pre-ordained conclusions.

With all due respect to the reviewer, I do not accept this criticism. My framework is comprehensive, novel and innovative. In particular, it produces generalized macroscopic relationships that support the calculation of partial derivatives. In my opinion, this is the only reliable and scientifically sound tool to approach the issue dealt with. My paper does not reject all the scientific papers that deal with complex systems. Rather it criticizes some of them—and I contend that criticizing other results has been the cornerstone of science, since its cradle some 2500 years ago (see historical details Koutsoyiannis and Mamassis [42]). Other papers I fully accept and build on their results (see e.g. replies on comments R3.26 and R3.31 below), which the reviewer seems to dislike. There are no "jumps of logic" and the conclusions are not pre-ordinated.

R3.4. There are many self references on several topics in obscure places and very few to accepted core literature. This is especially important for datasets, which are misused without adequate accounting for uncertainties and inhomogeneities, and several are not observations but model values.

Yes, the paper contains a large number of references. Some of them are to my works, upon which I build. I do not know any other way to build upon a previous work than to use it as a reference. I do not think that repeating what was published in another work would be a good solution. Neither is it a good alternative to omit a reference that is necessary to support a claim or to invoke a result already found somewhere else.

The reviewer's statement "*very few* [references are] *to accepted core literature*" is blatantly untrue. One can easily verify that these are the vast majority. And those which are not, are the platforms that provide data or software. They are more necessary than "*accepted core literature*". There were a few other references to authoritative sources such as a NASA web site, or a *Nature* editorial, etc. I did not use the latter in support of my views, but to criticize the current state of affairs. Anyhow, in the revised manuscript several of the references of the latter type have been removed and only appear in the Appendix of the present Report.

R3.5. The paper is correct that the residence time for a water molecule in the atmosphere is about 9 days. But the value for carbon dioxide is nowhere near correct. Carbon dioxide, ozone, methane, nitrous oxide and chlorofluorocarbons do not condense and precipitate as water vapor does. The result is orders of magnitude differences in the lifetime of these gases (decades to centuries) compared with the nine days for water vapor. Hence, they serve as a stable structure of the atmospheric heating as the climate changes, and the resulting temperature is what enables the levels of water vapor as a powerful feedback. The author fails to comprehend and allow for feedbacks in his arguments.

The residence times are entirely irrelevant to the intensity of the greenhouse and the relative importance of the different gases. It is the concentration that matters, not the residence time.

Besides the "official" (IPCC) "lifetime" estimates of "decades to centuries" are plain wrong, in my opinion. I have shown that in another paper, which hopefully will be published—I am struggling to publish it as I do with the present paper. The reviewer may see why the "official" estimates are wrong in Appendix A.2 of my paper "*Reservoir routing and its application to atmospheric carbon dioxide balance*" [21]. The correct estimate for the residence time of the CO_2 is the one I give in the presnt paper, i.e. less than 4 years. Therefore, there is nothing to change in my formulations.

But even if the CO_2 residence time were decades to centuries, nothing would change once the concentrations are specified. Therefore, any additional discussion would be out of the paper's scope.

I have now added the following sentence:

> Detailed analysis of the CO_2 residence time (which differs substantially from the official IPCC's one) can be found in [21].

The issue of feedback is replied to below, in Comment R3.29.

R3.6. Carbon dioxide is recycled on shorter time scales (the annual cycle), but not taken out of the system. Accordingly, CO2 and other long-lived greenhouse gases can be considered as "forcings" of the system while water vapor is a consequence and has feedback effects. This framing is mis-stated in the paper.

With all due respect to the reviewer, I do not agree. As I stated above, the long or short residence time is irrelevant to the problem examined. My framing is correct. What is wrong is mainstream opinion which is why it is very important to publish research findings that contradict the mainstream in order to encourage debate. Blocking the publication of my papers with hostile reviews (particularly when my research contradicts mainstream opinion) hinders, not helps, scientific dialogue and progress.

R3.7. Moreover, as the climate warms, the air can hold more moisture and does, so that water vapor goes up following the temperatures but only causing them through amplifying feedbacks. This paper instead treats water vapor as a forcing, and this is fundamentally wrong. This is evident in many ways, from the huge variability in water vapor from day to day in weather systems, and its huge dependency on latitude and altitude.

This is what the mainstream "science" says. However, I do not subscribe to that establishment. Rather, I try to see and analyze reality, as reflected in observed data. I have already made this analysis four years ago and proved that the reality does not correspond to what the established "science" says. See Koutsoyiannis (2020) [25].

R3.8. There is a basic lack of understanding of the flow of energy and water through the climate system. It also fails to realize the nature of the comprehensive radiative calculations performed in climate models. The most definitive study on this topic is ref [67] and it is dismissed out of hand. Instead, there is a hodgepodge of calculations using an inappropriate framework.

As reflected in its title, the subject of the study is to derive estimates of the relative importance of carbon dioxide and water in the greenhouse effect. I am doing this independently of other studies and with a new methodology, based on partial derivatives of the outgoing and downwelling longwave radiation with respect to water vapor and CO_2 concentration. I contend that this is the most scientifically sound methodology, and it is novel. The results differ substantially from those of Schmidt et al. [35] (reference [67] in the earlier article version), which the reviewer likes, as seen in her/his statement "*The most definitive study on this topic is ref [67]*". I do not dismiss that study by speculation but I use mathematical calculations to prove that its results are wrong.

Assuming that the reviewer is correct that this is the most definite study, and given that neither this reviewer nor any of the other reviewers found any error in my calculations, my study offers an important service to science as it contrasts earlier estimates that are wrong.

R3.9. The paper is correct in that water vapor is important radiatively [67]. For clear sky the best estimate is that the atmospheric greenhouse effect is 67% water vapor, 24% carbon dioxide, and others (esp ozone) 9%. But with clouds included carbon dioxide drops to 19%, others are 7% and water vapor is 50% with clouds making up 25%. So water substance: clouds and water vapor make up 75% in total. This is substantial but notas large as claimed here.

These are the estimates of Schmidt et al. which are included in my study, so there is nothing to change. According to my calculations, my estimates are substantially different. The reviewer correctly points out that the estimates by Schmidt et al. for clouds and water are not as large as those in the present study. Naturally, I claim that my estimates are correct and those by Schmidt et al. are wrong. The future will show which of the two studies is closer to reality.

R3.10. But water vapor is not well mixed: it falls off rapidly with height (and temperature) and with latitude. Those aspects alone mean that the full 4D structure (3D spatially plus time) matter, and are not dealt with in this paper. Time includes the diurnal cycle and the annual cycle. None of these aspects are addressed here.

Of course, water vapor falls off rapidly with height (and temperature). This is fully taken into account in the MODTRAN model and in my paper. See the profiles in Figure A1 (lower left panel).

The diurnal and annual cycles are fully reflected in the data that are retrieved and processed in the study.

Furthermore, the generalized relationships developed in the paper include temperature and water vapor ranges that depart considerably from the typical profile values. Therefore, they fully cover the variability due to the diurnal and annual cycles. Therefore, there is nothing to add or change in the paper.

R3.11. The time series of observed LW radiation is irrelevant to determine the attribution since, as the climate warms, temperatures increase, and the system necessarily radiates more to space. Most thermal infrared radiation is absorbed by the atmosphere and re-emitted both up and down. A lot of the downwelling radiation comes from the lower atmosphere where most water vapor exists, and some is reflected at the surface. The radiation lost to space comes from cloud tops and from parts of the atmosphere much colder than the surface, producing a greenhouse effect.

Contrary to what the reviewer states, the time series of observations are the only tool to check whether our theories are right or wrong. This is the cornerstone of science, as opposed to speculation and fiction.

Obviously, temperature increase causes increase of the LW radiation, both outgoing (to space) and downwelling. This is not speculative but accurately quantified in Equation (2) with parameters as in Table 1.

R3.12. Some details
It would help to have line numbers

This has already been answered in Comment R3.2.

R3.13. The opening of the introduction is inappropriate

Following Reviewer #2's suggestion (her/his Comment R2.3) I have now changed the Introduction. However, I insist that the original introduction was appropriate and with high explanatory power—in particular in explaining the hostile reactions to the present paper. Therefore, I include the original Introduction in the present Report as an Appendix.

R3.14. P 2:
What an AI bot says is irrelevant.

Unfortunately, I had to remove what the bot says, but still, I believe it is quite relevant. As stated above, it can be found in the Appendix of this Report.

R3.15. There are many self references to articles published in obscure places and no references to accepted values.

I have already replied to this issue in Comment R3.4 above. It is interesting that the phrase “obscure places and **very few** to accepted core literature” in Comment R3.4 (emphasis added) has now become “obscure places and **no** references to accepted values”

R3.16. P 3 much of this is simply not correct.
The paper is correct that the residence time for a water molecule in the atmosphere is about 9 days. But the value for carbon dioxide is nowhere near correct. That is many centuries, although a lot is recycled on shorter time scales. Accordingly, CO2 and other long-lived greenhouse gases can be considered as “forcings” of the system while water vapor is a consequence and has feedback effects. Many of the values stated on p3 para 2 are wrong.

I have already replied to these issues—see my replies to Comments R3.5 and R3.6 above.

R3.17. Section 2 is irrelevant. Please see the radiation codes in line by line calculations and their approximations in climate models. Evaporation is not well known. But it has to satisfy both energy and water cycles.

Section 2 is now reduced and most of it moved to an Appendix; see also my reply to Reviewer #2’s Comment R2.4. Testing the radiation codes line by line is not in the scope of the paper. The code has been tested for decades by others. As mentioned in the paper (Section 2):

> here we use the MODerate resolution atmospheric TRANsmission model, or MODTRAN [43-45], which has been extensively validated in its over 30-year history, and serves as a community standard atmospheric band model.

What is irrelevant is the approximations of radiation codes in climate models. The paper does not need to resort to climate models, thus avoiding their problems which are explained in my reply to Comment R3.34.

R3.18. 2.3 the model must deal with the full spatial and temporal variability of water vapor.

It does. That is why in Section 4 I have applied the model with a range of the explanatory variables: temperature, water vapor pressure, CO_2 concentration, cloudiness, other greenhouse gases.

R3.19. Section 3 first seizes on two soundings at a location. These are used later and are not global nor comprehensive in time.

Yes, they are not global. The first paragraph of Section 3 clarifies the reasons (emphasis added):

> To compare MODTRAN results to observed radiation profiles in the atmosphere, we need radiosonde data. While radiosondes are routinely made in several hundreds of sites across the world, they typically measure temperature, humidity, pressure and wind. **Radiation radiosonde measurements are rare, yet it is useful to make at least a single comparison to get a general idea.** Here we use a couple of radiosondes from a day and a night flight on 23 September 2011, in cloud-free conditions, at the aerological station in Payerne, Switzerland (6.9440° E, 46.8130°, +491 m a.s.l.). In these, in-situ measurements of downward and upward radiation fluxes were taken through the troposphere and into the stratosphere, exceeding 32

km of altitude. They were presented by Philipona et al. [50] in graphical form in their figure 2, which was digitized here to recover the measurements.

R3.20. P9
It then uses ad hoc datasets from CERES and reanalyses without adequate care as to their homogeneity and consistency. Many papers exist on these aspects. Some CERES products result from a model, including all cloud-free values. The TERRA satellite is not stable and calibrations drift. EBAF attempts to adjust for these.

Since the reviewer thinks that CERES and reanalyses are ad hoc datasets without adequate care as to their homogeneity and consistency, I invite her/him to share this observation with NASA, NOAA, and ECMWF. Until she/he persuades them to withdraw their “ad hoc” datasets, I have no other option than to use them.

The paper uses datasets from observations, yet computed results like those in EBAF have also been considered. This is made clear in the following paragraph in Section 5:

> Another comparison is made in Figure 17 for the downwelling LW radiation flux vs. temperature, as calculated by MODTRAN and the CERES EBAF zonal distribution shown in Figure 4. For the former all five locality profiles are used with default settings as well as with temperature offsets from the default values of up to ±25 K. We recall that CERES EBAF data are not actually measurements but computed results.

R3.21. The ocean heating is woefully wrong. Many papers on this, none referred to.

The ocean heating is not in the paper’s focus, yet it is discussed in the frame of the CERES data. Note that I have made my own calculations, which I refer to in the paper. They could be wrong, as the reviewer thinks, but I have published them in Koutsoyiannis (2001) [5925] and after three years no errors have been spotted. Anyhow, I give several estimates, now adding one more. The relevant text of Section 3 now reads:

> In addition, as also noted in CERES [46], with the most recent CERES instrument calibration improvements, there still is a net imbalance of ~4.3 W/m², much larger than the expected observed ocean heating rate which CERES assumes to be ~0.71 W/m². The latter value is not far from that of Trenberth et al. [47], who give the net absorbed energy at 0.9 W/m². However, according to the calculations by Koutsoyiannis [25], the latter imbalance value, again inferred from ocean heating data, is even lower, 0.37 W/m². The EBAF dataset adjusts the observations to remove the above inconsistency. All this information suggests that the observational uncertainties are far too high to allow calculations of Earth’s imbalance and of temporal climatic changes, yet they are quite useful for the scope of this study.

R3.22. NCEP reanalyses are dated and seriously flawed and should not be used at all. They did not include data from water vapor. ERA5 reanalyses are quite good except water vapor suffers from inhomogeneities in the data sources (microwave).

I invite the reviewer to contact NCEP/NCAR and let them know that their reanalyses are dated and seriously flawed so that they announce that we should not their reanalyses any more. Until this happens, I will use them, in addition to other relevant products. Besides, I also use the ERA5 Reanalyses which the reviewer prefers. The relevant text in the paper (Section 3) is this:

Additional atmospheric variables used here, namely temperature and water vapor pressure, are taken from the ERA5 and NCEP/NCAR Reanalyses at monthly scale. ERA5 stands for the fifth-generation atmospheric reanalysis of the European Centre for Medium-Range Weather Forecasts (ECMWF; ECMWF ReAnalysis). Its data are publicly available for the period 1940 onwards at a spatial resolution of 0.5°. NCEP/NCAR stands for Reanalysis 1 by the National Centers for Environmental Prediction (NCEP) and the National Center for Atmospheric Research (NCAR). Its data are publicly available from 1948 to the present at a horizontal resolution of 1.88° (~ 210 km). Both data sets can be retrieved from the Climexp platform [48] and from the Physical Sciences Laboratory platform of the US National Oceanic and Atmospheric Administration (NOAA) [49].

R3.23. Fig 2 is misleading as the contour intervals are not constant.

This comment is a really interesting one. I wonder why the reviewer thinks that the contour intervals are not constant, and why she/he thinks that the figure is misleading. Do I intend to mislead the readers? Below I copy the figure, in which it can be seen that the intervals have a constant step of 20 W/m^2. The caption provides the link to the software system that I used to produce the figure and therefore, if the reviewer thinks that the figure is misleading, she/he can try her/himself and address her/his accusations of producing misleading figures to the developers of the Climexp platform and the Grid Analysis and Display System (GrADS).

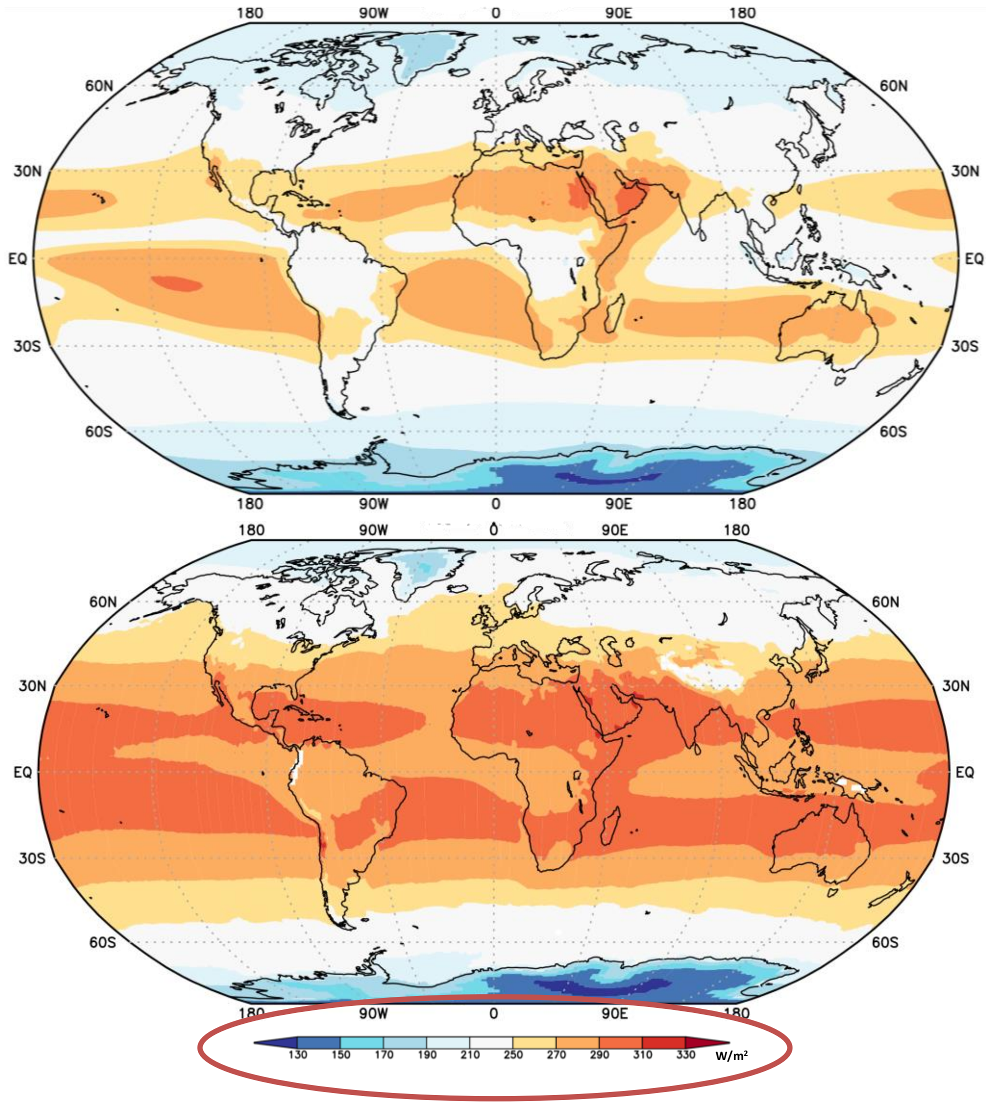

Figure R3 [original caption in paper follows]
Figure 2 Geographical distribution of outgoing LW radiation averaged over the period of 2000 – 2022 as given by the CERES data: (**upper**) all sky; (**lower**) clear sky. Data retrieved from https://ceres-tool.larc.nasa.gov/ord-tool/jsp/SSF1degEd41Selection.jsp; graph generated by https://climexp.knmi.nl.

If the reviewer complains because the central interval's step is double, she/he can try her/himself and address the accusation about misleading figures to the developers of the Climexp platform and the Grid Analysis and Display System (GrADS). Their system always produces graphs with this convention. As an additional example, I give in Figure R4 a screenshot from Climexp readily produced for the indicated temperature field, which the reviewer can conveniently reproduce and see that the middle interval has double the size of other intervals.

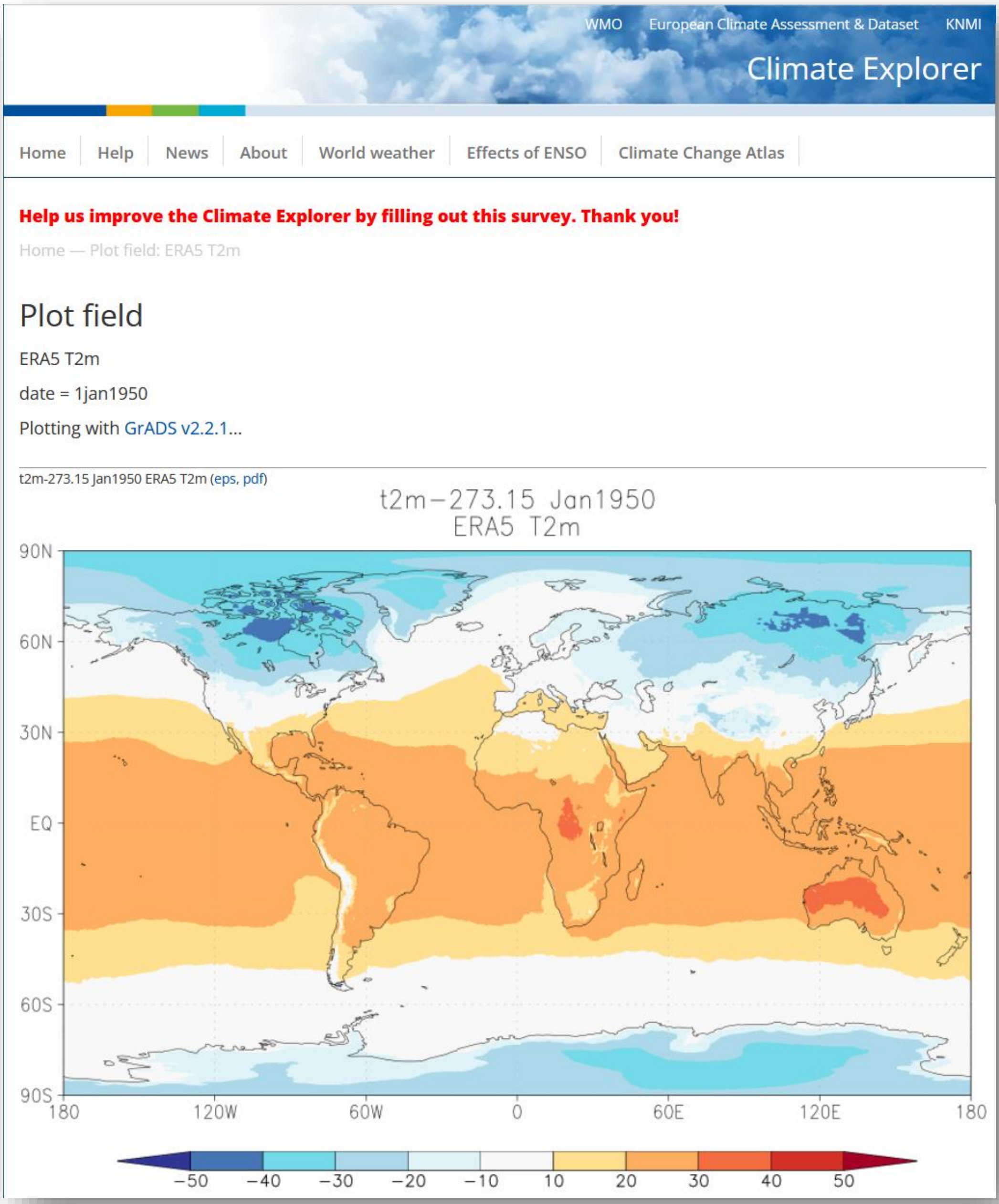

Figure R4. A screenshot with a graph generated by https://climexp.knmi.nl for the monthly temperature fields from the ERA5 reanalysis and for the indicated period.

For me, the graphs produced are just fine and I have no complaint at all. On the contrary, I feel grateful, and I have now made the relevant mention in the Acknowledgments section as follows:

> I am grateful to the colleagues and organizations who have put their huge data sets online along with the data processing and computational systems they have developed. These include the CERES data, the ERA5 and NCEP/NCAR Reanalyses, the CLIMEXP data and software platform, and the MODTRAN and RRTM software systems.

R3.24. What is "temperature", such as in Figs 4, 5 and 6? What level etc. Radiation to space depends on 4D structure and cloud tops.

It is difficult to understand the meaning of this comment. All figure captions provide full details. For example, the caption of Figure 4 is:

> **Figure 4** Zonal distribution of LW radiation averaged over the period of 2000 – 2022 as given by the CERES data: (**upper**) downwelling; (**lower**) outgoing; the temperature zonal distribution, as given by ERA5 Reanalysis is also plotted. Radiation data retrieved from https://ceres-tool.larc.nasa.gov/ord-tool/jsp/SSF1degEd41Selection.jsp for outgoing and https://ceres-tool.larc.nasa.gov/ord-tool/jsp/EBAF42Selection.jsp for downwelling; temperature data retrieved from, and graph generated by, https://climexp.knmi.nl.

So, I wonder what the reviewer would want to know in addition to the information that what is plotted is the temperature zonal distribution, as given by ERA5 Reanalysis. The ERA5 Reanalysis is available online in several platforms (e.g. Climexp), and the reviewer may feel free to remake and test the plot; it is easy.

R3.25. Section 4 is about modtran and irrelevant. These kinds of things, if worthwhile need to account for all the globe and all times, day and night and seasonally. They don't.

I respectfully disagree. The reviewer may feel free to do this her/himself. I certainly prefer to make something transparent and verifiable by anyone interested, without requiring demanding computational means, such as climate models etc. My calculations and results are representative for all typical profiles and a lot of modifications thereof. I am confident that Section 4 has given very general, novel and powerful results—perhaps this is what irritated the reviewer? And these general results allow for the calculation of partial derivatives that offer the only reliable mathematical technique to properly assess the importance of the different agents of the greenhouse effect.

R3.26. Section 5.1 uses two stations.

No, it's two radiosondes at one station. This is described in the first paragraph of the section:

> As already mentioned (Section 3), the most appropriate way to test the validity of a model that determines the LW radiation, such as MODTRAN, would be to compare its results to observed radiation profiles. As described in Section 3, here we make a single comparison to get a general idea, using the two radiosondes launched on 23 September 2011 at Payerne, Switzerland, and reported in the study by Philipona et al. [50], whose LW radiation profiles were digitized here.

The first paragraph of Section 3 clarifies the reasons; see my reply to Comment R3.19 above.

R3.27. 5.4 fixed relative humidity means increasing water vapor with temperature!

The reviewer is right. Actually, this is well known, but I have also given the relevant details in Section 2.1—see Equation (A8) and combine it with Equation A6) (produced by myself in my paper [28]) and (A7) (produced by myself in my present paper).

R3.28. Section 5.4 concludes that the results are all over the place and have large uncertainties, but then they use them anyway. There is inappropriate use of data in many places.

If we would not use results that are uncertain, we would not do science at all. Everything is uncertain to a lower or higher degree. My approach is to do science with uncertainty and to emphasize, rather than hide, the uncertainties. This is precisely described in the following text, to which I have nothing to change:

> *5.4 Final Assessment*
>
> The above tests illustrate the high uncertainties not only in the CERES LW radiation data, but also in the other atmospheric variables, and the relationships among them and the LW radiation, as represented in MODTRAN. The uncertainties do not allow accurate representation of quantities calculated as differences between different variables or between the same variables in different periods, such as in attributing changes. On the other hand, the macroscopic behaviour of MODTRAN seems consistent with what is observed for clear sky, and therefore MODTRAN is suitable for the scope of this paper, which is the investigation of the relative importance of carbon dioxide and water in the greenhouse effect, as detailed in the next section. As regards clouds, MODTRAN seems to underestimate their effect, but by using the cumulus or altostratus cloud conditions, we get results close to reality for average all-sky conditions.

R3.29. Section 6 has some results for a model, but does not allow feedbacks. The assumptions are not viable. And the conclusions wrong.

It is interesting to see the contradiction with Reviewer #2 who in Comment R2.7 opines:

> Section 6 is the heart of the paper, the experiments are well constructed but this whole section could use more text and explanation.

The feedbacks are, indeed, an issue, but first we should find out which the driver is and which the feedback is. And Section 6 provides such information, which is summarized in Table 6 and especially in Figure 24. The phrase "*Does the tail wag the dog?*" in the title of the paper reflects the importance of deciding which the dominant factor is before we examine the feedbacks. It is relevant to repeat here the following paragraph from Section 7:

> The distinction between feedbacks and forcings, also appearing in the quoted statements by Lacis et al., is problematic. Both H_2O and CO_2 have always been present on Earth and both are greenhouse gases, with the difference being that the former is much more abundant in the atmosphere and determinant for the greenhouse effect, as already demonstrated. Calling CO_2 forcing and H_2O feedback is like claiming that the tail wags the dog.

R3.30. Section 7 fails to recognize that water is rained out of the atmosphere if it gets cold. It does not account for the short lifetime of moisture and is mistaken in discussing forcings and feedbacks. Moreover, it does not realize that energy is moved around primarily by storms and convection: atmospheric dynamics. The latter warm the troposphere which is cooled by radiative processes. Yes latent heat helps but convection of all sorts matters a lot more and is not included.

Good for the reviewer to remind me that the water is rained, but it is unnecessary as I am a hydrologist and I know those mechanisms. Besides, being a Greek, may I remind the reviewer that this has been known since 2500 years ago (cf. Aristotle and other Greek philosophers; see Koutsoyiannis and Mamassis [42]).

R3.31. Fig 26 fails to include the greenhouse effect or any movement of energy laterally. Since it does not rain much at high latitudes, and precipitation is very uneven, atmospheric dynamics play major roles, not included.

For the readers' convenience, Figure 26 is reproduced below.

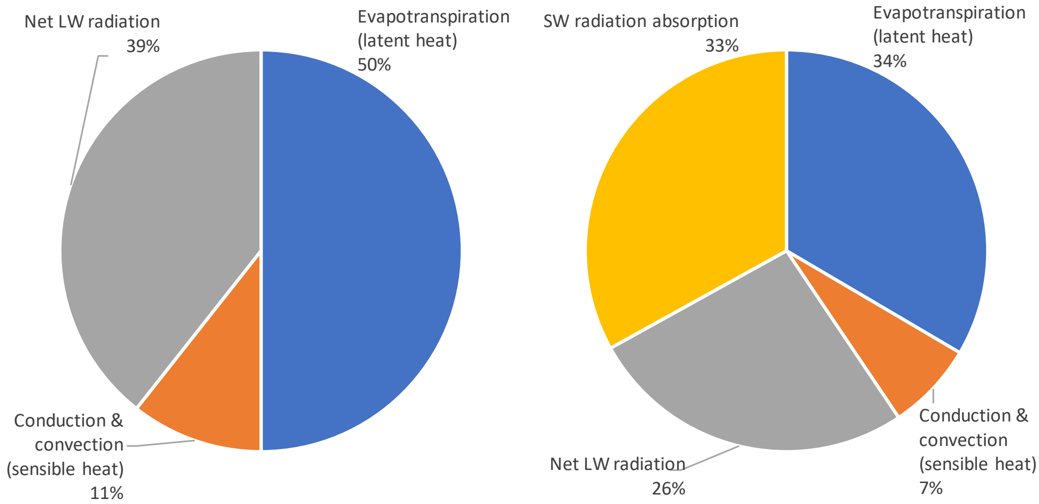

Figure R5 [original caption in paper follows]
Figure 26 Contribution of (**left**) the three mechanisms responsible for the cooling of Earth's surface and (**right**) the four mechanisms responsible for the warming of Earth's atmosphere, based on the energy balance by Trenberth et al. [47].

As clearly stated in the caption, this was totally based on the global energy balance by Trenberth et al. [47]. If the reviewer disagrees with Trenberth et al., e.g. she/he finds that they did not include any movement of energy laterally, she/he may feel free to dispute their results in a paper discussing this energy balance. Personally, I find this criticism irrelevant, as the energy balance by Trenberth et al. is global, and, in a global setting, the horizontal movement is irrelevant.

R3.32. "Turbulent motion" is mentioned but quite incorrectly as a mass.

I can state with confidence that I am not as ignorant as the reviewer assumes. I know what turbulent motion is and what mass is. The relevant statement, repeated below, is:

> Remarkable is its *abundance* on Earth, as only the part that is in turbulent motion amounts to 1.34×10^9 Gt (not counting quantities that are stored in the soil, ground and glaciers), 260 times larger than the total mass of the atmosphere.

It is 100% correct and clarifies that the given mass quantity is for the water that is in turbulent motion, i.e. oceans and surface water, and does not include soil and groundwater, whose motion is typically laminar, and glaciers, in which the water is solid.

R3.33. The author goes on and on about water's importance and never mentions or realizes the role of the ocean, and earlier, grossly underestimated the heat uptake by the ocean. The arguments are grievously and fatally astray.

The oceans are not in the focus of this paper. In addition, the reviewer fails to specify the arguments and the reasons why she/he uses insulting phrases such as "*grievously and fatally astray*".

R3.34. Section 8 is simply wrong. It illustrates a huge lack of understanding of how the climate system works, including the hydrological cycle. It never mentions scores of climate model results that increase carbon dioxide and produce large warming: larger than observed, since some is offset by aerosol/cloud cooling.

Understanding is not objective but subjective. My understanding is different from the reviewer's. I did not criticize the reviewer's understanding and I did not invite her/him to criticize mine.

As per the climate model results, they clearly are out of the scope of the present paper. Nonetheless, I invite the reviewer to read the several papers I have authored or coauthored, which have shown that the results which she/he has referred to are poor and mostly have nothing to do with reality. Specifically, the poor performance of climate models in representing real-world processes can be seen in papers [14,15,16,17,51,52]. In addition, in paper [25], I have shown the poor performance of the climate models in representing the hydrological cycle. Most importantly, the recent paper by Koutsoyiannis et. al. [8] shows that the causality direction in climate models is opposite to that identified from real-world data. However, as I stated, this discussion is out of the scope of the paper and its potential inclusion would be a distraction.

4.5 Appendix: Introduction of the original version of the paper

A notable feature of the current period is that the classical value of science as the pursuit of the truth, independently of other interests, is gradually being abandoned [42]. People pride themselves on being scientists and (political) activists at the same time [8], while calls for political actions to "save the planet", including enhanced global governance mechanisms, are published even in scientific journals (e.g. [53]).[6] A relevant example is provided by the high-profile journal *Nature*, which declares that it is "committed to supporting the research enterprise" [54], but admits being involved in politics and proclaims that this is the right thing [55,56], even though it is recognized that this affects scientific credibility and causes loss of confidence in science [57,58]. It is thus natural to wonder if articles published in scientific journals promote the truth or political aims, perhaps hiding or distorting the truth, and even reversing cause and effect [6,7,8,59].

Seeking truth may have been even more difficult due to a recent development, the appearance of artificial intelligence (AI) bots, which certainly affects common opinion, as well as scientific perception and advancement. Assuming that AI bots more or less reflect the common perception in the scientific community and the general public, the Microsoft Bing's Chat bot was asked the following question:

> *Which gas determines the greenhouse effect on Earth?*

The bot provided the following answer (also supporting it with references [60,61,62]):

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[6] At this place, initially there were also a couple of sentences that the Editor of MDPI *Hydrology* disliked and asked to remove. For completeness, I include them in this footnote.

> While common perception promotes the idea of science-based policies and politics, the reality is the exact opposite, i.e., politics-based science. For it is self-evident that by mixing science and politics the end product is politics.

> *The greenhouse effect is the process through which heat is trapped near Earth's surface by substances known as 'greenhouse gases.' These gases consist of carbon dioxide, methane, ozone, nitrous oxide, chlorofluorocarbons, and water vapor. Water vapor, which reacts to temperature changes, is referred to as a 'feedback', because it amplifies the effect of forces that initially caused the warming. Scientists have determined that carbon dioxide plays a crucial role in maintaining the stability of Earth's atmosphere. If carbon dioxide were removed, the terrestrial greenhouse effect would collapse, and Earth's surface temperature would drop significantly, by approximately 33°C (59°F).*

A literature search reveals that the AI bot essentially (and selectively) reproduces the following statements by Lacis et al. [34]:

> *Noncondensing greenhouse gases, which account for 25% of the total terrestrial greenhouse effect, thus serve to provide the stable temperature structure that sustains the current levels of atmospheric water vapor and clouds via feedback processes that account for the remaining 75% of the greenhouse effect. Without the radiative forcing supplied by CO_2 and the other noncondensing greenhouse gases, the terrestrial greenhouse would collapse, plunging the global climate into an icebound Earth state.* […]
>
> *If the global atmospheric temperatures were to fall to as low as $T_S = T_E$* [where T_E = 255 K is the global mean effective temperature] *the Clausius-Clapeyron relation would imply that the sustainable amount of atmospheric water vapor would become less than 10% of the current atmospheric value.*

On the other hand, Koutsoyiannis and Vournas [4] recently examined longwave radiation observations extending over a period of 100 years and found that the observed increase of the atmospheric dioxide concentration ([CO_2]; from 300 to 420 ppm) has not altered, in a discernible manner, the greenhouse effect, which remains dominated by the quantity of water vapor in the atmosphere. Naturally, given the uproar about the enhancement of greenhouse effect due to human emissions by fossil fuel combustion, this finding appeared surprising to many. Some (including a knowledgeable reviewer of [4]) postulated that this would be expected for the downwelling longwave (LW) radiation flux, which was the subject of Koutsoyiannis and Vournas [4], but would not be the case for the outgoing radiation, where the effect of [CO_2] increase would be substantial. However, no long data series exist to verify such a conjecture and hence this question was not investigated in [4], whose scope was to make inference based on data.

Hence the following research questions are raised:

1. Are Lacis et al. (and the bot) right about the importance of CO_2 in the greenhouse effect, and is it meaningful to state that without it the terrestrial greenhouse would collapse? Or is the effect of CO_2 negligible as Koutsoyiannis and Vournas [4] claimed, and that of H_2O dominant?

2. Is the role of H_2O as a greenhouse gas limited to the downwelling LW flux or does it extend also to the outgoing LW flux?

We will examine these questions below, noting that the first one refers to a fictitious case (removal of atmospheric CO_2) for which no empirical data can exist. Rather, paleoclimatic studies and geological facts suggest that CO_2 existed, mostly in much higher concentrations than today, in most periods of Earth's history, and also before oxygen appeared in the atmosphere. Also, the second question cannot be studied on an empirical basis, as no long-term data exist (systematic satellite measurements of outgoing LW flux have only been made in the 21st century). Therefore, to study these questions we need to resort to theoretical arguments and analyses. We will do this by applying the established greenhouse theory and by enrolling standard models, without considering doubts that have been cast on the validity of the theory or alternative hypotheses (e.g. [19,20]).

Human CO_2 emissions are 4% of the total8] but there are also human H_2O emissions over the terrestrial part of Earth of a comparable percentage [22,23]. Specifically, according to Koutsoyiannis [25] the quantity of evaporation and transpiration over land is 91 400 km^3/year. According to Food and Agriculture Organization of the United Nations [26], the human water withdrawal in 2010, including the evaporation from reservoirs, was 4 300 km^3/year, of which 69% and 19% were for agricultural and industrial use, respectively. Considering the fact that almost all of agricultural and a large part of industrial water are evaporated, as well as the increasing trend in water withdrawal, with simple calculations we may conclude that the current human addition to natural water cycle over land is about 4%. One could speculate that each of these 4% additions might influence climate to a degree comparable to that percentage, but the reasons that only the influence of CO_2 is investigated and highlighted by the scientific community, being regarded a driver of climate, despite the fact that H_2O is much stronger as a greenhouse gas, should be non-scientific.

We may note, though, that this percentage becomes lower, of the order of 1%, if we also consider the evaporation over oceans. However, it appears reasonable for this estimate of human water emissions to consider only the terrestrial part of Earth, because of the high local variability and the small residence time of atmospheric water. Indeed, the mean residence time of atmospheric water is a few days (specifically, 12 250 km^3 / (522 700 km^3/year) = 0.023 years = 8.6 d, where the total volume of liquid water in the atmosphere, 12 250 km^3, was taken as the average atmospheric water content, 24.0 kg/m^3, as estimated from the ERA5 Reanalysis over the globe for the period 1950-2023, while the atmospheric inflow volume of water, 522 700 km^3/year, was taken from [23]). In contrast, CO_2 is well mixed as it has a much higher residence time, of a few years (specifically, 870 Gt / (226.9 Gt/year) = 3.8 years, where the total mass of atmospheric CO_2, 870 Gt was taken from the Intergovernmental Panel on Climate Change (IPCC) report [27], namely its figure 5.12, while the atmospheric inflow mass, 226.9 Gt/year, was taken from [25]). Detailed analysis of the CO_2 residence time (which differs substantially from the official IPCC's one) can be found in [21].

The thesis expressed in this paper is that none of these anthropogenic additions to hydrological and carbon cycle drives climate. On the other hand, both H_2O and CO_2 are important elements of climate and their quantities and fluxes are determined by natural processes, with the human factor being rather negligible. Both are elixirs of life and in this respect they act complementary to each other. Thus, it may be pointless to compare them to each other. Yet this comparison is the main focus of this paper, as lately the scientific efforts to study each of them has been inversely proportional to their respective importance.

By its definition by UNESCO [63], hydrology is the science which deals with the waters of the Earth, and its domain covers the entire history of the cycle of water on the Earth. Water is a critical element of life and of climate as well. While climate has become a hot topic and its research a top priority, it is odd that hydrology has lost importance, as evident from the abundance of papers examining climate change impacts and applying model projections for the future based on CO_2 emission scenarios. This totally misses the fact that water is the dominant element on Earth in driving climate and that the hydrological cycle is self-ruling rather than a feedback or impact of another cycle—namely the carbon cycle, which has also been downgraded to an issue governed by human carbon emissions (the 4% of the total).

By emphasizing the relative importance of water in climate, in comparison to carbon dioxide, this paper tries to show that the picture of Earth's climatic system may have been distorted, and to give prominence to hydrology (and its branch of hydrometeorology) in climate. The paper is made as a stand-alone and therefore it includes a synopsis of the related theoretical concepts and a model (Section 2). Its foundation is not only theoretical, but also empirical, utilizing observed data (Section 3). By combining the model and data, it extracts simple macroscopical empirical relationships representing the greenhouse effect as accurately as the detailed model whose results are based upon (Section 4). These relationships are tested against observational data (Section 5) and their simple and analytical expressions, which enable extraction of partial derivatives, allow the comparison of the effect of water relative to other greenhouse gases (Section 6). The findings are put in a more general context (Section 7) and allow drawing relevant conclusions (Section 8).

4.6 References

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5 Information file for the rejection (Round 2) by *Ecohydrology and Engineering*

5.1 External editor's opinion

Notes for authors

Lack of Novelty: The manuscript does not present novel findings or insights. The dominance of water vapor and clouds in the greenhouse effect is well-established in climate science. Therefore, the conclusions drawn do not provide significant new contributions to the existing body of knowledge. Methodological Flaws: The manuscript relies on empirical formulas and satellite data for validation without sufficient detail on the model's calibration and accuracy. This lack of methodological rigor and transparency raises questions about the reliability of the results. Misinterpretation of Data: The manuscript appears to downplay the role of carbon dioxide in the greenhouse effect, contradicting established scientific consensus. The claim that a rise in CO_2 from 300 to 420 ppm has a negligible effect on downwelling and outgoing radiation is inconsistent with a large body of evidence indicating the significant impact of CO_2 on global warming. Unsupported Claims: The manuscript suggests that hydrology should play a more prominent role in climate research based on the findings. However, this claim is not sufficiently justified or supported by the data presented. The role of hydrology in climate research is already recognized, and the manuscript fails to provide compelling evidence for a shift in research focus. Inadequate Discussion of Uncertainties: The manuscript does not adequately discuss the uncertainties associated with the atmospheric radiative transfer model used or the empirical relationships derived. This lack of discussion limits the manuscript's scientific validity and robustness. Grammatical and Structural Issues: The manuscript contains several grammatical errors and awkward phrasing, such as "We validate them these macroscopic relationships" and "thoughthrough latent heat transfer." These errors suggest a lack of careful proofreading and editing, reducing the manuscript's overall quality and readability. Overstatement of Results: The manuscript overstates the implications of its findings, particularly in the conclusion that "hydrology should have a more prominent and more active role in climate research." This assertion is not sufficiently backed by the results and may reflect a bias rather than an objective scientific conclusion. Lack of Comprehensive Review of Literature: The manuscript does not provide an adequate review of existing literature on the role of different greenhouse gases in climate change. This omission prevents the reader from understanding how the current study fits into the broader context of climate science.

5.2 Decision email

Subject:[Hydroecology and Engineering] Decline for Publication of Manuscript ID:hee00317
Date: Wed, 04 Sep 2024 16:39:25 +0800
From: xxxx
To: dk@itia.ntua.gr
CC: hee@sciepublish.org

Dear Prof. Dr. Koutsoyiannis,

Thank you for submitting the following manuscript to Hydroecology and Engineering:

Journal: Hydroecology and Engineering

Manuscript ID: hee00317

Title: Relative importance of carbon dioxide and water in the greenhouse effect: Does the tail wag the dog?

Authors: Demetris Koutsoyiannis*

We are writing to inform you that we will not be able to process your paper further. Papers sent for peer-review are selected on the basis of discipline, novelty and general significance, in addition to the usual criteria for publication in scholarly journals. Therefore, our decision is not necessarily a reflection of the quality of your research.

You may find the review reports as well as our academic editor's comments from the following link: [link deleted]

We wish you every success if you choose to submit the paper elsewhere.

Kind regards,

xxxx
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Hydroecology and Engineering Editorial Office
https://www.sciepublish.com/journals/hee

SCIEPublish,
https://www.sciepublish.com
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5.3 Addendum to decision email

Subject: Re: REMINDER: [Hydroecology and Engineering] hee00317
Date: Wed, 4 Sep 2024 16:43:44 +0800
From: hee@sciepublish.org <hee@sciepublish.org>
To: Demetris Koutsoyiannis <dk@itia.ntua.gr>
CC: xxxxx

Dear Prof. Koutsoyiannis,

We are sorry that we have brought you a negative decision.

We invited a member of our editorial board to check your manuscript. He gave a decision to reject the manuscript and provided detailed comments. Based on the principle of peer review, we are unable to publish your manuscript now.

We very much recognise the contribution of your research to greenhouse research. We hope that your manuscript will be successfully published in other journals.

Kind regards,

xxxxx

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5.4 Reply to decision emails

Subject: Re: REMINDER: [Hydroecology and Engineering] hee00317
Date: Wed, 4 Sep 2024 14:15:50 +0300
From: Demetris Koutsoyiannis <dk@itia.ntua.gr>
To: hee@sciepublish.org <hee@sciepublish.org>, xxxx, yyyy

Dear xxxx and yyyy,

Let me remind you of the main points from the timeline of our exchanges and after that ask you a question.

1. On 2023-12-11, with a follow-up on 2023-12-18, you invited me to join the Editorial Board of your journal /*Hydroecology and Engineering*/.

2. On 2023-12-21, I replied that due to health problems in my family I do not have the time required for this commitment.

3. On 2023-12-21, you wrote to me that you hope will have a chance to cooperate in future.

4. On 2024-05-23, you sent me a feature paper invitation (full APC waived).

5. On 2024-6-4, I sent you the preprint of the paper and stated, "If the editors and the reviewers find it suitable for publication, then I am accepting your invitation." Furthermore, I notified you that "my paper contrasts conventional wisdom. Hence, systemic journals do not want to publish it, despite the fact that my paper is correct, novel and innovative."

6. On 2024-06-07, you confirmed your invitation about this specific paper and you stated: "We believe in the importance of diverse perspectives in scientific discourse."

7. On 2024-06-07, I submitted the paper.

8. On 2024-07-22, I received a decision for a major revision.

9. On 2024-07-22 I wrote to you "I can infer with safety that these reviewers [#1 and #3] would never agree the paper to be published. They will always be negative. The reasons why these (and others with similar mindset) will always be negative can be easily inferred from the Introduction of the paper. I also communicated a possible plan for how to proceed. Specifically, I wrote: "My plan is to address the review comments of Reviewer #2, which I find constructive. As per Reviewers #1 and #3, I plan to rebut each one of their comments in full detail, and, in case that I find any constructive suggestion among these comments, to follow that particular suggestion."

10. On 2024-07-26, you agreed with my plan, stating "I encourage you to proceed with your plan to revise and resubmit the paper. Please ensure that your rebuttals are detailed, respectful, and focused on improving the manuscript."

11. On 2024-07-28, I submitted the revised version in full accordance with your advice.

12. Between 2024-08-05 and 2024-08-22 we exchanged several emails in which I expressed my concerns for the delay, also clarifying that "I provided the rebuttals to facilitate the editor to make the decision--not to persuade these reviewers. They will never be persuaded because their mission is simply to block my paper."

13. On 2024-08-29 you wrote to me "The editor-in-chief was recently in hospital for treatment due to a sudden health scare" and that you "are seeking advice and decisions from other academic editors."

14. On 2024-09-04 you sent me a rejection based on ridiculous "Editor Notes", which are self-contradictory by diagnosing on the one hand "Lack of Novelty" and on the other hand "contradicting established scientific consensus", by criticizing my use of "satellite data for validation", and also by invoking two typing errors for justification of the rejection. Please note that Editors of many hydrological journals in a Joint Editorial published in eight hydrological journals (see list of journals and links to the published versions of the Joint Editorial in https://www.itia.ntua.gr/1603/) clarified that a negative reply to the question "Does the paper comply with the consensus ideas on its area?" is not a reason for rejection and that, on the contrary, "a controversial attitude, provoking discussion and thought, and challenging established ideas, methods or wisdom" are "qualities of a paper should in fact favour publication".

After the above, my simple question is:

Do you feel comfortable with the way you treated my submission and with your rejection? If not, are you willing to reconsider it?

If you do not reconsider it, I will feel very bad that I trusted you. In such case, please don't send any further invitations to me. Never again! I will also notify my colleagues, as widely as I can, about my adventure, so they can judge for themselves on what principles you operate.

Kind regards,

Demetris Koutsoyiannis
National Technical University of Athens
Recent publications:
Book: Stochastics of Hydroclimatic Extremes: A Cool Look at Risk, 3rd Edition
Articles: Revisiting causality using stochastics: 1. Theory 2. Applications
On hens, eggs, temperatures and CO2: Causal links in Earth's atmosphere
Stochastic assessment of temperature – CO_2 causal relationship in climate from the Phanerozoic…
Revisiting the greenhouse effect—a hydrological perspective
Net isotopic signature of atmospheric CO_2 sources and sinks: No change since the Little Ice Age
Refined reservoir routing (RRR) and its application to atmospheric carbon dioxide balance