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

by Demetris Koutsoyiannis

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Key:

> Review comment.

Response.

> Quotation from manuscript. (Note that the numbers of the citations, which in the manuscript appear between square brackets, do not appear here; the reader can see these numbers and the related references in the manuscript).

Reviewer 1

> This is an interesting submission addressing the important role of water in the maintenance and variability of climate. As an overall comment a significant proportion of the text seems to wander and should be tightened up. A number of comments are vague, and unsubstantiated. Many of the remarks refer to well-known phenomena and are not really relevant to the arguments presented here. Paper would benefit from a clearer narrative pathway and concrete suggestions.

I am glad the Reviewer found the manuscript interesting. I appreciate his (or her) opinion that the text seems to wander. However, being an old man now, I have developed through the years a style of writing which expresses myself. Some like it (cf. Reviewer 4), some not, but my personal taste is to avoid a stereotypical stylized text. I hope the Reviewer can tolerate that. I also hope that the statement “A number of comments are vague, and unsubstantiated” is not vague per se and I thus I interpreted it is as a summary of the specific Reviewer’s comments that follow, which I have tried to address.

I appreciate and have followed the suggestion for “concrete suggestions” and I added a Conclusions section (also moving there a paragraph from the Discussion section). It reads:

> 8. Conclusions
>
> Given the hot and polarized discussions and actions about climate, it can be anticipated that many readers would find this paper useless, if not harmful. Actually, one of the aims of the paper is to show that polarization stems from political, rather than scientific, roots. Many scientists have paralleled their scientific profession with political aims (cf. "Marches for Science"). At the same time, mixing up science with politics has been promoted by many as a positive development. In contrast, this paper tries to promote the ancient ideal of science being separated from other interests, such as economic or

political. It is reminded that Plato and Aristotle clarified the meaning and the ethical value of science as the pursuit of the truth-pursuit that is not driven by political and economic interests. For the latter, they used different terms, sophist (σοφιστής) and sophistry (σοφιστεία) [30, -].

In modern politics, a fuzzy language and subjectivity may be desirable as they serve several purposes such as inclusiveness and diffusion of responsibility. In contrast, in science the desiderata are rigour, clarity and objectivity. These desiderata may attribute some usefulness to this paper in clarifying concepts related to climate and water. Arguably, there is a strong need for such clarification, if we accept that political influences should be left out.

Specifically, the current definitions of climate do not highlight its non-static nature. Rather, they imply a static climate, as already analysed (Section 3). Hopefully, the definition proposed and illustrated here (Section 4), which highlights the stochastic character of climate, could be useful to dispel this fallacy or, at least, provoke some discussion toward a more rigorous definition. By dispelling the fallacy, the term "climate change" would hopefully disappear from the scientific vocabulary and remain where it exactly belongs, i.e., the political vocabulary (Section 6). Dispelling another set of fallacies about the relationship of water and climate, also investigated here (Section 5) could be equally useful.

The potential usefulness relies on at least two facts. Highlighting the stochastic character of climate and its huge variability helps understand the failure of current deterministic modelling approaches in describing past climate and points to a potentially more promising direction in climate modelling within a stochastic framework. And highlighting the strong role of water in the climate can help shake the prevailing views on roles and causality chains in climatic processes, which may currently be opposite the real ones.

> Among some other things that struck me was that there was no mention of the ‘Anthropocene’, and the vast rigorous literature on this now (e.g., Daniel Buschmann, 2021: What is critical in the Anthropocene? A discussion of four conceptual problems from the environmental-political philosophy perspective. *Ethics and Bioethics (in Central Europe)*, **10**, 190-202, doi: 10.2478/ebce-2020-0018).

To address this comment, I have modified and expanded the last bulleted paragraph of Section 6 which now reads as follows (note that the suggested reference has been included and that this paragraph also tries to address a related comment by Reviewer 2):

" Elixir of life. The phrase "water is the elixir of life" has appeared in the 19th century in a book by Allen [ ], who attributes it to "Scriptures" and asserts that three fourths of diseases are caused by abuse of water. More recently, Eagleson [ ] used the same phrase justifying it by the facts that water is a universal solvent and that cell membranes are permeable only to dissolved substances. Thus, the biosphere depends on water, whose presence determines the type and extent of ecosystems. In turn, the ecosystems affect climate at large, through the carbon and oxygen cycles (where the vast majority of the CO_2 and O_2 emissions are products of life, through respiration and photosynthesis,

respectively), and their contribution in the water cycle (transpiration) and in the energy cycle (photosynthesis). Humans, as part of the biosphere, also interact with water and climate-they are affected by them and affect them. Since the invention of technology in the Neolithic age and, in particular, the establishment of perennial agriculture and the advent of urbanization, these human effects became larger in terms of land-use change, and contribution in the mass and energy cycles. Moreover, after the industrial revolution, the anthropogenic effects are marked and in certain aspects unsustainable. Notably, human interventions on land and on water bodies may have much more substantial effects on the entire Earth than the infamous fossil fuel burning and the resulting CO_2 emissions. For example, considering the sea level rise, the most prominent anthropogenic signal is the increased (and unsustainable) exploitation of groundwater, which transfers to the sea huge masses of water earlier stored in land [52].

To describe the growing impacts of human activities on Earth, including in geology and ecology, P. Crutzen and E. Stoermer proposed the term anthropocene for the current geological epoch [ ]. However, the proposal has not been ratified by the International Commission on Stratigraphy, nor by the International Union of Geological Sciences (note that neither of the proposers was a geologist; Crutzen was an atmospheric chemist and Stoermer a biologist). Nonetheless, the term is quite popular in other disciplines in an environmental, bioethical and political context [ ], with the latter sometimes related to "urging to action". The term has been criticised by M. Sagoff [ ], who asserts that the underlying idea that humans rule Nature "accomplishes a counter-Copernican revolution", in which "The Anthropocene makes humanity great again", hence implying that the term is equivalent to "Narcisscene" (which he uses in his article title).

> In a similar vein no mention was made that the present levels of atmospheric CO2 are highest over the last 1 Ma, and almost certainly since the Miocene, and have occurred in less than 200 years.

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

But I have doubts also for the last million years. Actually, I have perused paleo data from the Vostok ice core, covering 420 000 years (see Figure 4 of the same publication, copied below). And indeed, with a hasty assessment, as the paleo data show CO_2 concentration values below 320 ppm for the entire period, one would conclude that the current level is higher than in the entire period of 420 thousand years.

But who knows the CO_2 concentration 420 thousand years before, **at a time scale comparable to that of modern data?** Note, the time step/scale/resolution of this graph is 1000 years. Thus, each data point represents a rough average across 1000 years. If we take the average of the most recent 1000 years (rather than using annual values) we would likely find a value comparable to values of other interglacial periods.

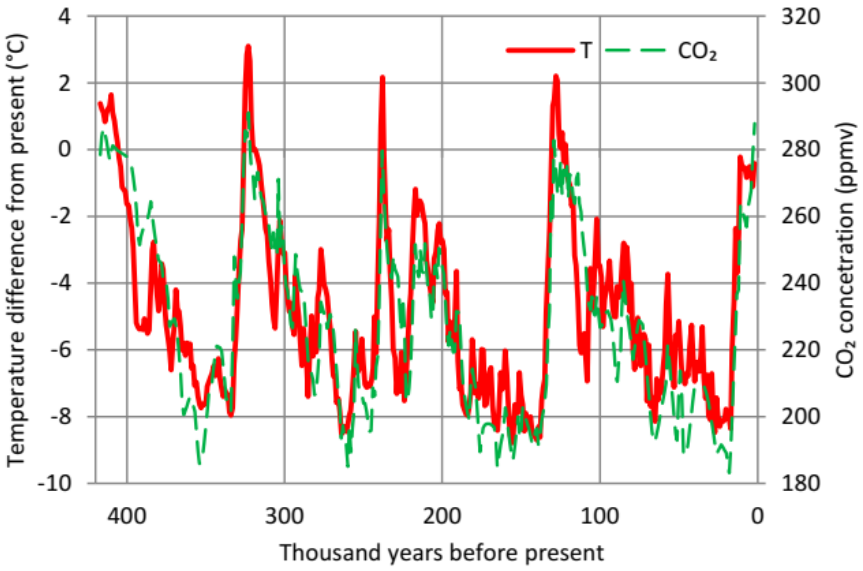

Figure 4 from Koutsoyiannis and Kundzewicz (2020).

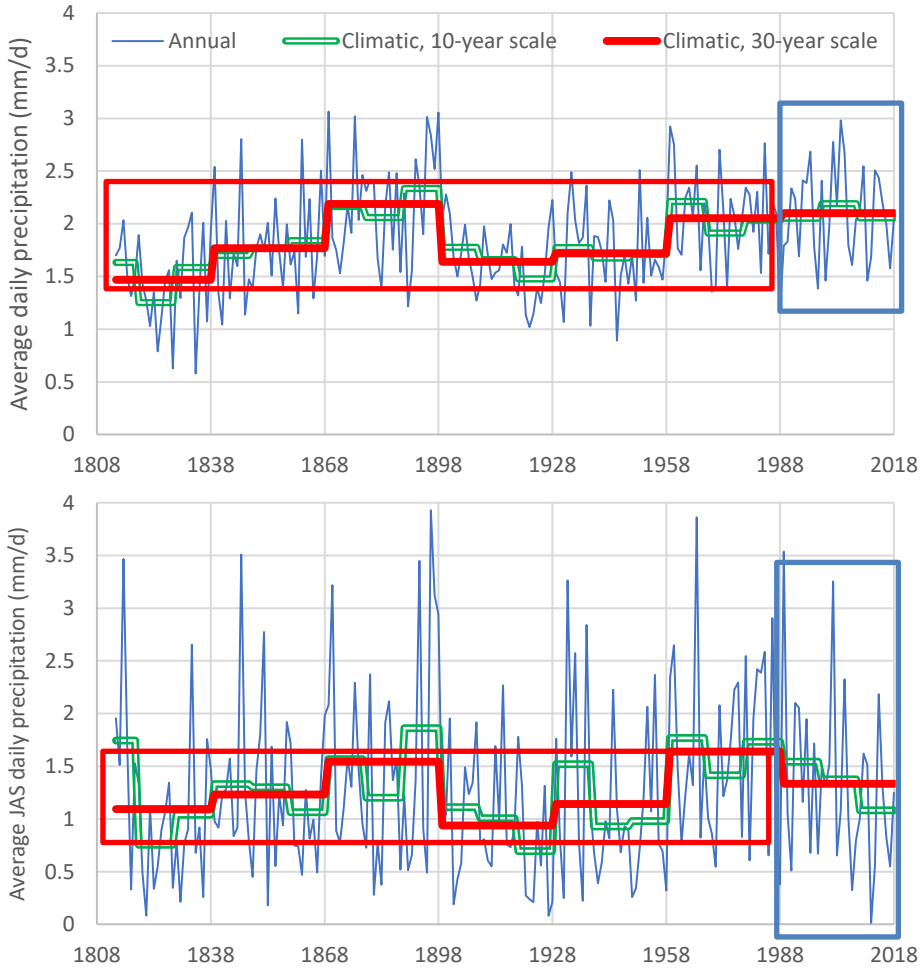

Figure 4. (upper) Evolution of average daily precipitation in Bologna, as a climatic element, seen at the annual and the climatic time scales of 10 and 30 years; (lower) as in upper panel but for a time window of the three summer months, JAS. [blue and red squares are not contained in the original figure but are added for the present illustration].

Hence, I contend that it is a big mistake to compare data of different time scales. Only data of the same time scale can be compared to each other. To make this point clearer, I now use the Figure 4 of the present paper, also copied above. Let us assume that we know the data values of the last 30 years (blue square) at an annual resolution and those of the earlier years (red square) at a 30-year resolution. If we used the mistaken "comparison" technique described above, disregarding the different time resolution, we would conclude that in the latest 30 years there are unprecedented extremes (maxima as well as minima). But this would be a blatant error.

> In addition to these I point to specific issues which must be addressed before I could recommend this manuscript for publication:
>
> Lines 445-452:
>
> Make clear that 341 W/m**2 is So / 4, and where this number come from. Also what is relevance of this statement on the seasonal cycle of albedo? If one were to argue that the multidecadal Earth's Energy Imbalance (EEI) is due to an overall reduction in albedo (surface or planetary?) some independent (solar-weighted) albedo data should be used to support the implied argument here.

I have now explained the value 341 W/m². But I am afraid I do not understand the question "where this number come from" because a citation is already there.

On the other issue, the relevance of the seasonal cycle, I am afraid there is no relevance. I just used the variability within a year for the single reason that it was not easy to find reliable data for a multiyear or multidecadal period. But the Reviewer is right, this does not provide proper means for comparison. Therefore, I have put a big of effort to analyse data from other sources to address this comment. I have added this analysis in the new Appendix E.

> Lines 454-470: I found some of the information presented in this subsection as very misleading and confusing. Firstly, the article by Gavin Schmidt (Reference 47 - Gavin A. Schmidt, Reto A. Ruedy, Ron L. Miller and Andy A. Lacis, 2010: Attribution of the present-day total greenhouse effect. Journal of Geophysical Research, 115, D20106, doi: 10.1029/2010JD014287) was published eleven years ago and could hardly be regarded as 'current'. More importantly, it has been appreciated for decades and longer that the strongest Greenhouse gas is in fact water vapor (e.g., see any basic text on atmospheric radiation or refer to any of the IPCC documents). As stressed in these studies, and indeed explicitly in Gavin's paper, 'In a doubled CO2 scenario … the magnitude of the total greenhouse effect is significantly larger than the initial radiative forcing, underscoring the importance of feedbacks from water vapor and clouds to climate sensitivity.' That is, it is these feedbacks which introduce this multiplicity. A much more rigorous statement of the evidence and the physics is required here.

Right, I have replaced "current" with "recent". And also right, that "it has been appreciated for decades" that the strongest greenhouse gas is water vapour. Therefore, I guess I am allowed to stress something that has been known for decades. The reason I am stressing that is that not all colleagues (even in the hydrological community) know it. Also, some admirers of Arrhenius may still believe in his own erroneous estimates, which rendered CO_2 as stronger than water vapour.

As regards the situation "in a doubled CO_2 scenario", my reply is that I do not know and I do not find any reason to refer to future scenarios. In fact, I strongly endorse the epistemological thesis by Percy Williams Bridgman (1966; The Way Things Are, Harvard University Press) that "a combination of words in the grammatical form of statement is only a 'pseudo-statement' when it purports to be about the future".

> Very valuable to refer in the paper to some recent studies pointing to the role of water vapor (and clouds) in warming the Arctic environment – some that should be referenced here are Lee, Feldstein et al., 2017: Revisiting the cause of the 1989-2009 Arctic surface warming using the surface energy budget: Downward infrared radiation dominates the surface fluxes. Geophys. Res. Lett., 44, 10,654–10,661,
>
> Luo, and co-authors, 2017: Atmospheric circulation patterns which promote winter Arctic sea ice decline. Env. Res. Lett., 12, 054017, doi: 10.1088/1748-9326/aa69d0,
>
> Screen, J. A., et al., 2018: Polar climate change as manifest in atmospheric circulation. Current Climate Change Reports, 4, 383-395, doi: 10.1007/s40641-018-0111-4.

I really appreciate this suggestion and have followed it by fully modifying and expanding the penultimate bulleted paragraph of Section 6 which now reads:

> " Turbulent motion. The climate is generated by the everlasting turbulent motion of two fluids, water and air. The turbulent dynamics in the circulation of both fluids is much more complex and less well known than thermodynamics [ ]. The motion of both fluids is thus inherently uncertain and produces patterns that can hardly be predicted in advance. In the atmosphere, the complexity in motion is further perplexed due to the presence of water in the air, in the form of vapour and clouds, which play a crucial role. For example, recent studies [,] point to the role of water vapour and clouds in warming the Arctic environment. Because humans (as contrasted to fish) live in contact with the atmosphere, the motion in the atmosphere is better observed and studied than in the hydrosphere. This does not mean that the motion in the latter, particularly the large-scale fluctuations, is less important for climate. For example, the rhythm of coupled ocean-atmosphere fluctuations, such as the El Niño-Southern Oscillation (ENSO), Atlantic Multidecadal Oscillation (AMO) and Interdecadal Pacific Oscillation (IPO), significantly influences the variability of global mean annual temperature [ ].

Note that the citations used (shown in the manuscript and not repeated here) are those suggested by the reviewer.

> Also, Demetris comments in this paragraph that '… the atmospheric CO2 … product of human emissions, contribute … only 3.8% to the global carbon cycle' and quotes his Moscow lecture notes [Reference 48] on this. In reality, during the Holocene the carbon budget has been very close to balanced, made up of massive alternating seasonal fluxes from the atmosphere to the ocean and biosphere, associated with the seasonality of the biology and chemistry. The relatively small amount of anthropogenic activity is the factor which disturbs this balance. Valuable to refer to the 'Keeling Curve' (https://keelingcurve.ucsd.edu/), and the vast literature related to it.

First, I wonder how the Reviewer knows what happened "in reality, during the Holocene". Second, I respectfully disagree that the carbon budget could ever be balanced or close to it. In my view, balanced system is only a dead system, because it is the imbalances, whether small or big, that produce change, i.e., alive systems.

Being a hydrologist and an engineer, I am quite familiar with storages, fluxes and ever-changing balances. Well, my experience is about the water balance, but I do not think that the reality is much more different with carbon balance. Specifically, I think we can write an equation for the carbon balance as:

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

It is often tacitly assumed that there is change of storage in the atmosphere because of the last term in the right-hand side, human emissions. But simple inspection of data shows that the left-hand side term is not equal to the last term in the right-hand side. This means that the other terms of the right-hand side are also varying in time. And if they are changing now, there is no reason to assume that they did not change in the past. In turn, there is no reason to assume that before the human emissions the left-hand side was zero.

Note that each of natural emissions and natural sinks are more than 20 times greater than human emissions, so the calculation is very sensitive for the current conditions. For the past, the proxy character of the information does not allow one to think that he can make a reliable estimate of the difference of the two variable quantities of the right-hand side, and thus know the imbalance.

That is my opinion about this issue, which however is out of the scope of my paper. For the scope of the paper it suffices to say what I have said, i.e.:

> Another misconception, common in non-experts, is that the atmospheric CO_2 is the product of human emissions, while in fact the latter contribute by only 3.8% to the global carbon cycle [48]

(By the way, I have noticed with pleasure the friendly way that the Reviewer addresses me, i.e., using my given name, Demetris. I am sorry that I reciprocate inappropriately, using "him (or her)". I hope some time in the future the scientific community become mature enough to replace anonymity with eponymity.)

> In connection with the comment related to reference 50, I could not find this paper in the journal 'Science'.

The journal is not 'Science' but 'Sci' (an MDPI journal whose title is just these three letters). But I checked the doi given and I can confirm that it is correct, so I do not understand the difficulty to locate the paper. (In any case, I have no problem if the Reviewer is just kidding, playing with 'Sci' vs. 'Science', where the former could be thought of as a poor man's Science magazine.)

> Lines 851- (Appendix C): The material presented here refers to the authors lecture notes. A much better (and refereed!) article to cite here would be …
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> Lijing Cheng, John Abraham, Kevin E. Trenberth, John Fasullo, Tim Boyer, Ricardo Locarnini, Bin Zhang, Fujiang Yu, Liying Wan, Xingrong Chen, Xiangzhou Song, Yulong Liu, Michael

> E. Mann, Franco Reseghetti, Simona Simoncelli, Viktor Gouretski, Gengxin Chen, Alexey Mishonov, Jim Reagan and Jiang Zhu, 2021: Upper ocean temperatures hit record high in 2020. Advances in Atmospheric Sciences, doi: 10.1007/s00376-021-0447-x.
>
> Also, see my earlier comment on presenting solid evidence on a reduction of albedo by this amount, rather than just suggesting it is a possibility.

I really appreciate the suggestion for this reference. I have read and cited it. Henceforth, I adapted and expanded the old Appendix C, which now became Appendix D. Still, I had to rely on my own calculations for the reasons I explain in that Appendix. In brief, my calculations agree with one of the two estimates referred to in the paper by Cheng et al., which, unless I missed something, are contradicting each other.

About the suggested "solid evidence": Still I prefer to suggest possibilities, or even just highlight the interplay of orders of magnitude, because in my view the climatic system is chaotic and complex. Pretending knowledge of the exact causes in small (im)balance quantities, resulting as differences of big quantities that change all the time, is beyond my scientific calibre.

Reviewer 2

> The article by D. Koutsoyiannis "Rethinking climate, climate change, and their relationship with water" is, quite simply, an opinion or editorial piece and most certainly not original experimental research. At that, it remains grossly incomplete given the current state of knowledge in hydrology, water resources, and their relationships with climate.

I respectfully disagree with the reviewer. Tracing the history of the development of scientific ideas and trying to clarify the scientific concepts is not expressing opinions. It is scientific research. (See also Koutsoyiannis and Mamassis, 2021; From mythology to science: the development of scientific hydrological concepts in the Greek antiquity and its relevance to modern hydrology, Hydrology and Earth System Sciences Discussions, doi: 10.5194/hess-2021-7, 2021). That said, I also believe that expressing and debating opinions on scientific issues responsibly is also an important function of the scientific community (see also below).

> First, the author uses this article for an absurd quantity of self-citations.

What is exactly a non-absurd number of self-citations? Is the reviewer implying an ethical problem? Does he (or she) imply that I citing myself in order to improve my CV metrics for my next promotion? (For his—or her—information, I am retiring next year.)

I do not have the feeling that I am using self-citations at all. Just I am building on my own work and I justify several statements by referring to older works instead of repeating everything in this paper. That is the meaning of citations—not to improve metrics. The reviewer may see further examples of such self-citations above and below.

> The translations from Greek that the author provides are vaguely interesting, but including the original Greek text is unnecessary.

I understand Reviewer's frustration but as I have justified elsewhere (Koutsoyiannis and Mamassis, 2021 mentioned above) it is important to retrieve the ancient documents in their original version and quote relevant extracts, rather than resort to what modern scholars have

said about them. I usually put original quotations in footnotes, in order not to annoy the reader, but from my experience MDPI does not like footnotes and incorporates them in the text. To avoid conversions which may result in errors, I have included them in the body of the manuscript.

> Figure 1 is superfluous.

I respectfully disagree. It would be hubristic if, writing a paper that attempts to trace the history of the notion of climate, I would not pay tribute to its fathers. I thought that in this case this is even more necessary because, from my experience, only few modern scholars know that the fathers of the notion of climate are Aristotle and Hipparchus. I thought that a minimal requirement is to show their faces in Figure 1.

> Figure 8 is misleading: Google n-gram searches include only books (not scientific journal literature), and only those books scanned and indexed by Google up to the time of the search. A better search would include Web of Science and other scientific journal databases.

I am thankful for the suggestion, which I have followed. The revised version also contains a figure panel (Figure 8, lower panel) with data from the 78 million items contained in the Scopus database of scientific articles. The new figure panel confirms the result from Google n-grams in older years (before 2000) and also expands the timeline to the newer years, where the situation has dramatically worsened.

> The author proposes to show "that water is the main element that drives climate, rather than just being affected by climate as commonly thought" [lines 43-44]. A number of physical and thermodynamic characteristics of water in all phases are quoted in Section 5. However, these do not complete such a demonstration. Climate is, in my learning and view, driven by numerous elements: solar input, axial tilt, planetary rotation, land/ocean configurations and characteristics, and atmospheric composition, including water vapor. The author addresses almost none of these in order to demonstrate their minor contribution compared with water. In fact, the author seems to render many of these as "externalities" rather than integral components of the climate system. Declaring water as the principal driver of the climate system leaves out many larger influences in a questionable approach that the author's argument fails to justify.

First off, it is not only my own definition that renders these actions as external influences on the climatic system. As quoted in Section 3, in the definition of the American Meteorological Society, these are also external influences. Specifically, according to that definition, the climate system is “The system, consisting of the atmosphere, hydrosphere, lithosphere, and biosphere, determining the earth's climate as the result of mutual interactions and responses to external influences (forcing).”

This is actually a trivially common issue in a systems approach, where we have internal interactions among the system components and external influences.

Otherwise, I have addressed all other parts of this comment. Specifically, I have added the following two paragraphs in the beginning of Section 5:

> As evident in its definition, the climatic system is subject to external influences and particularly those determining the solar radiation reaching the Earth, such as the solar

activity, the Earth's motion and the volcanic activity. Changes in the solar irradiance (which is reflected in the sunspot number and is maximum and minimum when the sunspot number is maximum and minimum, respectively), as well as in the solar and terrestrial magnetic fields, are known to influence climate [,]. It has been suggested that even the galactic cosmic ray flux may be a climate driver via solar wind modulation [,]. The oscillations of the Earth's orbit, namely variations in eccentricity, axial tilt, and precession (with the latter having been discovered by Hipparchus, as already mentioned in Section 2) are important drivers of climate and are collectively known as the Milanković cycles after the Serbian civil engineer Milutin Milanković (1879 - 1958) who studied them [-]. Recently, it has been demonstrated in a persuasive manner by Roe [ ] that it is the effect of the Milanković cycles, rather than of atmospheric CO_2 concentration, that explains the large scale climatic evolution (namely, the glaciation process) in the Quaternary.

These external drivers have changed substantially through the lifetime of Earth. According to Kuhn et al. [ ], 4 billion years ago the solar irradiance was about 80% of the current value (or, according to other estimates, 75%), the Earth's rotation rate was 170% of the current, the land area was very small, less than 4% of the current value, and the atmospheric CO_2 concentration was about 3 orders of magnitude higher or more (up to 250 000%; see this information in a combined graph in [ ]). Note that even in the Cenozoic (the last 65 million years) the atmospheric CO_2 concentration has varied by more than 2 orders of magnitude (see graph in [ ]). Amazingly however, despite these cosmogonic changes, the temperature remained fairly constant (varying by only 10%, which is equivalent to 29 K) during all these 4 billion years. For example, evidence shows existence of liquid water on Earth even in the earliest period, when the solar activity was smaller by 20-25%, a puzzle known as the faint young Sun problem [ ]. One may attribute the temperature stability to the regulating properties and processes of the climatic system and may conjecture that the hydrosphere in particular must have played some important role in it.

Substantively, and of great consequence to the author's thesis, there is no mention in the manuscript of agriculture (other than the citation of a CIA report on Russian grain production), stationarity (other than in the title of a single reference), and the relation of those to water resources management. The current fundamental touchstone reference on stationarity, by Milly et al. [2008, Science], is not included here. The societal (and somewhat scientific) concept of climate stationarity is fundamentally intertwined with the establishment of perennial agriculture and the advent of urbanization more than 6Kya. With more sedentary lifestyles and less far-ranging seasonal migration, people developed water resource management strategies that, in a feedback loop, further reinforced the entrenchment of agricultural and urban systems. This feedback loop was supported by relatively stable (stationary) climate patterns over the past several millennia, to the point that sudden changes in those patterns today are creating unexpected stresses on, and threatening the continued viability of, those systems. None of these were established as political systems, though the author prefers to stress that "climate change" (which refers to those recent sudden changes in consistency and stationarity and does not deny that climate indeed has changed before, just not as suddenly in the span of human expectation) is an inherently and purely political construct. The author's argument is fundamentally flawed,

> primarily by neglecting the wider scientific, social, and political contexts in which "climate change" is embedded at this stage in human history.

These are quite interesting observations and I would be glad to have a formal debate with the Reviewer about them. Certainly, they cannot be addressed in the framework of anonymous reviewing. I would welcome a Commentary by the Reviewer on my paper and I would be pleased to make a Reply. Alternatively, I would welcome an invitation by the Reviewer to submit a Commentary on her (or his) own publication, if she (or he) has published these ideas.

At present, in response to these comments, I have added several paragraphs, and completely rephrased some statements that existed in the original version, as I specify in the two points below.

1. In the beginning of Section 4:

> The importance of definitions is highlighted by the quotation by Luzin given in the beginning of the paper. However, this importance may not have been widely appreciated as exemplified by the popularity (almost 4 thousand citations in Google Scholar) of the paper entitled "Stationarity is dead" [ ], which does not refer to a definition of stationarity at all. Even worse, the use of the term "stationarity" in this paper is not consistent with its existing scientific definition, as thoroughly explained by Koutsoyiannis and Montanari [,]. A second example of disdaining definitions is Mandelbrot's [ ] opinion that absence of a definition "ought not create concern and steal time from useful work".
>
> Yet one may wish to adhere to the principle that definitions are a necessary element of the scientific method. In this case, one may wish to revisit the definition of climate, given the problems already examined in Section 3 and Appendix B.

The Reviewer may notice in the above quotation from the manuscript that the critique about misusing the concept of (non)stationarity is by myself and Montanari. Therefore, I had to add a couple of self-citations in the manuscript because it is us who have made that critique for the paper that he (or she) regards as “fundamental touchstone reference”.

2. In the last bulleted paragraph of Section 6 (note that this paragraph tries to address a related comment by Reviewer 1):

> " Elixir of life. The phrase "water is the elixir of life" has appeared in the 19th century in a book by Allen [ ], who attributes it to "Scriptures" and asserts that three fourths of diseases are caused by abuse of water. More recently, Eagleson [ ] used the same phrase justifying it by the facts that water is a universal solvent and that cell membranes are permeable only to dissolved substances. Thus, the biosphere depends on water, whose presence determines the type and extent of ecosystems. In turn, the ecosystems affect climate at large, through the carbon and oxygen cycles (where the vast majority of the CO_2 and O_2 emissions are products of life, through respiration and photosynthesis, respectively), and their contribution in the water cycle (transpiration) and in the energy cycle (photosynthesis). Humans, as part of the biosphere, also interact with water and climate-they are affected by them and affect them. Since the invention of technology in the Neolithic age and, in particular, the establishment of perennial agriculture and the advent of urbanization, these human effects became larger in terms of land-use change,

and contribution in the mass and energy cycles. Moreover, after the industrial revolution, the anthropogenic effects are marked and in certain aspects unsustainable. Notably, human interventions on land and on water bodies may have much more substantial effects on the entire Earth than the infamous fossil fuel burning and the resulting CO_2 emissions. For example, considering the sea level rise, the most prominent anthropogenic signal is the increased (and unsustainable) exploitation of groundwater, which transfers to the sea huge masses of water earlier stored in land [52].

To describe the growing impacts of human activities on Earth, including in geology and ecology, P. Crutzen and E. Stoermer proposed the term anthropocene for the current geological epoch [ ]. However, the proposal has not been ratified by the International Commission on Stratigraphy, nor by the International Union of Geological Sciences (note that neither of the proposers was a geologist; Crutzen was an atmospheric chemist and Stoermer a biologist). Nonetheless, the term is quite popular in other disciplines in an environmental, bioethical and political context [ ], with the latter sometimes related to "urging to action". The term has been criticised by M. Sagoff [ ], who asserts that the underlying idea that humans rule Nature "accomplishes a counter-Copernican revolution", in which "The Anthropocene makes humanity great again", hence implying that the term is equivalent to "Narcisscene" (which he uses in his article title).

> Finally, some line-by-line notes:

I am grateful for the attentive reading and for spotting all the errors below which I have corrected in the revised version. Below I provide explanations about how I addressed them.

> line 43: "show" (not "sho")
> line 100: "It comprises not only those conditions that can obviously 'near average' or 'normal' ..." (check grammar against original source)
> line 292: "it" (not "in")
> line 384: the "hot half-year" in Bologna, Italy, is in the AMJJAS period (the periods are reversed)
> line 416: "larger" (not "smaller")
> line 417: "respectively)" (not "respectively_")
> line 438: "known as" (not "or else")
> line 472: "ENSO, AMO, and IPO" should each be defined (but references are not likely necessary)
> line 507: "IPCC" (not "IPPC")
> line 528: "in its" (not "it is")

All done.

> line 553: the process was not "concluded with the establishment of the IPCC in 1988" (the process continues even today). However, one might say that the process following Kissinger's speech "culminated in the establishment of the IPCC" with efforts continuing under that institution through the present day.

I have rephrased the text as follows:

> This was followed by several actions and events by diverse American and international organizations, which, fourteen years after Kissinger's talk, resulted in the establishment of IPCC in 1988 [ ]. The efforts continue under that institution through the present day, while the political dimension of the efforts is highlighted by the accompanying activism, lately expanded to include schoolchildren.

> line 567: "in its" (not "it is")
> line 579: "In the 1975 RF report..." (not "In next year's RF report...")

Done.

> line 606: "ice caps" (not "icecaps") (confirm with original source)

I confirm that it is it is "icecaps" in the original.

> lines 627-8: this is not a complete sentence; also, what is "the Earth salvation"?

I have rephrased it as follows:

> " In 1992, the salvation of the Earth begun, as announced in the front-cover of Time's issue of 1 June: "Coming Together to Save the Earth" [ ].

> lines 641-2: "commended" or "condemned"? One would think that scientists would condemn (not commend) such "mixing up," although here the author's point might be clarified by explaining what is meant by "the climate change agenda."

I confirm that “commended” was indeed intended and the citation I give justifies it. To make it clearer, I have now rephrased it as follows:

> History teaches that mixing up science with social aspects such as politics (cf. Eugenics and Lysenkoism) or religion (cf. Giordano Bruno and Galileo) has had tragic results both for science and society. And such mixing up has been admitted with pride by scientists who are proponents of the climate change agenda [80; Sect. 2] (also cf. "March for Science").

> lines 730-1: I am certainly not a scholar of the Greek language, but I *think* the word translated as "prodigy" might be better translated in context (on the English side) as "portent" (they are synonyms but with different connotations)

I appreciate this comment, which indeed impressed me. But I prefer to leave it as is because the translation is not mine but G.C. Macaulay’s from 1890, as noted in the paper. I guess the connotations were different in 1890.

> line 799: "days" (not "dates")
> line 821: This "final quotation" is then followed by three additional quotations

To make it clearer, I have rephrased it as follows:

> A final quotation from the category of highly cited books on climate, namely from the book Physics of Climate by Peixoto and Oort [103], is quite useful:

> line 836: "... the state of the atmosphere longer timescales..." (check grammar against original source)
> line 838: "... conversely there physical processes..." (check grammar against original source)
> line 853: "or" (not "of")

I thankfully corrected those errors.

Reviewer 3

> In this study, the author includes a historical review of the notion of climate, modern definitions of climate, a new definition of climate that includes the hydrosphere and stochastics processes, an analysis of the relationship between climate and water, and finally the author argue that the term “Climate Change” is political and shouldn’t be used by the scientific community.
> I see the paper as a review of concepts instead of a novel approach. In my opinion the main topics of the paper are:
> A new definition of climate that includes the hydrosphere and stochastics processes for a range of time scales.

I appreciate the Reviewer’s effort to provide this summary of my work in his own interpretation.

> However the relationship between hydrosphere and climatology is well known. In the same way, the climate as a stochastic process has been studied before.

I am confident that what I present, which is not necessarily identical to what the Reviewer has interpreted, is new. I had highlighted that even in the original manuscript (last paragraph of the Introduction) in this way:

> While the subject of this paper looks general and its content perhaps trivial, the investigations performed, the information given and the synthesis thereof are mostly new.

Besides, the Reviewer does not justify his phrase “has been studied before” by providing specific references.

> The suggestion that the term “climate change” disappear from the scientific vocabulary.

Right, I suggest that.

> The author argues that climate is always changing…But it is changing in the same manner? Should we use the term “Change in the climate change”?

No, there is no such need in a stochastic framework. Change in change can also be dealt with using stochastic processes.

> I think the term “Climate change” is necessary in science and politics.

Of course, I do not dispute the Reviewer’s right to have this opinion. If he or she wishes to express this opinion formally, I would welcome any type of formal debate he (or she) wishes.

> In the last decades the observed changes in climate are unprecedented and the expected changes are worrying.

I think that the phrase “expected changes are worrying” is not a scientific statement but a political or ideological one. As I state in the paper:

> Assuming that we have democracy, and freedom of opinion and speech, agreement or disagreement with any political agenda is any citizen's inalienable right. On the other hand, political agendas do not belong to the domain of science.

Being a citizen myself, I also have worries of political and ideological type. However, my worries are not at all for the climate, but for the political agendas hidden behind “climate change” and their impact on democracy. Again, these worries do not belong to the domain of science and hence I have tried to keep them out of the paper. At least, I have tried to present facts documented by citations, rather than opinions.

I wish to add this: More than 2300 years ago, Epicurus pronounced science as the enemy of fear and of superstition. It would be a pity if modern science were used to cultivate fear or worry.

Reviewer 4

> I must admit that I cannot remember when and where I have read such interesting and quality paper. Very original, I must say!

I am delighted and grateful for the very positive assessment of the paper.

> Despite this, I am proposing a major revision. Only one thing is missing. This is a comparison with other locations in case study, i.e. Bologna case. If authors will do this, I will not have nothing to say, and this will be one of my "soft & easy" reviews.

The purpose of the Bologna case study is the illustration of the definition, not the comparison of different cases or the extraction of general conclusions. More case studies are contained in my recent book (Koutsoyiannis, 2020; Stochastics of Hydroclimatic Extremes - A Cool Look at Risk, 330 pages, Edition 0, National Technical University of Athens, Athens, 2020; http://www.itia.ntua.gr/2000/). Nonetheless, after the Reviewer’s suggestion, I have added the new Appendix C with two more case studies, in order to illustrate the framework in other climatic variables with different behaviours. The new Appendix (more than 3 pages long, including 3 figures) is referenced to in the end of section 4 as follows:

> The effect and the modelling of seasonality are shown in Appendix C using for illustration different variables, the maximum and minimum daily temperature, and as study cases different sites, Vienna (Austria) and Melbourne (Australia), which again are among those with the longest time series for these particular variables.