Post-production review on

Title: The temperature–CO2 climate connection:
an epistemological reappraisal of ice-core messages
Author: P. Richet
Hist. Geo Space Sci., 12, 97–110, 2021 https://doi.org/10.5194/hgss-12-97-2021

This paper analyses temperature, CO2 and CH4 from the Vostok ice core over the last 423 kyr. It comes to some conclusions on cause and effect and argues that there is only a minor feedback from CO2 on temperature, from which the author concludes: „If not refuted, the demonstration indicates that the greenhouse effect of CO2 on 20th century and today's climate remains to be documented".

This is a very strong statement. Unfortunately, it is wrong in many ways, which I try to enroll in the following. In other words, the following is the requested refutation. Briefly, in a few words: Just looking at time series of CO2 and temperature over a few glacial cycles (and their leads/lags and correlations) is a much too simple approch to understand the natural carbon cycle and fails for principle reasons to say anything about the anthropogenic (recent) global warming.

In conclusions:

- (1) One needs to understand the difference between the natural and the anthropogenic C cycle.
- (2) The present day (anthropogenic) emissions lead to the measured CO2 rise, whose impact on Earth's energy imbalance is now also measurable, and not only a theoretical product of models.
- The basic chain of argument in the paper of Richat (2020) is either wrong (see #1), or has been refuted in the mean time (see #2).
- In my view this paper should be withdrawn due to major flaws.

In detail:

- What is seen in the ice cores is the imprint of the natural carbon cycle on glacial/interglacial timescales and its relation with temperature (briefly in the following as natural C cycle). The changes in CO2 and temperature we see since the begining of the industrialization is the perturbation of the carbon cycle by the addition of fossil fuel emission into the natural C cycle. This anthropogenic perturbations leads to a change of cause and effect. It is correctly noted in the paper, that the feedback between temperature and CO2 is first of all the solubility effect (a warming ocean stores less CO2, thus warming leads to oceanic outgassing of CO2 and a rise in atmospheric

CO2, and cooling to ocanic CO2 uptake and a decline in atmospheric CO2), but also keep in mind that it is nowadays well established, that glacial/interglacial changes in atmospheric CO2 cannot be explained by one single process (e.g. Brovkin et al., 2007, doi: 10.1029/2006PA001380; Kohfeld and Ridgwell, 2009, doi: 10.1029/2008GM000845). Significant change in atmospheric CO2 on glacial/interglacial timescales is expected from a rise in sea surface temperature, rising sea-level, reduced marine export production, and responses from carbonate compensation, together with changing land carbon storage (Kohfeld and Ridgwell, 2009). Although models still disagree on the contribution of individual processes, the common consensus is, that the glacial/interglacial rise in temperature (more precisely sea surface temperature) might be responsible for a rise in atmospheric CO2 mixing ratio of 20–30 ppm. On glacial/interglacial timescales the total amount of carbon contained in total in atmosphere+ocean+biosphere (land+ocean)+mixed layer of the marine sediments stays roughly constant. The additional anthropogenic CO2 emission into the atmosphere now changes this total sum of carbon and leads to a gradient in CO2 FROM atmosphere TO the ocean. Thus, nowadays the ocean takes up CO2 from the atmosphere because atmospheric concentration rose due to fossil fuel emissions. In other words, the ocean is slave to the atmosphere. This is the opposite of the natural C cycle, in which the atmosphere is roughly speaken the gas phase of the ocean, thus a slave to the ocean. The amount of CO2 caused by anthropogenic emissions that stays in the atmosphere is a function of time and emission size. In the worst case (all known fossil fuels are emitted) it might take 1 million years to come back to natural CO2 below 300 ppm, since only the long-term processes of continential weathering are able to reduce the last part of the emissions (Lord et al., 2016, doi: 10.1002/2014GB005074; Köhler 2020, doi: 10.3389/fclim.2020.575744).

– The radiative forcing or greenhouse effect of CO2 and of other atmospheric gases lead to an energy imbalance of the Earth. As consequence Earth's temperature rises (and in the mean time the excess heat is mainly stored in the ocean, von Schuckmann et al. 2020; doi: 10.5194/essd-12-2013-2020), and more infrared radition is emitted until a new equilibrium is reached (see Hansen et al. 2011, doi: 10.5194/acp-11-13421-2011). As consequence heat The greenhouse effect is typically calculated with models. However, it has been directly measured in the meantime by Feldman et al (2015, doi: 10.1038/nature14240) stating: „These results confirm theoretical predictions of the atmospheric greenhouse effect due to anthropogenic emissions, and provide empirical evidence of how rising CO2 levels, mediated by temporal variations due to photosynthesis and respiration, are affecting the surface energy balance".

– The direct effect of CO2 on Earth's temperature on glacial/interglacial timescales has been estimated by Köhler et al 2010, doi: 10.1016/

j.quascirev.2009.09.026). The „pure“ first order effect (only considering the Planck feedback,) for CO2 reduction from 278 to about 190 ppm during the Last Glacial Maximum (LGM) is about 0.6°C, or about 10% of the global cooling of about 6°C (Tierney et al 2020, doi: 10.1038/s41586-020-2617-x). All other temperature change has to be explained by fast and slow feedbacks in the climate system.

- The climate model in the group of Jim Hansen was one of the first that proposed a significant global warming as response to the ongoing rise in atmospheric CO2. In the meantime enough time has past and it is possible to check how good/bad the proposed model-based warming has been. To my knowledge this comparison is only available on realclimate.org (run by Gavin Schmidt (NASA-GISS)) and not in peer-reviewed journals https://www.realclimate.org/index.php/archives/2018/06/30-years-after-hansens-testimony/, but it is an overwhelming comparison concluding that Hansen's proposed warming was right. This is the more remarkable considering the state of climate models in the 1980ies. There is a more general comparison of model projections against observations some decades later in Hausfather et al (2020, doi: 10.1029/2019GL085378).

- Other statement from the abstract:
  - „...do not rely on any independent evidence for the influence of greenhouse gases on climate over long enough periods of time.„ I believe the arguments above give these independent evidence.
  - „Similarly, the constant 7 kyr time lag between temperature and CO2 decreases following deglaciation is another important feature that needs to be understood.„ The time lag between temperature decrease and CO2 decrease is one specific aspect of the natural carbon cycle, which is subject to recent research (e.g. Brovkin et al., 2016; doi: 10.1016/j.quascirev.2016.01.028; Köhler et al 2018: doi: 10.1029/2018GL077717). Saying this, this open discussion is no argument against CO2 being responsible for recent global warming, since this is a problem of the natural C cycle, not of the anthropogenic effect (see points above on this misconcept of the whole paper).

- Other issues:
  - Section 2. background: ... „Hence, the first goal of this study is to analyze the nature of these relationships to ascertain whether or not CO2 is the real climate driver of the Earth.“ This is a complete misconcept of the understanding of how CO2 influences temperature and vice visa by mixing natural and anthropogenic C cycle effect. Until

this is not understood any further discussions are meaningless.

- Humlum et al 2013 cited here in support for an atmospheric CO2 adjusting itself to temperature changes for 1980-2011 has been disputed heavily by Richardson 2013 (doi: 10.1016/j.gloplacha.2013.03.011)

- Section 5: „In marked contrast, a lack of correlation resolutely rules out any causality.„ This is also thought too simple, see, e.g. Causal feedbacks in climate change in van nes et al 2015 (doi: 10.1038/NCLIMATE2568)

- Section 5: „A second factor is the fact that the temperature variations of up to 12 ◦C during climate cycles observed in polar regions are much greater than the 2–3 ◦C that affected the entire Earth,“. The present knowledge of LGM cooling is not -2-3 °C, but about -6 K (Tierney et al 2020, doi: 10.1038/s41586-020-2617-x)

- CH4 changes are smaller than that of CO2, but their global warming potential is 20-100 larger (depending on the time window of interest), a fact which has been completely ignored here (see chapter 8 in IPCC 2013).

Review of Richet, 2021: "The temperature-CO_2 climate connection: an epistemological reappraisal of ice-core messages" June 28, 2021

This paper argues that past variations in atmospheric carbon dioxide concentration, as revealed by ice core records of the past atmosphere, cannot have caused the planetary warmings and coolings known as ice age cycles. The main evidence cited is that the Antarctic air temperature is observed to change *prior to the carbon dioxide changes* by several hundred to several thousand years. In turn, the author argues that the current rise in atmospheric carbon dioxide concentration due to human activities therefore cannot be the cause of the current warming observed in numerous temperature records from all over Earth.

These conclusions are deeply flawed for several reasons. First and foremost, the author implicitly commits the logical error that "temperature variation on Earth can have only one cause". It is well known to the scientific community that multiple causes of temperature variation were at work in the ice age cycles (e.g. Caillon et al., 2003; He et al., 2013), including the Earth's orbit (Milankovich forcing), greenhouse gas concentrations (CO_2, CH_4, N_2O, tropospheric O_3), and variations in the overturning circulation of the ocean that redistributed heat from one part of the planet to another (Crowley, 1992; Stocker and Johnsen, 2003).

Therefore, the author's contention that the *longer duration* of high-CO_2 conditions than the duration of high Antarctic temperatures, somehow proves that CO_2 does not cause temperature change, is simply wrong. There are at least two other candidate mechanisms, orbital forcing and ocean circulation, that could have caused the observed short-duration interglacial warmth in Antarctica. Indeed, it is a logical fallacy to test the hypothesis that CO_2 causes climate change by asking whether temperature and CO_2 time series are correlated, in a system with *three* known climate forcing mechanisms.

Furthermore, the author commits the error of assuming that Antarctic temperature changes are representative of, and synchronous with, temperature changes on the rest of the globe. In fact, it is well known that temperatures are highly heterogeneous across the globe at any one point in time (WAIS Divide Project Members, 2015), precluding the use of a record from Antarctica as a global thermometer.

In summary, this paper should have never gotten past peer review. It appears that quality peer review was not done. It is inherently flawed and cannot be remedied by further revision. It must be withdrawn.

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Caillon, N., Severinghaus, J.P., Jouzel, J., Barnola, J.-M., Kang, J., and Lipenkov, V.Y. (2003). Timing of atmospheric CO_2 and Antarctic temperature changes across Termination III, *Science* **299**, 1728-1731.

He, F., J. D. Shakun, P. U. Clark, A. E. Carlson, Z. Liu, B. L. Otto-Bliesner & J. E. Kutzbach (2013). Northern Hemisphere forcing of Southern Hemisphere climate during the last deglaciation, *Nature* **494**, 81 – 85.

Crowley, T. J. (1992). North Atlantic Deep Water cools the southern hemisphere. Paleoceanography **7**, 489–497.

Stocker, T. F. & Johnsen, S. J. (2003). A minimum thermodynamic model for the bipolar seesaw. Paleoceanography **18**, 1087.

WAIS Divide Project Members (2015). Precise interpolar phasing of abrupt climate change during the last ice age *Nature* **520**, 661-665.

Recommendation: Reject. The manuscript does not present novel analyses or advancements that are both substantive and correct, nor does it provide an accurate review or perspective.

Synopsis: The only "analysis" in the manuscript is that the author:

1) drew horizontal bars at levels that the author interprets as the half-power points of some of the peaks of Figure 1, which shows previously published data on the histories of atmospheric CO2 concentration and the estimated temperature at the deposition site of the ice in the Vostok ice core;
2) noted that, as drawn, the bars on the CO2 curve are longer, indicating greater duration, than on the temperature curve;
3) concluded that this invalidates the idea that rising atmospheric carbon dioxide concentration has a warming influence on temperature, for ice age cycling and for all other variations.

As noted below, the scientific understanding of "global warming" by itself does not lead to a specific prediction about the relative widths of these horizontal bars, so one cannot test that scientific understanding in this manner. This invalidates the manuscript.

Many other aspects of the manuscript are also wrong or misleading.

Additional Review Comments:

Starting from the beginning, the manuscript title includes "epistemological reappraisal", but the manuscript lacks substantive epistemology. As one example, the Abstract says "*The epistemological weakness of current simulations originates from the fact that they do not rely on any independent evidence for the influence of greenhouse gases on climate over long enough periods of time*." Use of "simulations" is odd here, as paleoclimatic data are not primarily used in simulations. (Models can assimilate paleoclimatic data, but doing so over long times is still a very new field. Paleoclimatic data are used in numerous, important ways that are not described in the manuscript; Chapter 6 of Working Group I of the IPCC Fourth Assessment Report is a good starting point.) Assuming that this statement in the manuscript really refers to the broader field and not just to simulations, the manuscript fails to provide cogent analysis demonstrating why evidence is required "*over long enough periods of time*", or how long is long enough. Note that the evidence for a warming influence from rising atmospheric CO2 concentration did not start with correlations between CO2 and temperature over time, but instead with understanding of the controls on Earth's energy balance and of radiative transfer involving the major and trace gases in Earth's atmosphere (a fact that is partially noted elsewhere in the manuscript). Furthermore, contrary to the statement in the manuscript, evidence of influence of greenhouse gases on climate does extend over longer periods of time, and is in many ways easier to interpret over those longer times than over glacial-interglacial cycles (e.g., the Paleocene Eocene Thermal Maximum, mid-Cretaceous warmth, the end-

Permian extinction, and others further back in time), but the manuscript fails to consider any of that large body of evidence. I believe that the community of scientists studying climate change would welcome additional work on the epistemology of the field, but the current manuscript does not provide such scholarship.

The journal addresses the history of science, but the manuscript does not present newly discovered historical results, nor any novel analysis of previously presented results, nor any serious review or synthesis, and the little history included in the manuscript is incomplete or inaccurate. For example, in the first paragraph, "*Among innumerable examples… the central position of the Earth in the universe … remained uncontested for 2 millennia in spite of shortcomings that were pointed out early on*." The history is fascinating of how geocentrism became generally accepted and was then made useful in at least some ways, was challenged by some Persian and Arab astronomers, and was eventually replaced. The "uncontested" claim, though, is far too broad for a historical work. Similarly, at the start of section 2, the manuscript states "*Real attention to the greenhouse effect of atmospheric carbon dioxide is not at all recent, since it had already been drawn by Fourier (1827), Tyndall (1861) and especially Arrhenius (1896)*." While Fourier did correctly identify what we now call the "greenhouse effect", he did not know about the role of carbon dioxide. Other "historical" statements in the manuscript also are inadequate.

The main thrust of the manuscript appears to be the erroneous claim that differences in detail between the history of stable isotopic ratios of the ice (hence, with certain qualifications, temperature at the site of deposition of the ice) and atmospheric carbon dioxide concentration, as archived in the Vostok ice core, are sufficient to invalidate the idea that carbon dioxide released by fossil-fuel burning is driving globally averaged warming, or more broadly that rising carbon dioxide concentration in the atmosphere has a warming influence on the climate.

Start with this statement in section 2:

"*Thanks to this specific focus, a rigorous assessment of the ice-core data can rely on pure logic and, thus, on the fundamentals of scientific reasoning as early laid down by Aristotle in the 4th century BCE (Before Common Era). The conclusions derived should, thus, be particularly robust because they are directly drawn from the ice record itself. As such, they are not restricted to a specific geographical setting, and they depend neither on any physical mechanisms assumed to be at work in the complex Sun–Earth system nor on any particular climate models and values of their respective input parameters.*"

If an analysis relies on data with potentially complex transfer functions, then the analysis does not depend on "*pure logic*". The temperature history calculated from the isotopic ratios of water in the ice core is generally interpreted as the local temperature at the site of snow deposition, which, for the Vostok core, is not too far from the core site. This is unequivocally "*restricted to a specific geographical setting*", contrary to the premise of the manuscript. (Perhaps more precisely, the isotopic ratio is primarily controlled by the temperature difference between the moisture source and the depositional site, with appropriate corrections for

seasonality of precipitation and other issues. Increasingly, time-evolving calibrations are derived from other indicators because of the complexity of the isotopic indicator.) The carbon dioxide record is close to being a global signal, as the residence time of carbon dioxide in the atmosphere is long enough that mixing largely removes the effects of local sources or sinks.

The local temperature does depend on the strength of the greenhouse effect, but also on numerous other factors, including status of modes of variability (e.g., ENSO, SAM), the sun-blocking effects of volcanic particles, the sun's output, the orbital parameters, etc. Antarctic temperatures are strongly influenced by changes in the Southern Ocean, which during colder times have been strongly tied to changes in the Atlantic Meridional Overturning Circulation on millennial timescales. Many of these other climatic influences can affect globally averaged surface temperature, and also affect local and regional temperature with strong spatial patterns, so the effects often differ greatly from the effects of well mixed greenhouse gas concentrations. In turn, this means that while changes in globally mixed greenhouse gas concentrations are indeed expected to affect local temperature, the local temperature is never expected to be solely a record of greenhouse gas changes. In turn, deviations between a local temperature record and the global carbon dioxide record are insufficient to invalidate the idea that increasing carbon dioxide concentration has a warming influence. (There certainly are ways to test this "global warming" hypothesis using paleoclimatic data; additional information is provided below with pointers to studies that validate the hypothesis.)

The manuscript contains numerous other incomplete or mistaken statements. For example, the manuscript states "*Even though the acid base properties of CO2-bearing aqueous solutions and the biological role of carbonate and bicarbonate ions make the picture difficult to unravel quantitatively (see Michard, 2008), temperature rises cause an overall decrease in the CO2 solubility in the ocean and, correlatively, an increasing concentration of atmospheric CO2.*" The physical solubility of carbon dioxide and other gases in the ocean does depend on temperature, and warming does tend to shift gases from the water to the atmosphere, as stated; however, warming also melts ice, which reduces ocean salinity, which increases the solubility of gases in the ocean, by an amount that partially offsets the temperature effect. The changes in atmospheric carbon dioxide concentration from physical changes in ocean solubility are in total much too small to explain the amplitude of the glacial-interglacial changes recorded in the ice cores. There is a rich literature on this; Sigman and Boyle, 2000, in *Nature* is one starting point. The main controls on carbon dioxide storage in the ocean are not addressed in a substantive way in the manuscript but are extensively addressed in the relevant scientific literature; these controls are not nearly as directly tied to atmospheric temperature as the author implies, although the orbitally paced ice-age cycles do indeed shift carbon dioxide between the atmosphere and the deep ocean and other reservoirs.

The author chooses to use "full width at half maximum" to characterize the variations in temperature and carbon dioxide from the Vostok core. This is a recognized technique. It is surely not a unique way to study such records, nor is it generally preferred. No justification is given why this particular technique is necessary and sufficient. The records studied here involve strongly nonsinusoidal variations. The records all return to higher values than the half-

power level as chosen by the author after dropping below it before the next peak analyzed by the author, but the author does not even consider these other peaks (or address whether they are parts of the main peak). Looking at only the first drop, and not the subsequent rises and drops, the author states "*The fact that the peak widths are systematically larger for CO2 than for temperature thus implies that the variations in CO2 concentrations were driven by temperature changes throughout all cycles and not only at their onsets.*" This is illogical. The general similarity between the temperature and CO2 records is much greater than could occur by chance, and clearly demonstrates interesting behavior, but both could be driven by some other factor, or one or both could reflect partial driving by the other and partial driving by additional forcings; many suites of processes could produce such behavior, and the statement by the author is wrong. The similarity of the temperature and CO2 curves motivates research, but surely does not demonstrate causation, or lack thereof. The published scientific literature does address causation, as noted below, but extensive, essential contributions have been ignored in the manuscript.

The errors or shortcomings of the manuscript chosen for discussion in this review are illustrative rather than comprehensive; many more are present. There is no body of accurate analysis in the manuscript, whether on epistemology, or history, or science, that merits revision and resubmission. If the author wishes to conduct research on the influence of changing CO2 on temperatures during ice-age cycles, he might start with Tierney et al., 2020, Nature 584, 569-573. Additional information on non-CO2 influences on temperature is available in many sources; a useful starting point is P.U. Clark et al., 2002, Nature, 415, 863-869. A better analysis of the timing of temperature and CO2 changes is given in Shakun et al., 2012, Nature, 484, 49-54. The author might also look more broadly at the role of paleoclimatic data in climate change science; Chapter 6 of Working Group 1 of the Fourth Assessment Report of the IPCC may prove useful. The relevant literature extends far beyond these few examples.

Review of "The temperature–CO2 climate connection: an epistemological reappraisal of ice-core messages" by Pascal Richet

First, I must confess I have probably never read a "scientific" paper of such low quality, and I am appalled that it was published in an international scientific journal. The paper looks more like a strongly biased political indictment against climate science, using a plethora of historical impossibilities and without any knowledge of the most basic concepts in climatology. According to usual scientific standards, this paper should certainly not have been published and I suspect there was no climate scientist in the former review process.

Major misunderstandings:

1 - On the science of greenhouse gases.

The author seems to believe that the pCO_2 measured ice cores during glacial cycles is:

"the most compelling direct evidence for the climate effects of greenhouse gases".

He states that these measurements are:

"*a Rosetta stone of climatology*";

that:

"*current simulations… do not rely on any independent evidence*".

This is certainly not true since the science of greenhouse gases was already well established well before the ice core results. Already in the 1970s, it was very clear for meteorologists and specialists of atmospheric radiative transfers, that the increasing CO_2 or CH_4 concentrations will have obvious visible impacts, in terms of increasing temperatures, in the following decades. This prediction turned out to be true, since the world's temperature is now about 1.2°C above pre-industrial levels, just as anticipated almost fifty years ago. There is certainly no need of a "Rosetta stone" from Antarctic ice cores to ascertain such results. This was already made very clear in the Charney report (1979), which was presenting the consensus view from climate scientists at that time. The Vostok record was published 20 years later. This had little or no impact on the scientific question of global warming, which was then already well established.

Page 105, the author cites Petit (2013) as a "proof" of the central role of the Vostok results on the "global warming debate". This reference appears to be an interview of Petit (by the way not written by Petit) in the local Grenoble city news website. I am not sure this stands as a strong solid demonstration of the "central role of ice cores in the scientific debate" of global warming.

2 - On Milankovitch theory

The author is an Earth scientist. It is therefore a pity that he has such wrong misconceptions on classical Milankovitch theory. In particular, it is strange that he plots the summer insolation at 65°N together with Antarctic records, on Figure 1, without even discussing the possible link between to two. Glacial and interglacial are primarily about ice-sheets, not about Antarctic temperature and apparently, the author has difficulties in connecting the astronomical forcing with the Vostok record according to standard Milankovitch's theory.

"astronomical control of glacial cycles… exerted through variations in the energy received by the Earth"

If the author means the total amount of energy received by the Earth, then he has fully misunderstood the astronomical forcing, since it is almost constant in global average. In other words, the astronomical forcing, in the Milankovitch theory, does not affect (directly) global temperatures.

"Acting also first on temperature and ice volume, the opposite changes take place when the Earth's net radiation energy budget becomes negative"

How could the net budget change through time if the global incoming forcing does not? There is clearly a causality problem in this reasoning. In contrast, the astronomical theory states that it is the local seasonal insolation forcing, in summer at high northern latitudes that affect ice sheets (but certainly not global climate). Several millennia later, which is the time necessary for the ice to grow or melt, global climate can then be altered by the changing size of the ice sheets, and its effect on albedo and more generally on the atmospheric temperatures.

"temperature changes were, thus, necessarily the triggering causes of all episodes…"

"climate changes and the ensuing sea level variations"

Again this contradicts Milankovitch theory, which states that it is ice-sheet changes that are triggering (global) temperature changes, not the reverse. This should therefore read *"ice-sheet and sea level changes and the ensuing climate changes".*

This is indeed precisely why it is usual to use the insolation at 65°N in summer, as a simple way to represent the forcing on ice-sheets. It is obviously not a forcing for global climate, and even less for Antarctic climate.

The author therefore builds up a whole discussion on ice ages, its causes and consequences, but has obviously misunderstood the very beginning of the standard astronomical theory: global climate changes are a consequence of ice sheet changes, since they cannot be a cause, at least in the astronomical framework.

"… one could alternatively claim that other factors than CO2-temperature interactions are involved"

Indeed… May be ice sheet changes could have some role during ice age cycles? But this idea does not seem to appeal to the author:

"… one can neglect climate variations caused by factors such as changes in solar activity and… differences between freezing and melting dynamics of ice sheets…"

Overall, it is simply not acceptable for an Earth scientist to discuss the dynamics (causes and consequences) of ice ages and simply neglecting the central phenomenon, which is obviously the growing and melting of ice sheets. I must admit this is probably the first paper I read of ice ages that simply "neglects" ice sheet changes. The Milankovitch's astronomical theory is fundamentally a theory of ice sheets, not a theory of climate. The author has obviously missed this basic point.

3 - On time lags

Focussing the discussion on lags between Antarctic temperatures and CO_2 as little relevance for the question of causes of ice ages. Indeed, if (global or Antarctic) temperature changes are mainly caused by ice sheet changes, according to mainstream astronomical theory, then the author should look at lags between ice sheet changes and CO_2 in order to decide if CO_2 has some responsibility in these ice age cycles. If CO_2 is a

cause, then it should lead ice changes. If it is a consequence, it should lag. The only mention to this key data is in the following sentence:

> *"… the constantly lagging effects of CO_2 concentrations with respect to the rate of change of the global ice.. (Roe, 2006)"*

But this is simply not true. The Roe (2006) paper only looks at the mean averaged lag. The author therefore cannot reasonably state that CO_2 is "constantly" lagging, something which is well known to be incorrect. Indeed, it turns out that CO_2 leads during deglaciations and lags during glacial inceptions. The truth might therefore be slightly more complex than what the author suggests: maybe it is not simply caused by *"astronomy or CO_2"* in an exclusive manner, but it could possibly be *"astronomy and CO_2"*, as clearly suggested by the changing lead-lag relationships.

Antarctic temperature depends both on global climate (and therefore on ice sheet change), but also on CO_2, and certainly also on many other factors. It is therefore quite natural that Antarctic temperatures are cooling during glacial inception, when ice sheets are growing, even if CO_2 remains high. Indeed, it is reasonable to think that we might need at least two mechanisms (ice sheets and CO_2) to account for global climatic changes in a conceptual framework. This is also, by the way, what climate models predict, with an approximate respective role of about 50%. The request of the author to explain temperature changes in Antarctica during ice ages with a single mechanism is obviously over simplistic. In any case, it stands in full contradiction with observations and it does not reflect current scientific knowledge on the subject.

4 - On the historical account of ice age theories

Indeed, the author appears to be locked in the early 20th century debate between promoters of the astronomical theory against defenders of the CO_2 induced ice age cycles. This appears very anachronistic and does not fairly represents our current knowledge of the problem. The author states:

> *"the control of ice ages exerted by variations in the Earth's insolation was… firmly established" (Hays et al. 1976)*

but "control" is certainly much too strong a word which does not reflect what Hays et al. are stating in their 1976 landmark paper "variations in Earth's orbit, pacemaker of ice ages". If the pacing of ice ages is indeed astronomical, Hays et al. are in contrast extremely clear concerning the lack of a direct physical link between eccentricity and glacial interglacial cycles: They write that "the 100,000 climate cycle is driven in some way by changes in orbital eccentricity… we abandon the assumption of linearity".

Since the 1970s, there have been many suggestions to provide some physical mechanism between eccentricity and the large ice age cycles, some of them being restricted to ice sheet dynamics while others are involving the carbon cycle. In any case, it is plainly wrong to present the recent developments in the understanding of ice ages as a debate between astronomical and CO_2, since we now understand that both mechanisms are obviously playing a role.

5 - On climate models

> *"Current climate models are... not open to falsifiability in Popper's sense…"*

I do not understand why, since these models made predictions about 40-50 years ago that have been fully verified today: they predicted a global warming of about +1°C at the beginning of the 21st century, something which is now well measured, not only by

climatologists, but which is also now visible to the layman. A model making a prediction is easily falsifiable: either the prediction is right or wrong. This is indeed the core of Popper's argument and climate models do obviously follow the standard scientific pattern in this respect.

> *"… any claim that models accurately reproduce the reported climate evolution since the late 20th century would rather illustrate their spurious nature and not prove their validity if the temperature rises of this period are not caused by increases in CO_2 concentrations".*

A more correct statement, following Popper's methodology, would be to state that climate models are now proven scientifically correct, until there are proven wrong. It is clearly dishonest to call "spurious" the physical phenomena that are based on sound physics and thermodynamics, that have been observed in great details for many decades and that do deliver valid predictions. In Popper's sense, the climate models have therefore been fully validated, until someone finds a better theory, which is clearly not the case. I certainly would like the author to provide an alternative model and explain what could cause the observed temperature rise, if not CO_2. Though many fancy suggestions have been put forward by climate skeptics, none have yet passed the very first test of physical consistency.

> *"It would be in fact an arbitrary assumption to posit that a system as chaotic and as highly heterogeneous as the Earth… would always evolve in a smooth manner"*

This certainly appears to contradict the rest of the manuscript, which attempts to provide an over simplistic explanation of ice ages in terms of astronomy but even excluding the possibility that CO_2 could have a role, since the author wants to have a single simplistic mechanism. More importantly, it seems to me that the author is mixing "climate" and "meteorology" since there is no indication that the climate system is "chaotic" in the mathematical sense, in particular over the ice age timescales. Again many scientific publications are discussing this point. On the contrary the ice age problem appears quite deterministic, therefore allowing (for instance) the formulation of an astronomical theory.

Other major comments

> *"the synchonicity of the episodes of warming and cooling… between Greenland and Antarctica… »*

I do not understand this statement, since it is well established that Greenland and Antarctica are NOT synchronous. This is precisely the subject of a vast literature in climate sciences over the last 30 years. Either the author has missed these previous studies on ice core, something difficult to imagine since they are mentioned in many papers. Or the author tries to fit the data to his own simplistic pre-conceptions of the system, that past temperature changes are "global" (they are not) and that the Northern hemisphere and the southern ones have a similar pattern (they have not) forced by a single global insolation (which is known since the 19th century to have no climate impact). In any case, this sentence alone is not acceptable in a serious scientific paper about ice cores, since it contradicts decades of observational data.

I really do not understand why the author is using the Vostok ice core with only four glacial cycles while the Dome C record is twice as long. The reasons given by the author are certainly not satisfying:

"Possibly because of perturbations… the older material exhibits warming-cooling episodes in the form of broader features"

"there is no reason why temperature spikes would have been less frequent or less intense in the most ancient than in the most recent part…. The temperature was progressively smoothed out back in time"

There are many ways to ascertain that the isotopic record was not significantly smoothed in the earlier part of the Dome C record. The fact is that, indeed, the ice age cycles are different before 400 kyr BP, something which is well described in the scientific literature and known as the mid-Brunhes transition. It is also observed in many marine records. Though the physical mechanisms behind this transition are not clear yet, there is no doubt that this transition is real. There is no need to invoke some smoothing in the older part of the Vostok record. Climate changes are not a simple repetition of the same pattern, neither in the past, nor in the future. The most likely explanation for not using Dome C is probably the fact that it did not fit in the author's simplistic "cyclic" scheme and he preferred to neglect this additional data.

"Of particular interest in this respect are the peaks signaled by one or two solid dots in Fig. 1. Because, in each instance, a single CO_2 peak correlates with a temperature doublet, such features would again plainly violate the non-contradiction principle if variations in CO_2 concentrations were considered as causes and temperature changes as effects. "

This might also be the reason why the author "sticks" to the old Vostok record: the CO_2 is quite a low resolution record while the temperature one is much better resolved. When using higher resolution CO_2 records (eg. Bereiter et al. 2012) it becomes clear that every temperature peak is associated with a CO_2 one, even for the smaller ones. The "demonstration" of the author is therefore only an artefact linked to his choice of data. Again, this resolution difference between isotopic data and gas measurements is a rather obvious fact in the ice core community. It is not acceptable in a scientific paper discussing ice core results to ignore it and to draw fancy conclusions out of it.

On future glaciations:

"According to a model … the current interglacial period would probably last for 50 000–100 000 more years, depending on the amount of anthropogenic CO_2 emissions (Ganopolski et al., 2016)…. the reliability of such a model should first be established from its ability to reproduce quantitatively the respective durations of past ice ages. As long as such an assessment is lacking… "

I suspect that the author has not read the Ganopolski paper, otherwise he would have noticed that the results are in fact based on the ability of the model to reproduce the respective durations of ice ages and interglacial periods: this is precisely what allows Ganopolski et al. to provide future scenarios.

If the author had read the paper, he would also have noticed that without anthropogenic CO_2, the current interglacial is expected to last about 50,000 years. This is indeed a classical result from the astronomical theory, that the current astronomical forcing is very likely insufficient to trigger a glaciation today or in the next few centuries. According to the astronomy and without any anthropogenic CO_2, the start of the glaciation is therefore postponed by 50,000 years. This is only a direct consequence of the shape of the insolation curve…

"the succession of eight major glacial cycles during the last 800 kyr… suggests that an onset of global cooling might be observed within less than a couple of millennia from now"

It is interesting to see that the author is a strong "promoter" of the astronomical theory, but refuses to apply it when it predicts something that does not fit his own beliefs. The insolation curve is not periodic, and therefore there is not reason for ice ages to be periodic.

In the abstract:

"The climate debate… being the latest of the great controversies…"

There is no scientific controversy concerning global warming: climate scientists have been almost unanimous on this topic for many decades. But, due to the economical implications, it has been documented and demonstrated at numerous times by sociologists or journalists that there is indeed a lot of disinformation. This paper is only one example, and its citation list provides a few further ones, for instance:

"The radiative forcing of CO_2 and CH_4 has been found to be several times smaller than changes in solar insolation…(Soon, 2007)"

Willie Soon is indeed known as a scientist paid by the fossil fuel industry, according to the new-york times:
https://www.nytimes.com/2015/02/22/us/ties-to-corporate-cash-for-climate-change-researcher-Wei-Hock-Soon.html

"… doubling the current concentration of CO2 and CH4 would increase their forcing by a few percent… (van Wijngaarden and Harper, 2020)"

This preprint (never published in a scientific journal) was subject to an article in Science (2019) with the comment that « William Happer… worked within the White House (for the Trump administration) to conduct a hostile review of climate science ».
https://www.sciencemag.org/news/2019/11/unpublished-paper-former-white-house-climate-adviser-calls-methane-irrelevant-climate

Last but not least

Page 101:

"An effect cannot last for a shorter period of time than its cause"

Of course it can… There are myriads of example, based on many different mechanisms, where a permanent (or long duration) change induces a temporary (short) response. For instance, transient phenomena in physics: switching on the light sends a transient electromagnetic wave. Adaptation in biology: resilient systems come back to their initial states after some time even when the cause of perturbation remains. I am quite amazed by the amount of peremptory wrong statements made by the author.

Page 103:

"A lack of correlation resolutely rules out any causality"

This is of course also plainly wrong. Correlation and causality are two very different notions that share nothing in common. The classical textbook counter-example is furthermore the simplest possible physical system. For a linear oscillator, forced by a sine, the response might be exactly a cosine. The correlation between the two is then precisely zero, even though the cosine is obviously "caused" by the sine signal. See for instance Altman et al. (2015) for a discussion on these basic statistical topics. I am

surprised that the author, who is so keen on discussing scientific rigour, can make such beginner's mistakes.

Altman, N., & Krzywinski, M. (2015). Association, correlation and causation. *Nature Methods*, *12*(10), 899–900. http://doi.org/10.1038/nmeth.3587

Bereiter, et al. (2012). Mode change of millennial CO2 variability during the last glacial cycle associated with a bipolar marine carbon seesaw. *Proceedings of the National Academy of Sciences*, *109*(25), 9755–9760.

Referee report — re-review of a previously published article
History of Geo- and Space Sciences

Richet, Pascal. "**The temperature–CO_2 climate connection: an epistemological reappraisal of ice-core messages."**

This article claims to be an "epistemological" analysis of whether carbon dioxide played a causal role in the ice age cycles visible in Vostok ice core records covering 423 kyrs. It concludes that CO_2 concentration changes were an effect of temperature changes caused by Milankovitch orbital cycles, and themselves played at most a "minor" causal role (as a feedback) in Earth temperature change. In a somewhat separate argument, the article criticizes "climate modeling" (apparently referring to general circulation models) as suffering from "circular reasoning." It seems to assert that virtually all existing paleoclimatology relies fundamentally on such models. Prof. Richet concludes that the Vostok ice core data demonstrate "at most, a minor greenhouse effect of atmospheric CO_2 (and CH_4) compared with that of water vapor throughout the last 423 kyr" (p 99). Finally, he imagines that CO_2 concentrations during the historical period of record are random fluctuations, at least until the 1910s, and that anthropogenic CO_2 and methane emissions are likely unrelated the approximately 1.2°C rise in global average surface temperature since pre-industrial times. The article's empirical base is limited to the Vostok data. Prof. Richet has read widely, but quite selectively, citing a number of papers of dubious quality that pretend to refute the dominant theory of CO_2-induced climate change. According to his publication record, Prof. Richet has no previous publications in climate science.

In a nutshell, Prof. Richet would like us to believe that simply by eyeballing a graph of CO_2, CH_4, Antarctic temperature, and insolation changes at 65°N, he can see what generations of scientists before him have failed to understand, namely that carbon dioxide has played little or no role in Earth temperature variations over the last 423kyr.

This article has already been published, but the quality of its peer review process was questioned, which is the occasion for this re-review.

Recommendation: In my opinion, despite many interesting features, the central claims of this article are highly misleading. Further, the article fails to take into account key elements of physical theory that disconfirm its findings. The article **should have been rejected** in view of the points that follow below. I leave it to Copernicus Publications and the journal editors to decide what to do.

Misdirection is the stage magician's technique of keeping the audience's attention focused on one thing — even seeming to explain exactly what the magician is doing — while surreptitiously doing something else so the audience won't notice.

There's a lot of misdirection going on in this article. It takes at least five forms.

(1) **Using historical anecdotes to exaggerate the possibility of a dramatic reversal in scientific understanding of the carbon dioxide greenhouse effect.** The author correctly notes that historically, scientific paradigms have occasionally changed, even dramatically. Sometimes even a single individual (e.g. Copernicus) has created a scientific revolution by finding a new, simpler way of explaining observed phenomena. This is certainly true (Kuhn, 1962), though Kuhn himself ultimately rejected the stronger forms of his own incommensurability hypothesis in favor of more incrementalist views (Kuhn, 1977).

Presumably Richet's historical anecdotes are what justify publication of this article in a journal ostensibly devoted to "history of geo- and space sciences," even though the article is not really about the history of the geosciences and except for one section, actually ignores most of that history. Instead, they are presented only in order to conclude that "the history of science is **nothing more than** a long stroll through the cemetery where ideas that were overwhelmingly accepted are now resting in peace."

This is misdirection. It is an unjustified exaggeration in service of the idea that in 2021 it is permissible, even salutary, to dismiss or ignore most existing research on the phenomenon he is seeking to explain.

Science is a cumulative enterprise that functions through research communities (Merton, 1973). If this article were truly about history of science or epistemology, it would acknowledge that across the 20th century and into the present one, scientific communities have grown much larger; methods of measurement and analysis have been increasingly well codified, tested, and refined; mutually supporting results have created large areas of science that are no longer contested; and the number of dramatic revolutions and reversals (relative to the number of scientists and their publications) has declined as the use and cross-checking of results by larger numbers of researchers has grown. Revolutions are always possible, but they are considerably less likely today than in the time of Aristotle, Copernicus, or even Kelvin or Einstein.

Richet uses the Milankovitch controversy (described on p. 103) as an example of his "cemetery of once overwhelmingly accepted ideas," but this was a controversy rather than a revolution. The concept of astronomical causes of ice age cycles dates to speculations by (Herschel, 1832) and more detailed studies by (Croll, 1864, 1885) and others. (Paillard, 2015) discusses speculative astronomical hypotheses reaching as far back as Aristotle. As Richet notes, the plausibility of orbital controls was widely rejected in the mid-20th century. However, even as other explanations gained favor, orbital controls appeared regularly as one possibility to be considered in the absence of definitive evidence (Malone, 1951, 354).

A better reading of that controversy is that increasingly refined measurement techniques and new data (carbon dating, then ice cores) became available to evaluate a theoretical claim.

The "Arrhenian paradigm," as Richet calls it, followed a similar trajectory. Based on her experimental measurements, Eunice Foote first proposed that higher CO_2 concentrations would warm the atmosphere (Foote, 1856; Ortiz and Jackson, 2020). CO_2's radiative activity was measured more precisely and elaborated by Tyndall (Tyndall, 1861) long before Arrhenius. Geological carbon cycles were hypothesized by Chamberlin (Chamberlin, 1897, 1898). Arrhenius's theory was rejected by many around 1900 due to Angstrom's conclusion that water vapor absorbed radiation in the same bands as CO_2 (Anonymous, 1901), but it continued as a minority view throughout the early 20th century (Arrhenius, 1908; Hulburt, 1931; Malone, 1951, 1015-16). New, more precise instruments detected CO_2 absorption bands that did not overlap with water vapor and allowed more precise calculations of CO_2's radiative effects in the atmosphere. Callendar's careful studies (Callendar, 1938, 1939, 1941, 1949, 1958, 1961) were received skeptically at first (see the Royal Society discussion included in the 1938 paper). But they became accepted as foundational by the 1970s, as the core results of his research were confirmed by others (Weart, 2003; Fleming, 2007). Recently, Hawkins and Jones (2013) compared Callendar's 1938 and 1961 reconstructions of global temperature with the best modern records and found exceptionally close correspondence.

The Milankovitch and Arrhenius controversies are not examples of scientific revolutions like those of Copernicus or Einstein, but of what Kuhn would have called "normal science": puzzle-solving through the waxing and waning of attention to different hypotheses in discussion and debate among qualified experts, with wide acceptance arriving as improved scientific methods and measurements supported some and disconfirmed others.

(2) **Unjustified characterization of his argument as logic rather than empirics.** Prof. Richet frames his argument as based on "epistemology," "pure logic," and "fundamental logic," often citing Aristotle. Frequent repetition of this assertion gives the impression that his argument is deductive, or at least so simple and direct in its chain of reasoning as to be unimpeachable.

This is misdirection. In fact, most of his main argument is inductive and empirical. He sees patterns in data and induces relationships, without genuinely accounting for other patterns and data that contradict or temper his assertions. He mentions – but then simply brushes aside – all uncertainties in his primary data source. He dismisses all knowledge of the physics of greenhouse gases, and essentially all of modern geophysics and geochemistry.

We might have expected an analysis such as this to consider, for example, the known radiative properties of carbon dioxide and methane. For well over 150 years, these and other greenhouse gases (GHGs) have been analyzed in many different ways. Their radiative properties have been measured in the laboratory with increasingly sensitive instruments, demonstrating that CO_2 absorbs radiation in bands that water vapor does not. Water vapor and clouds account for the majority of greenhouse forcing, as the author notes. However, because water vapor's lifetime in the atmosphere is days to weeks, long-lived, non-condensing, globally well-mixed GHGs such as CO_2 control greenhouse forcing (Lacis et al., 2010).

We might also have expected this analysis to look more closely at studies of the most recent deglaciation, for which more data of greater accuracy and precision are available. For that deglaciation, a "seesawing" of heat between the northern and summer hemispheres as well as changes in the Atlantic meridional overturning circulation (AMOC) have been observed in some 80 proxy temperature records from around the world, with northern high latitudes warming first, followed by an AMOC shift, then a northern cooling and an Antarctic temperature increase, with a delay of centuries to millennia (Shakun et al., 2012).

This finding highlights another problem: Richet treats a local record (the Antarctic temperature proxy) as if it were global. Temperature reconstructions for the rest of the world are available from ocean cores and other sources, but Richet prefers to wave them away as irrelevant on the longer time scales he is considering.

As always, the devil is in the details. Milankovitch cycles change the distribution of insolation between the hemispheres, but oceanic and atmospheric mechanisms of heat transport from one hemisphere to the other certainly play a role in how the Earth responds.

3) **Vague statements masking major unresolved issues.** The article recognizes that the complexity of the Earth system makes it very difficult to analyze the relative significance of the many factors operating to cause changes in Earth's climate, such as Earth system feedbacks. It discusses many of these complexities, including (for example) water vapor feedback and changes in ocean circulation, but only in general, qualitative terms. Although instrumental and proxy measurements as well as theoretical

calculations of these factors are widely available, the article quantifies few of them and thus does not confront critical issues of measurement and uncertainty.

This is misdirection. It relies on vague terms *s*uch as these: "the concentration of atmospheric CO_2 up to 300 ppm had **minor effects at most** on temperatures during the past 423 kyr" and "[Earth] temperature variations were **mainly** determined by insolation changes during Milankovitch cycles." What quantities do these terms represent?

4) **Attacking a straw figure.** A good deal of effort is spent arguing in favor of a conclusion that is widely if not universally accepted, namely that Milankovitch orbital periodicities played a key role in initiating glacial terminations over at least the past 800kyr, with GHG concentrations lagging Antarctic temperature changes.

This is misdirection; Richet attacks a straw figure. Few if any climate scientists reject the "pacemaking" role of Milankovitch cycles in Quaternary climate change (Hays et al., 1976). Who has argued that "temperature rises along Milankovitch cycles could have been **triggered** by increasing CO_2 concentrations," as he puts it (p. 103)? It is hard to understand why he thinks this view must be attacked, since (as he himself states) most hypotheses about these cycles view CO_2 change as an amplifying feedback to changes in insolation and ice sheets, not as the initiating cause.

Richet would like us to conclude from "logic" and "first principles" that orbital cycles are the **only** cause of the sharp 100kyr temperature increases seen in the record, because (he asserts) no Earth-based phenomenon corresponds with the strikingly steep and regular 100kyr deglacial temperature increases. He further reasons that the CO_2 peak widths to which he draws attention are approximately twice those of the peak widths of temperature, and that the existence of (global) CO_2 increases without accompanying (local) Antarctic temperature increases mean that temperature must be the cause and CO_2 change purely an effect.

Prof. Richet's logic certainly shows us that CO_2 and CH_4 changes did not initiate deglaciations by themselves, and that CO_2 and CH_4 changes alone are insufficient to explain all of the observed temperature oscillations – but so far as I know, no recent science has claimed that they were.

Few scientific problems can be resolved by logic alone, and this one is no exception. They are empirical and require mechanistic explanations. How much additional radiative forcing do Milankovitch insolation cycles provide? By itself, is that forcing enough to account for the observed temperature changes in the Vostok record? These would seem to be critical questions given the author's hypothesis, yet they are not discussed at all. Estimates of Earth insolation changes have been computed from theory and observations (Milankovitch, 1920; Berger, 1978; Imbrie, 1982) and refined over time (Berger and Loutre, 1991; Laskar et al., 2004). These estimates suggest that the total radiative forcing necessary to achieve the temperature peaks seen in the ice core record is approximately double the amount of energy input available from Milankovitch insolation peaks (Hansen et al., 2008; Hansen and Sato, 2012).

If insolation changes due to orbital cycles account for only half of the observed glacial/interglacial temperature changes, the other half of the forcing must be due to Earth-based feedback mechanisms.

The physics of CO_2 and the observed rough correlation of CO_2 with temperature over long time periods virtually guarantee that CO_2 plays *some* role, but there are many other feedbacks, so the interaction of CO_2 with these other mechanisms must be considered in any potential explanation. Models — which

Richet rules inadmissible by fiat — are simply tools for investigating different hypotheses about how these complex interactions might have worked together to produce the climate record seen in measurements and proxy data.

In the case of the 100kyr glacial terminations of interest to Richet, an explanation remains elusive. The strongest insolation effects of Milankovitch periodicities are obliquity and precession. Milankovitch himself believed that the 41kyr obliquity cycle mattered most. And indeed, before the Mid-Pleistocene Transition (MPT) about 1 million years ago, Quaternary ice age cycles matched the 41kyr obliquity cycle (Elkibbi and Rial, 2001). After the MPT, they shifted to the 100kyr regime reflected in the Vostok record. So far, the best available explanation of this shift involves declining CO_2 and regolith removal (Willeit et al., 2019).

The 100kyr eccentricity cycle has considerably weaker insolation effects than obliquity or precession, yet it is associated with the largest and steepest temperature increases in the 423kyr record. The mechanism by which eccentricity changes could cause such large temperature increases would therefore seem to require other, non-linear, Earth-based mechanisms triggered by the eccentricity cycle, as clearly stated in (Hays et al., 1976, 1131) but ignored by Prof. Richet. This remains an unresolved puzzle (Paillard, 2015), and Prof. Richet's "fundamental logic" notwithstanding, Earth-based mechanisms have been proposed to explain these events. Ice-sheet, sea ice, and land surface dynamics doubtless play a role, as do albedo feedback effects. So do thermohaline circulation shifts, as well as carbon cycle changes related to such shifts. A non-astronomical mechanism involving a "sea ice switch" has been proposed in which obliquity and precession cycles play key roles, but the 100kyr cycle is "phase locked" by Earth-based mechanisms which may also play a part in other rapid climate changes (Gildor and Tziperman, 2001).

The literature on these issues is vast. Nonetheless, ***based only on a superficial reading of that literature,*** Richet would like us to conclude that because carbon dioxide levels do not perfectly track the (local) Antarctic temperature record, CO_2 can play no more than a "minor" role "relative to water vapor." This is a dubious way to put the point, since CO_2's direct effect is *always* "minor relative to water vapor," which at present-day temperatures accounts for about 75 percent of the total greenhouse effect; the key thing, as already stated, is that due to its short lifetime in the atmosphere, water vapor is controlled by long-lived, non-condensing greenhouse gases such as CO_2 (Lacis et al., 2010).

As stated by the American Chemical Society, "water vapor does not control the Earth's temperature, but is instead controlled by the temperature. This is because the temperature of the surrounding atmosphere limits the maximum amount of water vapor the atmosphere can contain. If a volume of air contains its maximum amount of water vapor and the temperature is decreased, some of the water vapor will condense to form liquid water… If there had been no increase in the amounts of non-condensable greenhouse gases, the amount of water vapor in the atmosphere would not have changed with all other variables remaining the same."

Richet fully accepts that water vapor feedback is critical to Earth temperature, even though it cannot explain temperature change by itself. How is it logical to simultaneously reject CO_2 feedback?

5) **Baseless rejection of climate modeling as a scientific tool.** Richet rejects (apparently all) "climate models" out of hand as unfalsifiable. While this is true of general circulation models and Earth system models, it is also well understood by climate modelers, who have developed numerous experimental methods to diagnose and evaluate their failures and shortcomings, most rigorously in the series of

Coupled Model Intercomparison Projects underway since 1989. He does not seem to know much about climate models, since he completely ignores simpler climate models (zero, one- and two-dimensional) that reach similar conclusions. Nor does he display any knowledge of how GCMs and Earth system models are tested or how they are constrained by empirical observations (Edwards, 2010).

Richet makes use of this out-of-hand rejection to avoid examining or responding to *all* studies, including paleoclimate studies, that use models to assess the relative importance of different factors. He argues that because the period of instrumental record (since roughly 1850) is brief relative to the Vostok record, it is impossible to tell whether the observed CO_2 rise — at least through the 1910s —is a cause of post-industrial global warming, or merely a random fluctuation. The claim is essentially that "a long record trumps a short one," even though uncertainties in the Vostok record are far larger than those of the instrumental period. He then questions (again in vague, unquantified terms) the idea that anthropogenic CO_2 emissions — already substantial by the 1910s, following over a century of rapidly growing coal- and oil-fired industrialization — have any effect on global temperature.

This is misdirection. It ignores the well-established physics of greenhouse gases. It ignores satellite measurements of the Earth radiation budget and of the radiative signatures of CO_2 and other GHGs (Evans, 2009). Multiple independent lines of evidence — not only models, but process understanding, instrumental measurements, and paleoclimate proxy data — converge on similar conclusions about CO_2 warming effects. Recent research constrains climate sensitivity to CO_2 doubling to the likely range of 2.6-3.9°K, less than half the width of previous estimates (1.5-4.5°K) — in a study that used GCMs only as checks, not as primary sources for the value of climate feedback variables (Sherwood et al., 2020). The IPCC WGI Sixth Assessment Report, due out on 9 August 2021, reaches similar conclusions, also informed by *but no longer based mainly on* climate models (Chapter 7).

These are, quite simply, unfounded and unscientific assertions. By Richet's argument, **all** complex modeling is unfalsifiable and unscientific – yet virtually all modern sciences make use of complex models. Climate scientists are well aware of the shortcomings of climate models, and frequently publish work evaluating them (AMIP, CMIP3, CMIP5, CMIP6). During the instrumental period, many more variables have been measured and their roles in climatic processes examined than can ever be measured in paleoclimate studies. Modeling is only one aspect of climate science; process understanding, instrumental measurements, and paleoclimate proxy data are others.

Further, the empirical data on which Richet himself relies (the Vostok record) are presented as bedrock, incontrovertible, unmediated evidence. Yet as in the case of virtually all long time-series data (Edwards, 2010), in fact the Vostok data were produced by applying various models to raw information, and they are subject to ongoing refinement and revision. For example, Petit et al. (Richet's primary data source) created an age model, GT4, to estimate the dating of their record, with considerable associated uncertainties: "accuracy of GT4 is always better than ±15 kyr, better than±10 kyr for most of the record, and better than ±5 kyr for the last 110 kyr" (Petit et al., 1999, 432).

Richet's entire presentation appears geared to support the following claim:

> In the atmosphere, the maximum CO_2 concentration of 300 ppm found in the Vostok record was reached again in the 1910s. The main effect of such high concentrations was simply to increase considerably the subsequent CO_2 time lag behind the temperature leads, without significant effects on past climate. Hence, **it is doubtful** that any significant global warming could have

been caused by human emissions during most of the 20th century as a result of the additional 50 ppm CO_2 increase observed until the 1980s.

The physics of greenhouse gases is one of the best-studied fields in modern science, but Prof. Richet would simply erase it all based on a simplistic reading of three global and one local variables in a single dataset.

Much is made here of Aristotle's version of Occam's razor: the principle that the simplest explanation is the best. Parsimony is a very useful heuristic, indeed, but it is essentially an aesthetic principle, not a logical or epistemological one. Further, it carries an indispensable *ceteris paribus* caveat: *all other things being equal,* the simplest explanation is to be preferred. In other words, in the case of two competing explanations that work *equally well* to explain *all known elements* of some causal process, the simpler one is to be preferred — but even under these conditions, nothing guarantees that the simpler explanation is actually correct or true. In the case of Prof. Richet's analysis, much of my preceding discussion shows that the *ceteris paribus* condition does not apply. All other things are NOT equal, and the simplicity of his explanation does not guarantee its truth.

The "epistemological weakness" Prof. Richet ascribes to climate models is in fact most apparent in his own analysis.

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Post-publication review of

The temperature-CO2 climate connection: an epistemological reappraisal of ice-core messages

by Pascal Richet

I have been invited to carry out a post-publication review of this paper. I should declare that I have not been involved in any way in the process that lead to the publication of this paper, nor have I seen previous reviews. Therefore, my judgement is based only on the published text.

First, some formalities. I note that HGSS has published this paper in a remarkably rapid way. From submission to revision less than two months have passed. This period includes screening the paper, assigning reviewers, receiving and summarizing reports, taking an editorial decision including requesting revisions, and preparation of a revised version by the author. All this in just 8 weeks? Only three weeks later, the paper was accepted suggesting neither a second round of review nor a thorough revision. From the Review Statement at the end of the paper I note that four reviewers have provided reports. Two of these reviewers are either biased or not experts in the two fields this paper addresses (ice core science and climate modelling). The first (Lindzen) has a long and documented history of unsupported views on the influence of humans on the climate system, the carbon cycle, the physics of the greenhouse effect, and global climate feedbacks, and has never worked on ice cores. The second (Hovland) is a deep sea geologist with peer-reviewed articles mostly dealing with deep sea floor characterisation, apparently not an expert on CO2-climate connections, climate models or ice cores. The other two reviewers remained anonymous. Evidently, the fast turnover of this manuscript with four reviewers indicates that the reviews were either insubstantial or were essentially ignored. In my view, as substantiated below, this paper does by far not, in my view, reach the quality for publication in a peer-reviewed journal of Copernicus. An editorial action seems necessary and urgent to safeguard the integrity of this journal, and of Copernicus at large.

Major Comments:

1) In this paper the author makes two fundamental claims: i) variations in the atmospheric concentration of CO2 have a negligible effect on global climate, and ii) climate models are insufficient tools to understand the climate system. These two claims are made on the basis of a combination of a few considerations of eminent philosophers, one paleoclimate record, and unsubstantiated statements on climate modelling.

Clearly, the two claims can and have been made in the past. But in order to be convincing and becoming a valuable contribution to the scientific progress in this area, the scientific standards must be maintained and the burden of proof is extremely high. Neither is the case for this paper. Usually, Popper is invoked in this context, but the subject of investigation is not a problem of pure mathematics or logics but a complex dynamical system that requires thee elements: observations, theoretical understanding, and a hierarchy of models. These three elements have established the pillars of climate science of the past 60 years. The author's understanding of paleoclimate archives (in particular ice cores and their many environmental indicators), understanding of climate theory (in particular the greenhouse effect, both natural and anthropogenic), understanding of climate models (in particular model development, assessment,

application, and model hierarchy) is demonstrably insufficient, or outright flawed to make the above sweeping claims.

2) The author almost completely ignores 20 years of ice core research which has yielded many independent lines of evidence for CO2 and other greenhouse gas concentrations of the past and temperature reconstructions. Restricting the discussion to the CO2 and Antarctic temperature records from one single core, the Vostok ice core published in 1999, seriously limits, or invalidates, the conclusions put forward in this paper.

3) The author almost completely ignores 30 years of climate science, particularly as concerns observations during the instrumental era in the atmosphere, the ocean, the cryosphere, and on land. These observations constitute a fundamental part of the foundation on which statements about the dominant drivers of observed changes in the climate system rest. Limiting reference to observed changes in the vertical temperature profile of the atmosphere to argue that current models do not perfectly reproduce this, is inevitably insufficient to categorically refute their validity or physical realism.

4) The author almost completely ignores 20+ years of science that lead to the current generation of comprehensive climate models. Such climate models are numerical representations of well-known physical processes (e.g., the flows of air and water in the atmosphere and oceans, transfer of radiation in the atmosphere, heat, momentum and substance transfer between atmosphere, ocean and land, phase changes of sea ice, and many more), plus representations of small-scale processes such as the formation of clouds. While models are certainly not perfect, and never will be, the most highly resolved members of these models generate large-scale atmospheric and oceanic circulation systems that now are hardly distinguishable from satellite images. This demonstrates that such models do have predictive capabilities. Weather forecast is a point in case, as are climate hindcasts of the 20th century climate development scrutinized in many international coordinated efforts through UN WMO's projects. For physical reasons predictability does not currently extend beyond about 10 days for known reasons. However, this does not preclude reasonable estimates of many large-scale quantities and statistics of weather elements for the coming decades and centuries based on scenarios, owing to the strong constraint of planetary energy balance. This is why the less complex, less highly resolved, and less complete ancestors of today's climate models have already projected quite accurately the warming in response to continuing CO2 emissions more than 30 years ago and that is now observed (see the First Assessment Report of the IPCC and successive assessments).

5) The author completely ignores 40+ years of carbon cycle research which has by now provided a very detailed understanding of the Earth System components that are important drivers of changes in atmospheric CO2 changes on various time scales. For example, the terrestrial biosphere, as well as the Southern Ocean, particularly the deep part, are both substantial carbon reservoirs that are fully implied in the glacial-interglacial variations. These range from interannual to decadal, centennial and millennial to ice age cycles. This body of knowledge is essential to make meaningful statements about possible links between CO2 and climate as seen from the ice cores. This science also makes evident, that the self-imposed limitation to one single, low-resolution and low precision record, the Vostok record, deprives the author of making the type of conclusions put forward in this paper.

6) The author lacks a deeper understanding of one of the key variables that this study focuses on. The information from the Antarctic ice core is NOT global mean temperature, but it is Antarctic temperature at the location of snow accumulation. While this is acknowledged at the beginning of the paper, this important point gets lost in the following. A recurring argument is a purported 7 kyr lag of CO2 with respect to temperature. This lag is apparently extracted from figure 1 on the basis of a "width-at-half-maximum" determination. The analysis does not stand the least test of rigor as inspection of Figure 1 makes evident. Since this signal is far from harmonic or pulse-like, the notion of "half maximum" becomes highly subjective. The author does not even bother to discuss this issue nor to estimate an uncertainty on such estimates. Therefore, any statement that is based on the claimed 7 kyr lag, is questionable unless a careful analysis, not only on Vostok data but on ALL available Antarctic ice cores, is carried out.

6) The author seems to lack an understanding of some of the most basic climate system processes. For example, the author is apparently unaware of the role of albedo and dust during glacial-interglacial cycles (energy balance of the Earth), unaware of the ocean as a reservoir of heat and carbon (understanding leads and lags of temperature and CO2), ignorant of the role of the water cycle (transport of the isotopic signal to Antarctica), unaware of internal processes of the climate system ice sheet flow and ice-ocean interactions (modification of individual Milankovic cycles with respect to length and amplitude), unaware of sea level changes (as a global synchronizer), not knowledgeable on circulation changes in the atmosphere and ocean (modifying heat transport and generating interhemispheric connections), etc., etc. However, this knowledge would be a prerequisite to declare the current understanding of the climate system and the response to increasing levels of greenhouse gases "one of the great controversies that have punctuated the march of Earth sciences".

7) The author uses several references that are out of context, pseudo-scientific, incorrect, not peer-reviewed, mis-interpreted or irrelevant for the argument, to underpin affirmations that would otherwise not be tenable. This can be considered as purposefully misleading the reader who would accept such underpinning on the basis of trust. The average reader will not delve into the individual references to assess, one by one, whether the citation is appropriate at this place, whether it has the expected quality, and whether it actually supports the argument made. Examples of references that fall into at least one of the above mentioned categories are Allmendinger (2017), Blaauw (2017), Chiligar et al (2008), Delgado-Bonal (2017), Frank (2019), Herzberg et al. (2017), Hertzberg and Schreuder (2016), Humlum et al (2013), Kukla and Matthews (1972), Lewis and Curry (2018), Lindzen (1997), Mitchell et al (2020), Nikolov et al., (2017), Petit (2013), Soon (2007), Van Wijngaarden and Happer (2020).

8) The is a fundamental flaw in the chain of arguments presented by the author. The author implicitly assumes that CO2 is ALWAYS a driver in the climate system. This assumption is used to apply Aristoteles' argument of cause and effect when a lag for CO2 relative to temperature is found, and the author concludes that therefore, CO2 CANNOT be a driver. This reasoning is completely wrong. As an infra-red absorbing molecule CO2 can operate both as a driver and a feedback in the climate system, the latter due to the global carbon cycle. A key process that is responsible to CO2 taking a strong role a driver is that the molecule is added to the climate system in great quantities through the burning fossil fuels. Hence new responses are generated in the climate system that are superimposed on the still ongoing processes that produce a CO2 feedback.

10) This reviewer is not a philosopher nor have I experience in epistemology. However, I can judge whether a specific statement by a most respected authority some centuries ago would be today a justified argument for affirmations and conclusions in this paper. I find that none of these authority-commanding quotes are actually applicable to the questions at hand. One could actually consider these quotes, and references in Comment 7 above, as inappropriate references generating an illusion of erudition and scientific approach. Examples are Aristoteles on p. 102, to falsely support the claim that CO2 cannot both driver of climate change and driven by climate change (see comment 8), or Popper, whose insightful statement is inappropriately applied, or Aristoteles again on the complexity of an argument when the author does not offer a simpler explanation of all evidence so far accumulated by climate science. Or Macrobius' statement that is applied to a time where climate science combines observations, paleoclimate reconstructions and model simulations, all multiple lines of independent evidence, again a quote stemming from a time when the modern scientific tools were not yet developed. The paper closes by a quote from Buffon: "… to distinguish well between what there is of the real in a subject from that which we add to it arbitrarily as we consider it: to recognize clearly which properties belong to the subject and which properties belong to the subject and which properties we only imagine it to have." Nothing could be more appropriate as this statement fully applies to the author's flawed exposition.

Specific Comments:

1) The statement that anthropogenic and natural gases cannot be distinguished is plain wrong. Both for CO2 and CH4 the isotopic signature of the anthropogenic signal is measured and permits an independent quantitative estimate of the anthropogenic emissions (Suess effect).

2) Referring to Broecker (1975) when estimating climate sensitivity amounts to ignoring 46 years of research on this topic drawing information from observations, paleoclimate archives and climate modeling (e.g., Sherwood et al., 2020).

3) Vostok is not the "Rosetta stone of climatology". No serious colleague would claim that, although it has been a most influential study whose effect cannot be underestimated. However, the complex Earth system and its past natural variations provide a context for the changes today only if the environmental record is analysed comprehensively. That is, marine sediments, lake sediments, tree rings, speleothems, polar and high-altitude ice cores, all must be considered. A paper, not referred to by the author, but centrally relevant is Shakun et al (2012), who estimate changes of the global mean temperature and its temporal relation to CO2 concentration is not only based on one ice core but on a collection of information from different archives. The conclusion of this study is not consistent with the claims made in this paper.

4) The paragraph preceding the statement "It is in this way that the modern anthropogenic increases …" indicates that the author is unaware of the physical processes of the natural and enhanced greenhouse effect.

5) The author seems unaware of the past 10+ years of international projects that have assessed the performance of climate models in simulations of past climate states and comparison with the evolving knowledge from paleoclimate archives.

6) The statement "... one can then neglect climate variations caused such by factors such as changes in solar activity …" is factually wrong as ice sheet dynamics, atmospheric dust load and ocean heat content and circulation status are precisely those processes that are need to understand the ice core records (plural).

7) To assess the ice core data by "… pure logic and thus on the fundamentals of scientific reasoning …" is factually wrong as an assessment of this paleoclimate archive requires the best possible measurements, and understanding of the processes of archiving (for example the bubble formation and the transport of gases in the firn), as well as the large-scale climate processes (e.g., the water transport providing the stable isotope signal in the ice cores).

8) The statement on the "… minor greenhouse effect of atmospheric CO2 compared with that of water vapor" is wrong as water vapor is considered to be internal to the climate system and approximately doubling the response due to other forcings (irrespective of its origin, e.g. solar, volcanic or greenhouse gases). Without the variations in CO2 and CH4 the glacial-interglacial temperature changes in the atmosphere and the ocean cannot be explained quantitatively, i.e. albedo, dust and clouds are not sufficient.

9) The author is not correct when he claims that the "… lower resolution prevents further information …" and thus ignores results from the EDC core.

10) The Gest et al study has not been accepted in final form. The published paper on the same topic is Beeman et al. (2019).

11) The statement "Even for CO2, however, diffusion appears to have been generally unproblematic …" reveals a fundamental lack of understanding of polar ice cores as a paleoclimate archive (e.g. WAIS Divide Members, 2015).

Comments on specific sections:

1) section 3.2: Geochemical inferences

i) wrong. None of the glacial-interglacial signals in the polar ice cores shows perfect periodicity.

ii) vague to wrong. No mention of albedo and dust as most important additional drivers.

iii) wrong. Ignores the role of the ocean heat reservoir.

iv) wrong. The climate system is not linear, long recognized by the saw-tooth shape of the ice age cycles. Building an ice sheets takes approximately 10 times longer than melting it.

v) incomplete. The most important process influencing concentrations of carbonate species CO3=, HCO3- is the ocean's acidity.

vi) wrong. Toba has been found in ice cores from Greenland and Antarctica (e.g. Svensson et al., 2013).

vii) trivial statement, and misleading. Only considering Vostok does not provide a currently uptodate view of the "variety of intensities and timescales".

viii) reasoning not clear and incomplete if the EDC ice core is not considered.

2) section 4.1: The atmospheric CO2 concentration: a plain temperature effect?

The author claims a close coupling between CO2 and temperature during the ice age cycles by noting that the observed increase of temperature and CO2 from 1980 to 2011, and thereby turning upside down the physics. The Humlum et al (2013) study is quoted, a study has been shown to be plainly wrong (see comment articles it generated).

The reasoning in the last para on page 101 is wrong because it is assumed that no other processes than Antarctic temperature changes are responsible for CO2 variations (see comment 6). The width at half amplitude is completely arbitrary and not a robust method to define "amplitude" on this record.

3) Section 4.2: The CO2 feedback

This section evidences a complete lack of knowledge of the processes that can influence atmospheric CO2 concentration on the glacial-interglacial time scale. Instead of consulting what is known about the global carbon cycle (e.g., Fischer et al., 2010) the author ruminates about peak shape, synchroneity and higher complexity of the temperature record than the CO2 record. All these arguments lack any quantitative information, nor do they consider physical-biogeochemical processes that are well known. The exposition does not meet the scientific standard and rigor of an article in a peer-reviewed journal. The contorted reasoning becomes further evident when CO2 and CH4 variations are compared through "peak widths". And finally, radiative forcing information in W/m2, seems unknown to the author.

4) Section 5.1: The CO2 conundrum

This section presents a sequence of wrong statements and exhibits a complete lack of understanding of modelling climate change. The author claims, invoking Popper, that "responses [of the climate system] under widely different conditions" cannot be checked. This is categorically wrong. It is the very purpose of modern paleoclimate research to quantify "widely different conditions" of the Earth system in the past, e.g. in the past millennium, the Holocene, the last glacial maximum, abrupt climate change, past interglacials, the PETM, etc. etc. Therefore, there are plenty of realizations of natural climate states that can, and have been, explored by a hierarchy of models.

It is factually wrong that the ca. 100 ppm CO2 changes between the glacial maximum and an interglacial warm period had no effect. The combined radiative forcing of greenhouse gas variations has been determined based on an analysis of polar ice cores (Schilt et al., 2010) demonstrating in numbers that the forcing has been substantial.

The author argues that "greenhouse gases can affect climate only via thermal changes" and uses entropy as an argument. This statement is plainly wrong as it ignores the transfer of radiation through the Earth atmosphere and its interaction, at any height of the atmosphere with the different infrared-absorbing molecules in the air. The "heat engine" of the Earth is completely consistent with the basics of thermodynamics including entropy (Peixoto and Oort, 1992, or Gibbins and Haigh, 2020), and so the author's argument about entropy is mute.

The statement “it is doubtful that any significant global warming could have been caused by ….” is not substantiated physically but simply underpinned by claiming a “lack of evidence for feedback [] by the CH4 record”. The author seems to be unaware that the 20th century is an period of unprecedented releases of CO2 and CH4 to the atmosphere through the burning of fossil fuels and food production. The chain of arguments is embarrassingly flawed, and this reviewer remains doubtful that this part has been read by the previous reviewers or the editor.

Surely, no serious climate scientist claims that the climate would “evolve in a smooth manner”, as the non-linear equations are precisely those that are numerically solved in global climate models. While this reviewer does not capture the point the author intends to make in the last paragraph of this section, it is clear that the author appears not to understand the different sources and sinks of CH4 in the climate system (tropical, boreal, stratosphere).

5) Section 5.2: The threat of circular reasoning:

The first paragraph is based on an interview by Petit, and cites the view that the “the importance of the Vostok results has, thus, been central in the current debate…”. This author attempts to rewrite the history of climate science by using this quote, and by ignoring thousands of studies since the publication of Petit et al. (1999). Petit et al. is undoubtedly a historic paper, but the scientific progress since then cannot be ignored. The last sentence of the second paragraph is completely unfounded, polemic, and is an embarrassment for the journal in which it is printed.

In the paragraph beginning with “There is, additionally, a great epistemological weakness …” the author reveals his blunt ignorance of climate models. He also ehibits his flawed logical thinking why climate models cannot be used to estimate the effect of increasing greenhouse gas concentrations in the atmosphere. He claims that 150 years of “direct or indirect observations” [not clear what the latter is, actually] are not sufficient to understand the climate system. In fact, the remarkable physical understanding, revealed by the increasing realism of coupled atmosphere-ocean general circulation models has apparently escaped the author. The only support for this claim is the analogy of ripples at the ocean surface and tides. Fact is that understanding ripples at the surface of the ocean does not require quantification of the gravitational action of Moon and Sun while that of tides would not need consideration of capillary forces. Either physical phenomenon can be sufficiently accurately described by models. The same situation holds in the climate system. Comprehensive climate models are powerful tools to quantitatively understand the current 150 years of observed changes in the climate system. A reduced-complexity model, also based on physical-biogeochemical principles, is a powerful tool to understand the transition from the last ice age to the current Holocene. This also invalidates the author’s claim that “climate models suffer from the circular nature of the reasoning”, and the analogy with the magnetic north pole (figure 3) is simply absurd.

6) Section 5.3: Earth sciences – a pitfall factory

The first two paragraphs are an interesting read but not relevant to the discussion. In the third, the author quotes a few papers that fall into one or more of the categories mentioned in Comment 7. The fourth paragraph makes several affirmations, all of which are flawed or not supported by the cited literature:

i) ignoring that the purported lag is not constant (e.g., Shakun et al., 2012), and that CO2 can be both a feedback and a driver.

ii) The Soon (2007) study has no scientific merit. It has used the same single ice core for the lead-lag argument. *Local* radiative forcing at short waves (insolation at a specific latitude) is directly compared with *global* radiative forcing at long waves (greenhouse gases), a mistake that is often made when arguing that the effect of greenhouse gases is negligible.

iii) wrong statement. Only support is an unpublished manuscript.

iv) statement "… have been strongly called into question" factually correct but the calling into question has been made based on biased or erroneous publications.

v) the paper by Delgado-Bonal deals with entirely different issues on entropy. It mentions climate only in passing and presents no relevant analysis (see comment on section 5.2).

vi) statement "… overall picture …" completely vague, and references are given to papers that are biased or erroneous.

7) Section 5.4: Towards a new ice age?

This is an interesting topic and would make a good example of how climate science evolved addressing a specific issue of important future relevance. Unfortunately, due to the very biased and inadequate literature, it misses this opportunity. The for this argument comprises a reference 1979 (a great book), a meeting report (from 1979), and a scientific paper published in 1970. Since then many important papers have enabled a quantitative understanding of this interesting topic (e.g., see IPCC AR5, section 5.8.3, Berger et al., 2016). Again, ignoring more than 30 years of science in this field is not a scientific basis for drawing conclusions.

Overall conclusion of the post-publication review

As substantiated in detail above, this paper does not achieve the minimum level of scientific quality that would be required for a publication in a peer-reviewed international journal. Therefore, I recommend a clear and transparent editorial action in order to re-establish the credibility of this journal and safeguard its integrity as a member of the Copernicus family of journals serving the scientific community.

<end of review>

Although post-published peer review is special, there is no specific instruction from the publisher side. Thus, here, I have written a referee comment as if this paper was a preprint. My research area is mainly geochemistry and plaeoclimatology (including ice core studies). The following comment is the same format typically used in other Copernicus journals (eg, Climate of the Past or Cryosphere).

General comments:

This manuscript discusses the impact of a CO_2 on climate change based on CO_2 and temperature data from an Antarctic ice core. Although this MS was submitted to a Journal for *History of Geo- and Space sciences*, most of the discussion is focused on the relationship between ice core CO_2 and temperature. I think that deeper discussion of ice core research from the viewpoint of history of science is needed. For geochemical viewpoint, there are misunderstandings about the interpretation of the ice core temperature record. The author's discussion is mostly based on a qualitative (not quantitative) analysis of the published ice core data. In addition, recent studies were not properly cited and discussed. Please see the comments below. Overall, I do not think that this manuscript is suitable for publication in *History of Geo- and Space Sciences.*

Major comments

(1) Temperature-CO_2 relationship

P98 L32-L41 "…*marked time lags observed between temperature and CO_2 variations…., such analyses have not been pursued in any detail….Hence, the first goal of this study is to analyze the nature of these relationships to ascertain*…"

In Section 4. (e.g., P102 Section 4.2 L1-5. "*The simple temperature-CO_2 relationship described in the previous section is not commonly considered, however, because it ignores greenhouse effect.* "

These sentences are not true. Some papers published in major journals discussed the mismatch and/or the lag between CO_2 and Antarctic temperature. See references below, for example. Regarding the interpretation of temperature records derived from Antarctic ice cores, there are least two important points: (i) the reconstructed temperature reflects NOT "global" temperature but "Antarctic" temperature. Thus, difference between Antarctic regional climate and global temperature should be considered. (ii) validity of temperature reconstruction is still under debates.

For the first point, Shakun (2012) discussed the regional differences in temperature. Uemura et al. (2018) explained the systematic difference between Antarctic

Temperature and CO_2. They concluded that the Antarctic temperature is affected by the changes in local insolation induced by obliquity. In addition, local Antarctic temperature affected by changes in elevations of ice sheet. This topic is also discussed in many papers (e.g., Werner et al. (2018) and Buizert et al. (2021). For the second point, there is considerable uncertainty for temperature reconstruction based on isotope composition of ice (e.g., Erb et al. 2018). A recent analysis based on multiple ice cores shows significant discrepancy between temperature reconstructions (Buizert et al., 2021).

The author should follow these studies and references therein.

References:

Buizert et al., Antarctic surface temperature and elevation during the Last Glacial Maximum, Science (2021): 372, 6546, pp. 1097-1101 DOI: 10.1126/science.abd2897

Erb et al., Model evidence for a seasonal bias in Antarctic ice cores, NATURE COMMUNICATIONS, (2018) DOI: 10.1038/s41467-018-03800-0

Uemura et al., Asynchrony between Antarctic temperature and CO_2 associated with obliquity over the past 720,000 years, NATURE COMMUNICATIONS (2018) DOI: 10.1038/s41467-018-03328-3

Hasenclever et al. Sea level fall during glaciation stabilized atmospheric CO_2 by enhanced volcanic degassing. NATURE COMMUNICATIONS (2017) DOI: 10.1038/ncomms15867

Shakun et al., Global warming preceded by increasing carbon dioxide concentrations during the last deglaciation, Nature (2012) 484, 49–54, https://doi.org/10.1038/nature10915

Werner et al., Reconciling glacial Antarctic water stable isotopes with ice sheet topography and the isotopic paleothermometer, NATURE COMMUNICATIONS | (2018) DOI: 10.1038/s41467-018-05430-y

(2) Abstract L1-3 "*...epistemological examination of the geochemical analyses performed on the Vostok ice cores invalidates the marked greenhouse effect on past climate usually assigned to CO_2 and CH_4.*"

This sentence (and related discussions) can be read as if the "epistemological examination" of this paper made a new discovery. But it is not correct.

The main driver of glacial-interglacial cycles is changes in ice volume driven by changes in distribution of incoming solar energy. Compared to this, the effect of carbon

dioxide is small. For example, Köhler et al. (2010) estimated the radiative forcings at LGM: surface albedo changes over land cryosphere (-4.5 Wm^{-2}), sea ice (-2.1 Wm^{-2}) and vegetation (-1.1 Wm^{-2}), green house effect from CO_2, CH_4 and N_2O (-2.8 Wm^{-2}), and atmospheric albedo effect caused by dust loading (-1.9 Wm^{-2}). You may visually recognize the temperature changes driven by Greenhouse gases and ice volume in Fig.2 in Erb et al. (2018). Although these estimate for radiative forcing, especially for dust and vegetation, involves uncertainties, the CO_2 is not the primary driver of glacial-interglacial cycle but one of the important factors. Therefore, the mismatch between Antarctic temperature (which is different from global temperature; see the comment above) and CO_2 concentration itself is not surprising. It does NOT invalidate the greenhouse effect.

Importantly, paleoclimate sensitivity estimates based on these understandings are generally consistent with IPCC estimates for climate sensitivity (e.g., PALEOSENSE project, 2012). I guess that the author misunderstands a difference between climate sensitivity for paleoclimate and ongoing climate changes (please see, PALEOSENSE project, 2012).

References:

Köhler et al., What caused Earth's temperature variations during the last 800,000 years? Data-based evidence on radiative forcing and constraints on climate sensitivity, Quaternary Science Reviews (2010), 29, 1–2, 129-145, doi:10.1016/j.quascirev.2009.09.026

PALAEOSENS Project Members, Making sense of palaeoclimate sensitivity, Nature (2012) 4 9 1, 6 8 3, doi:10.1038/nature11574

(3) Some references are unreliable or inappropriate.

Two references (Allmendinger 2017 and Nikolov and Zeller 2017) are published in Environ. Polllut. Climate Change. "*Environment Pollution and Climate Change"* is published OMICS Publishing Group, which is suspected of being a Predatory Publishing. In fact, Nikolov and Zeller (2017) is based on a "finding" of Volokin and Rellez (2014). "Volokin and Rellez" are names spelled backwards "Nikolov and Zeller". I found a related news article.

https://www.washingtonpost.com/news/morning-mix/wp/2016/09/19/scientists-published-climate-research-under-fake-names-then-they-were-caught/

Petit (2013) is just an interview article in a French website. Without special reasons, this is not suitable for a reference of peer review journal.

Specific comments

P97 L15. "…*epistemological approaches are most valuable*…."

> "Epistemology" has a broad meaning. Please clarify the meaning and definition of your "epistemological approaches" with proper references.

P98 L30 "*..ice-core records have only been partially exploited*."

> I disagree with this. There are many scientific papers about ice core data.

P98 L51-P99 L2 "*Over the long enough time intervals considered, one can neglect climate variations caused such by factors such as changes in solar activity and even on timescales of thousands of years, differences between the freezing and melting dynamics of ice sheets and their dependences of local factors.*"

> Why can you neglect such factors? I don't understand. Please explain more with appropriate references.

P99 L4 "*..a rigorous assessment of the ice-core data can rely on pure logic*…"

> What is "pure" logic? Please explain more.

P99 L8-10 "…*they (conclusions drawn from the ice core record) are not restricted to a specific geographical setting, and they depend neither on any physical mechanisms assumed to be at work in the complex Sun-Earth system nor on any particular climate models ….* "

> This is not true. Except for gas species (e.g., CO_2, noble gas etc.), the ice core data (temperature, dust, chemical components etc) are restricted to a geographical setting. Temperature record from an Antarctic ice core reflects the air temperature at the drilling site.

P101 L. 36-L38 ".. *the beginning of each cycle have been used to determine the minima of the CO_2 and temperature peaks in an objective and consistent way*"

> Please explain in detail how you determined the vertical bars in Fig. 1.