

Reply to the Statement published by Copernicus on the paper by P. Richet, “The temperature-CO₂ climate connection: An epistemological reappraisal of the ice-core messages”, *Hist. Geo Space Sciences*, 12, 97-110 (2021)

Remarks on publication ethics

a. Concerning publication ethics, I read that some reviewers "have ties to industry benefitting from the manuscript conclusions". By the same standards, what should be said of critics who are tied in most various ways to the 89,000 billion dollars (World Bank figure) of the 2015-2030 climate-related business?

b. In the light of the aforementioned discussion, considering as problematic that neutral reviewers can be sought after outside mainstream climatology circles is rather a really serious problem.

c. For people who have taken the pain to read G. Gregori (2015) statements about rigorous ethical standards to be followed in the climate debate, the insinuations made about him will appear in a shameful light. It is because I did read his lofty recommendations that I listed him among other possible reviewers, although I did know that his scientific ideas on climate have nothing in common with mine.

d. Should IPCC-revolving scientists be the only holders of truth? Should they exert a monopoly on peer reviewing of papers dealing with climate, regardless of the approach followed? Should they be given the right to prevent theses dissenting from theirs from being published in the scientific literature, even though respected scientists would support them independently?

Academic aspects

1. *The scope of HGSS is “to document historical facts and knowledge and to improve awareness of the history of geoscience.” While the author discusses popular historic fallacies in the Earth sciences, there is no clear link between the manuscript’s central conclusions and the scope of the journal.*

- One of the main results of the study is to show and explain why “a preconceived notion of causality has led to misinterpret the data” and also led to assign to CO₂ a role that is contradicted by the data themselves. If this does not represent a “clear link between the manuscript’s central conclusions and the scope of the journal”, then let me admit that I don’t understand at all “the history of geoscience”. As for the “popular historic fallacies”, they illustrate how physical chemists have repeatedly misled Earth scientists in great geological debates for reasons pretty similar to those at work in the climate controversy. And these developments are in addition quite proper in a discussion of the irrelevance of the notion of "consensus" that is central in the climate debate,

2. *The author does not show a published track record in climate sciences, ice core geochemistry, or atmospheric sciences and no relations to major (French) climate research groups (e.g., IPSL or CNRM).*

- If Copernicus were endorsing this statement, then for consistency reasons this organization should change its name. May I recall that Copernicus, the man, was a Church administrator, known for his Treatise on Money (1526), who never published on astronomy before expounding his revolutionary system of the world? Or that, more recently, continental drift was the idea of a meteorologist, Alfred Wegener? My viewpoint is that of a geochemist, thermodynamicist and historian/philosopher of science. I'll thus turn the argument the other way round to ask whether such new outlook was a missing ingredient to make correct sense of the ice-core data? If I were a climatologist, I would

very likely follow the well-worn track of the field. As for the last remark, let me remind that I have made clear calls for "friendly" discussions with my colleagues, which don't seem to have been well received at all.

3. *The author describes their work as an epistemological approach focusing on "fundamental principles without the need for delving into technical details", hinting that the work represents either an opinion or review.*

- This conclusion about "opinion" or "review" is fundamentally wrong. Would a study in logics be a simple "opinion" or "review"? Showing that the interpretation of ice-core results flatly contradicts the fundamental principles of scientific reasoning is not an opinion, but a demonstration. I guess myself that it is the very reason why none of my opponents has been able to refute my reasoning yet.

4. *Interpreted as a review, the author frequently mentions ice cores and climate models but, apart from citing a single reference core, does not discuss any recent authoritative publications in either field of science.*

- The paper is not at all a comprehensive review of ice-core science. As explained, only Vostok data have been considered because they have played so major a role in the climate debate that they have "become a compelling target against which other records and modeling efforts are tested". Not only has this point still be stressed very recently by J. Jouzel himself, but I warned myself that my analysis would be incorrect if the ice record were "shown to be fundamentally misleading". This has not been the case yet.

5. *Far-reaching conclusions are derived from analysing the single ice core while none of multiple works on time–depth or CO₂–temperature relations on the same or other cores are discussed.*

- As very well explained long ago by Popper, a single counter-example logically suffices to demonstrate that a theory is wrong. If the assumed CO₂ forcing is clearly contradicted by the Vostok analyses, then it follows that it is not needed to consider other ice cores.

6. *A central argument is made that current climate models are based on false premises, while likewise, no or unspecific reference is given to the vast literature on these models (e.g., the considerable record on CMIP5/CMIP6).*

- As long as all these models are also relying on CO₂ forcing, why would it be useful to consider them as long as the present demonstration is not refuted?