

Review of the Omta's manuscript (submitted to CP) :

« Differences between the glacial cycles of Antarctic temperature and greenhouse gases »

First, I should mention that I was reviewer of a previous submitted version of this paper to another Journal. I had at this time some major criticisms related with the analysis made of the ice core data used, the approach for decomposing the temperature signal into an eccentricity component related to CO<sub>2</sub> and an obliquity signal, and the way the author invalidated hypothesis that CO<sub>2</sub> is follower and amplifier of temperature. I concluded that the paper, which presented neither new data nor convincing reanalyse and explanations, was in my point of view confusing, not scientifically sound and of no help for better understanding forcings and feedbacks involved in glacial-interglacial cycles.

Although the present manuscript has been partly restructured and shows some changes compared to the previous one, I have still more or less the same remarks.

Nevertheless, instead to develop in details my comments here and because I already read the comments of the 2 first referees as well as the answers of A.W. Omta (a benefit of a discussion paper!), I feel more useful to comment on the two questions proposed by Omta in his answer to referee #1.

1. What is the key point in this manuscript?

According to the author it is:

Subtracting the ice core CO<sub>2</sub> signal from the ice core  $\delta$ D time series (taken as a proxy of Antarctic temperature) after rescaling both signals with their respective standard deviations yields a residual similar to the rescaled obliquity cycle. This directly implies that temperature is approximately a linear combination of CO<sub>2</sub> and obliquity. After A.W. Omta, this has not been pointed out elsewhere.

To my point of view the key point of a paper, based on spectral analyses and decomposition of time series in components characterized with signatures of different frequency domains, should be thoroughly and quantitatively discussed with an evaluation of the uncertainties and by using and comparing several methodological approaches. For instance, why not filter the  $\delta$ D signal around the 41 ka frequency band to isolate the response of Antarctic temperature to obliquity variations? And if applicable, how such filtering will compare with the method used by Omta? In short, I believe that the methodological approach, on which the key point of the paper is based, is not seriously validated.

2. Why is this an important finding?

According to the Author, it is because a key question regarding the glacial cycles is the causal relationship between CO<sub>2</sub> and Antarctic Temperature (AT). My understanding is that A.W. Omta discusses two types of scenario: (1) CO<sub>2</sub> primarily responds to and amplifies Antarctic temperature and (2) Antarctic temperature and CO<sub>2</sub> independently respond to a 100 ka cycle of another variable (which one?) or there exists a 100kyr biogeochemical oscillation of CO<sub>2</sub> to which Antarctic temperature responds. On top of that, temperature responds to obliquity variations.

These two scenarios have already been discussed in the past and quite recently in different papers, and several mechanisms have been proposed to explain the phase relationship between temperature and CO<sub>2</sub>. It is still an important open question, but honestly it is difficult to believe, in the absence of a detailed discussion and modelling approaches, that only because AT would be a linear combination of CO<sub>2</sub> and obliquity, we will be able to solve or even to constrain the causal relationship between CO<sub>2</sub> and AT.. A much more elaborated approach is needed taking into account, for instance:

- the full dynamics of the CO<sub>2</sub>-AT relationship through the glacial-interglacial cycles (Transitions, onset of glaciations,...; for instance by analysing continuous wavelet transforms of the signal)
- the regional character of AT evolution
- the full spectrum of forcings and feedbacks

Also a detailed analysis of the phase relationship between CO<sub>2</sub> and temperature, based on paleo-data is definitely necessary in order to constrain the causal relationship between CO<sub>2</sub> and temperature during the glacial-interglacial cycles

To summarize, the key point of this work is not adequately validated, and even if it was, the simple exercise presented in the submitted paper doesn't appear to be an important finding in itself to constrain the CO<sub>2</sub>-temperature connection during glacial-interglacial cycles. I still feel that that this paper is not scientifically sound and of no real help for better understanding forcings and feedbacks involved in glacial-interglacial cycles. My recommendation is to reject the manuscript for publication in CP.

