

## ***Interactive comment on “Rapid coupling of Antarctic temperature and atmospheric CO<sub>2</sub> during deglaciation” by J. B. Pedro et al.***

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In this manuscript, Pedro et al. revise the estimated phasing between Antarctic temperature ( $T_{proxy}$ ) and CO<sub>2</sub> during the last deglaciation (or TI) based on data from coastal Antarctic sites.

They find that the average lag of CO<sub>2</sub> vs AT was only of 0 to 400 yr on average during TI, a significant revision with respect to earlier studies which suggested a larger lag. Previous evaluations were based on firn densification models experiments applied on the EDC or Vostok ice cores which have, in summary, been proven wrong (Louergue et al., CP, 2007). So it is certainly time to revise these previous lag evaluations which probably induce wrong interpretations of C-cycle mechanisms in our community.

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I however have technical remarks and questions concerning the method, which, I hope, will help to clarify the manuscript. I will not comment the discussion part.

Some remarks and questions concern Pedro et al. (CP, 2011) which is pivotal in the current study and which I had no opportunity to comment at the time of its publication.

- My first comments will be more easily understandable when looking at fig. 1 of the above mentioned study. The Law Dome isotopic record has been used to construct the stack. However, this isotopic record does not resemble the classical 'East Antarctic plateau' scenario. In particular, the timing of the beginning of the Holocene is unclear, since there is a warming trend during the Early Holocene which cannot be seen in other ice cores. I am not convinced that this isotopic trend is representative of Antarctic climate, even at a regional scale. I am not even convinced it is a climatic trend, it could well be due to glaciological artefacts (changes of ice sheet thickness, etc.). Therefore, the article misses an argument as to why LD is really improving the stacked record.

- Similarly, the Siple Dome isotopic record is quite different from the classical 'East Antarctic plateau' scenario, but this time the difference is more striking for the first part of the deglaciation (before the onset of the BA). Here, the difference might well come partly from inaccuracy in the dating. I am not convinced that the observed lag between Siple and the stack is due to different climatic scenarios. Indeed, the synchronisation of ice cores in Pedro et al. (CP, 2011) is based on CH<sub>4</sub>, but CH<sub>4</sub> does not present fast variations at the onset of TI. Also, Siple isotopic record presents a sharp event at 22 kyr BP (which cannot be seen of fig. 1 of Pedro et al.) which makes in my opinion the quantitative use of this record dubious for TI. Again, it is not clear at first glance that the use of Siple is improving the stacked record.

- Byrd isotopic record also presents a different scenario at the onset of TI which again,

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may not be representative of Antarctic climate. Again, it is not clear that Byrd isotopic record is improving the stacked record.

- fig. 2 shows that there is one CH<sub>4</sub> tie point at 16 kyr BP for the construction of the LD chronology. At this time, there is no fast transition in the CH<sub>4</sub> records, only a weak slope that extends from 17.5 kyr BP to 14.6 kyr BP. Personally I evaluate the  $2\sigma$  uncertainty of such tie point as half the duration of the transition. In this case, this would give 1.5 kyr. Looking at table 2,  $\sigma_{correl}$  has been evaluated as 0.3 kyr (I could not find in the article how this uncertainty has been evaluated). I find this estimate too optimistic, also given the fact that the CH<sub>4</sub> records from LD and GISP2 do not actually correlate so well during this time interval.

- In between the CH<sub>4</sub> tie points, the LD chronology relies on glaciological interpolation. The uncertainty attached to this interpolation should be quantified. It certainly increases as the distance to the nearest tie points increases.

Now coming to the new submitted article:

- The manuscript refers to previous studies for the quantitative determination of the lag, so that the method is unclear at first glance. I think it is important to write in details the method in the current manuscript since it is so pivotal. In particular, it is not clear to me how the 'methodological' uncertainty for the lag is evaluated. Explicitly writing a likelihood function of the lag would help a lot the reader.

- If I understand correctly, the authors use a simple linear model for the determination of the average lag over TI:  $CO_2(t+lag) = T_{proxy}(t)$ , where CO<sub>2</sub> and  $T_{proxy}$  have been normalized to unit variance and zero mean. If so, this should be explicitly stated in the manuscript.

- If I understand correctly, the authors took into account the uncertainties in the CO<sub>2</sub> and  $T_{proxy}$  measurements, but NOT the uncertainty in their model. If so, this would be a severe flaw which would considerably underestimate the 'methodological' uncertainty of the lag evaluation. The total uncertainty (data and model) is usually evaluated from

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the residuals between the data and the model (in this case  $CO_2(t+lag) - T_{proxy}(t)$ ). The total errors (data and model) are certainly not independent so that a covariance matrix should be used.

- The advantage of using the Byrd and Siple ice cores is that they have a well constrained gas/ice offset because of their high accumulation rates. However, in the present study, the authors treat independently the gas and ice records, both being synchronized onto GICC05. It is thus not clear at first glance that the advantage of using high accumulation ice cores is preserved.

- If the CO<sub>2</sub> and  $T_{proxy}$  age scale are indeed treated independently, the error on the CH<sub>4</sub> synchronisation,  $\sigma_{sync}$  and the error on  $\delta age$ ,  $\sigma_{age}$ , should be counted twice, since they impact both the  $T_{proxy}$  and CO<sub>2</sub> age scales. If this is not the case (as this seems to be suggested p. 627, l. 6-8), the method should be presented in a different way. A (in my opinion) better presentation would be: 1) construction of ice age scales for the Antarctic ice cores based on GICC05 and 2) construction of gas age scales from the ice age scales based on firn densification modelling. This way, we are that the ice and gas age scales are consistent.

- Especially important for the onset of TI, where there are no CH<sub>4</sub> tie points is the fact that the interpolation relies on glaciological models. Typically, if one assumes that the  $1\sigma$  uncertainty on event durations from glaciological models is 10- p. 624, l. 24-26: you are transferring gas records from GRIP to NGRIP using an ice synchronisation. Doing so, you assume that  $\Delta age$  as a function of the age is the same for GRIP and NGRIP. This should be explicitly stated and the error associated with this assumption should be evaluated. Same remark for the Siple CO<sub>2</sub> record which has been transferred from GISP2 to NGRIP via ice synchronisation.

- p. 625, l. 17: why the data need to be smoothed? Why not using the raw data? Specifically, the uncertainties of the measurements are defined for the raw data, not for a smoothed version.

- p. 626, l. 8: why the period younger than 11.5 kyr is excluded? This is actually where things get interesting! Indeed, if you restrict your study to one interval where both

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CO<sub>2</sub> and  $T_{proxy}$  increase linearly, the lag is not constrained anymore! (you will find a correlation coefficient of 1 whatever the lag) It is actually the break points, where the slope of  $T_{proxy}$  and CO<sub>2</sub> change, that allow to constrain the lag. Why do you think 'different processes are responsible'? The same processes could well act also during the remaining of TI.

- p. 627, l. 24-26: How do you combine the means and standard deviations of Fig. 2b? Do you take an average? (if so, what is the rational behind that?) Or do you assume these are independent estimates of the lag? (which is obviously not the case since they are based on the same datasets)

- p. 627, l. 10-11: Where the 200 years uncertainty comes from? The evaluation of uncertainties should be as mathematical, precise and as objective as possible. By the way is it a  $1 \sigma$  or a  $2 \sigma$  uncertainty?

- same question for the 0-400 yr lag estimate. In p. 626, l. 26, you give a 'best guess' value for the lag of 163 yr. Now the middle of the 0-400 interval is 200 yr. Why this difference?

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