In this manuscript, Jessica Ng et al. present a new marine dust record near South America which could be used as a target to date the forthcoming Oldest Ice ice core dust records from Antarctica. They show that their new U1537 marine dust record differs significantly from the published ODP 1090 dust record for the MPT and pre-MPT periods, 1.5-0.8 Myr ago. They argue their new site is better since the correlation with the LR04 stack stays more or less constant in the pre-MPT period with respect to the post-MPT period, contrary to the ODP 1090 record. They argue that it is possible to measure the dust record in Antarctica by logging the ice borehole, and they present such a new borehole dust logging from EPICA Dome C. They then show simple tuning strategies to quickly come-up with a time scale once the new ice dust record is available.

The paper is generally focused and straightforward, yet important, so it was a pleasure for me to review it.

Thank you for this assessment of the paper's readability and significance and for your helpful suggestions.

I find the new dust record from U1537 very interesting. We can discuss if it is really better than ODP1090 (personnally I am quite convinced), but at the very least it is an alternative record which shows that ODP1090 should be taken with caution.

I do agree with Eric Wolff that the new borehole dust record from EPICA Dome C could get a bit more attention if it is really the first time such record is published. A quick comparison with the Coulter and lazer records would be interesting.

We agree with this emphasis on the new EPICA Dome C record and will add a comparison with the Coulter and laser records.

Regarding the end of the manuscript with the tuning strategy, I think there should be more powerful strategies, this is only a first-step strategy (but I think the authors are honnests in presenting it this way). And I do agree with Eric Wolff that this part is not as well presented and straightforward as the other parts. For example, the authors use a Nye ice flow model, but never explicitely describe the parameters they used, while for example the melting is a primary parameter for dating the old section of an ice core.

I would suggest to use the 1D model used in, e.g., Parrenin et al. (TC, 2017), Lilien et al. (TC, 2021) and Chung et al. (TC, in press), which uses a Lliboutry velocity profile. This model has an analytical thinning function and accounts for temporal variations of accumulation through a simple change of time variable. It should give a far better accuracy, while still being very easy to implement. I do not make a strong requirement to use this model, but I think it would be an improvement and I can provide guidance on request.

We appreciate the suggestion to use a more complex model; however, our approach is to keep things as simple and transparent as possible so that an estimate of the basal age of the ice can be made quickly in the field. We will describe the parameters used in the Nye model and revise the

section for clarity. We will also emphasize that we are presenting a first step strategy and that other more complex methods will need to be used to establish a full chronology.

Moreover, the strategy is presented as decoupled between the modelling and the tuning, while I think both should be coupled: one first tune the top part, then apply the model with these dating constrains to extrapolate, then one tune the following part, etc. I personnally think the best approach would be to adjust the glaciological parameters of an ice core dating model in a Bayesian code like IceChrono/Paleochrono so as to optimize the fit with several targets, using a powerful MonteCarlo approach. (Such a method has been presented in the PhD of Jai Beeman in 2019, but unfortunately it has never been published elsewhere).

Our strategy is meant for rapid dating applications that could be used in the field to estimate the basal age of the ice, so we have kept it as simple as possible. Of course other more complex approaches such as described here should be used to establish a more accurate chronology later.

Minor comments:

- L. 40-45: The discussion of gradual vs abrupt MPT, as presented in Legrain et al. (2023) would fit nicely in this introduction, but I let you decide.
- We will add this reference to the introduction.
- L. ~50: In my opinion, the best evaluation of the age and state of basal ice in the Dome C area is from Chung et al. (TC, in press), but I let you decide if you want to cite it.
- We will add this reference.
- L. 96: If I am correct, DFO2006 is the O2/N2 age scale of the first Dome Fuji ice core, DF1. The age scale from Dome Fuji members (2017) is DFO2006, then extended using AICC2012 for DF2. Please check, I don't think this age scale has a proper name, but I would call it DFO2006+AICC2012.
- We have changed the age scale name to DFO2006+AICC2012.
- L. ~120: In Table 1, the correlation coefficients are given for the ice core dust records, not for their log. The coeffs for the log-records are given a bit later in the manuscript, but not in Table 1. I personnally think the coeffs for the log-records are more relevant than for the raw records and I would put them in Table 1.
- We find the comparison of the original unaltered records to be useful and will consider showing correlation coefficients for both the records and the log-records in Table 1.