Reviewer 1

This paper is a rather straightforward extension of previous work that suggested using dust records to date new, old, ice cores. For complete transparency, I was the lead author of that previous paper.

The most important addition of this paper is comparison with a new marine dust record which shows a different pattern in the pre-MPT era and therefore suggests a different template to that proposed so far. This is certainly worth pointing out and the authors make a good, though not cast-iron, case that the new record would be a better template. The second addition is the use of dust data obtained by downhole optical logging, opening up the possibility to date ice rapidly and before a full drilling campaign. This again seems valuable, and is perhaps underplayed in this paper. Finally the authors discuss how the dust templates might be used in practice – this section is less insightful, and quite hard to follow. Taking the paper as a whole, it is worthwhile (and I thank the authors for keeping it short in proportion to its findings), but does need some clarifications and minor additions.

Thank you for your thoughtful assessment. We have further emphasized the rapid dating application of our approach where appropriate throughout the paper and revised the section on using the dust templates for clarity.

Larger issues:
Line 135-140. Clearly the two marine dust records do diverge considerably before 800 ka. While you present reasons why 1537 should be the better template, I'd like to see a more subtle discussion. I think one could equally make the argument the other way, that 1090 more obviously integrates dust emissions from across South America, while 1537 is likely only to see the southernmost Patagonian emissions. Dust geochemistry does suggest a preponderance of Patagonian emissions in EDC dust, but it is not really clearcut, and who knows if this is true before 800 ka? I am not arguing for a major change but I think it would be better to leave it slightly open as to which dust record is the better template and let us decide once we have the ice! I do think you could though make more of the fact that 1537 is much better resolved (I think ~200 m of core for 1.5Ma cf 40 m at 1090).

We appreciate this nuance and have reworked the section accordingly.

Line 141-9. I think it is a bit dangerous to imply that you expect LR04 to look like dust. The point about using dust records as a template (and other records we might choose as templates) is that there is a good theoretical reason (same source and transport pathways) to expect them to look the same under most circumstances. This was our justification for proposing extending the match beyond 800 ka (even if 1537 suggests that may also be tricky). There is no similar theoretical reason (other than that all records show glacial cycles) to expect LR04 to look similar, or to propose that the match should extend into the 40 ka world, and using it as a template would be circular reasoning when we want to use the ice record to understand climate. By all means point out the similarities but then I would strongly recommend not using LR04 any further and not recommending that it forms any part of a template. In particular I would not show it in Fig 4.

This is a good point, although LR04 has already been used to establish the U1537 chronology (along with biostratigraphy and magnetostratigraphy). We have removed LR04 from Fig 4 and most of the analysis, and we have explicitly stated that why we don't use LR04 further in Lines 196-198.

Optical dust record, section 4.1 and Fig 5. To my knowledge this is the first time the EDC optical dust record has been shown, so you need to do more to show that it matches the laser dust record. Before playing with age scales, you should show both on a depth scale where there is no alignment issue. I suggest adding a figure where you show the whole record and some detailed parts so the reader can judge to what extent the optical record captures both the shape and amplitude of dust peaks. (Minor point: if this is indeed the first outing for these data then there should be an acknowledgment to EPICA!). In addition to this Fig 5 is confusing: panel a and b say they are the optical log but they have a colour that says (legend) they are laser dust. Please alter this.

We have revised Figure 5 and Section 4.1 to focus on the optical dust log. We apologize for the confusing legend in the previous draft.

Section 4.2 and Fig 6. I have now read this several ties and I'm afraid I can't understand what you have done. "such that the peaks in the artificial record (Fig 6c top) appeared older than they originally were". I just can't see in the figure what it is you claim to have done, or where the supposed mismatches are. You seem to have exactly the same peaks in exactly the same places. You appear to be suggesting that the peak at 1050 ka is displaced by 200 ka, but I am not seeing it. I imagine I have misunderstood the figure but I think others will too, so please make a new attempt to explain this perhaps highlighting using the curves in b and c exactly what the mismatches are.

We have reworked Section 4.2 and Fig 7 (previously Fig 6) for clarity with additional suggestions from reviewer Lorraine Lisiecki.

Honestly I don't find section 4.2 very useful or enlightening, but I think this is because I am imagining a situation where we have a core with multiple dating aids (gases with insolation cycles, dust, 10Be etc), while you are considering the case of a raid access hole with only the dust record available. I think the more likely problem for a core is not that peaks in good order are misidentified: the issue is how to know whether there has been folding putting peaks in the ice in the wrong order, and this is not addressed here. Maybe this could be mentioned as a potential hazard!

Our approach is really intended for rapid dating in the field using only the dust record, as you note. We have emphasized this application in the introduction and where appropriate throughout the manuscript (L87, 262-264). Per your and Lorraine Lisiecki's suggestion, we have addressed folding with a new artificial record and discussion in Section 5 (L364-366, 635-640).

Detailed comments

Line 12. I am not sure why "surprisingly" is used. At EDC at least there is melting at the bed so it's not a surprise that it's not so old. Perhaps just remove this word unless you had something specific in mind.

Removed "surprisingly."

Line 10 and 47. Sorry to be picky but IPICS is International Partnerships (plural) in Ice Core Sciences.

Corrected.

Line 90 (also 106). It's a shame you are using the very old EDC3 age model, especially as you use the AICC2012 alignment for Dome Fuji. I appreciate that ODP1090 was compared to an EDC3 age model as that was still the standard in 2012, but it should be quite straightforward to translate both EDC and EDC1090 to AICC2012, thus removing one unintended source of minor mismatch between the records you use. If you do stay with EDC3 perhaps you need to add a line pointing out that there are minor differences between EDC3 and AICC2012, but that they don't affect the pattern of glacial cycles being used in the template.

We have now put EDC and ODP1090 on the AICC2012 chronology.

Line 113: "the aridity (due to temperature) and circulation of the atmosphere, which influences the production and transport of dust". I suggest adding "and atmospheric lifetime". This is probably more important than the transport itself.

Added "and atmospheric lifetime."

Section 2.4, Table 1, etc. You use dust itself in 1090, whereas our previous paper used Fe_MAR. I think your dust record is OK, and it seems to give a good result in the last 800 kyr, and a very similar pattern before that. But it would be good if you just point this difference out so people can understand why the units are a little different. Similarly (and I know you have discussed this) it might need an extra line in 2.3 to explain that MS has been empirically shown to be more like EDC and ODP1090 than what might seem more direct measures of dust.

We have clarified these details, thank you for pointing them out.

Line 155. Of course the reason why the log records work better is because the dynamic range of the different records is different. (I think less so for EDC and 1537 but still the principle holds). Add a few words?

We have added a brief comment on the log records.

Fig. 5 – see comments above about the colours in panels a and b, which are confusing. In addition it should not go beyond 800 ka in panels b and c – this is as far as the EPICA age scale goes and beyond that it is assumed that the ice is disturbed and not necessarily in age order.

We apologize for the error in the legend colors that caused confusion. We have changed this figure to focus on the optical dust log and comparison to the laser dust data.

Lines 185-6. I found this confusing, and couldn't quite work out what you did (especially what you mean by "scaling the smoothed record by random factors between 0.4 - 0.7 linearly interpolated between 500 kyr intervals"), although I understand the intention. Please spell it out more clearly. I wonder why you smoothed with a 20 kyr running mean – this means that by design you have taken out some of the multimillennial features that might have been used to identify the correct peaks to tie records together.

We have revised this section for clarity, as well as implemented suggestions from Lorraine Lisiecki to reduce the amplitude of the artificial record farther back in time and compare multiple versions of an artificial record. The thought behind smoothing with a 20 kyr running mean was that some lower amplitude millennial scale features that are recorded in marine sediments may not make it to the ice sheet. We now include multiple artificial records with different modifications to U1537.

Discussion: Last para of discussion, you might add some thoughts about dating with multiple datasets in a full core and about the hazards of folding, as per my earlier comment.

See response to earlier comment.

Line 265. I'm sorry but I think the optical dust record needs to be made publicly available, not just on request, to meet the journal rules.

We will make the optical dust record available in a data repository.

Reviewer 2

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 have added a comparison with the laser record that we use throughout the paper (but not the Coulter data for simplicity).

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 have described the parameters used in the Nye model (L371-374) and revised the section for clarity. We 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 (L262-264).

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 added 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 added 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 (not logarithmic) records to be useful. We considered showing correlation coefficients for both the records and the log-records in Table 1, but we struggled to find a visually succinct way to do that. We have kept the raw records in Table 1 and the log-records in Table 2.

Reviewer 3

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