- **RC2**: 'Comment on cp-2021-37', Anonymous Referee #2, 06 Sep 2021

General comments:

This manuscript describes the improved method for reconstructing Antarctic temperature based on ice core water isotope record. Although the manuscript is very long (70 pages), it is organized well and easy to read. The previous temperature reconstruction methods based on the Rayleigh-type model has been properly improved and many potential uncertainties and/or assumptions (closure assumption, inversion temperature, mixing of air mass etc.) are carefully evaluated. Overall, this manuscript is very interesting and suitable for publication in Climate of the Past. To improve the manuscript, I made some comments below.

Thank you for this helpful review!

Major comments

(1) Abstract "..However, there are important nonlinearities that significantly affect such reconstrugion..Here, we describe a temperature reconstruction method that account for these nonlinearities.."

I think the Abstract (and main text) overemphasizes only the difference between linear and non-linerar reconstructions. The difference between this and previous studies results from not only linear/non-linear technique but also different settings (evaporation, supersaturation, etc.) used for isotope modelling. I think the important contribution of this study is the careful examination of various factors one by one, which surely improve the understanding of uncertainty of several important assumptions. I think it is better to write this point in the abstract. In fact, the authors themselves noted that "The difference between the results of this study and the previous temperature reconstructions arise from differences between the linear and nonlinear reconstruction technique as well as differences in the underlying water-isotope models used for the estimation of scaling relationship".

Thank you for this comment. This is an important point. We have updated the abstract. We have attempted to address this issue directly by also conducting linear and non-linear reconstructions within just our model (Figure 9 in the original submission). Our new abstract reads:

"Oxygen and hydrogen isotope ratios in polar precipitation are widely used as proxies for local temperature. In combination, oxygen and hydrogen isotope ratios also provide information on sea surface temperature at the oceanic moisture source locations where polar precipitation originates. Temperature reconstructions obtained from ice core records generally rely on linear approximations of the relationships among local temperature, source temperature and water-isotope values. However, there are important nonlinearities that significantly affect such reconstructions, particularly for source-region temperatures. Here, we describe a relatively simple water isotope distillation model and a novel temperature reconstruction method that accounts for these nonlinearities. Further, we examine in detail many of the parameters, assumptions, and uncertainties that underly water isotope distillation models and their influence on these temperature reconstructions. We provide new reconstructions of absolute surface temperature, condensation temperature, and source-region evaporation temperature for all long Antarctic ice-core records for which the necessary data are available. These reconstructions differ from previous estimates due both to our new model and reconstruction technique, the influence of which is investigated directly. We also provide thorough uncertainty estimates on all temperature histories. Our reconstructions constrain the pattern and magnitude of polar amplification in the past and reveal asymmetries in the temperature histories of East and West Antarctica

(2) Evaporation from the ocean (Appendix A2.1).

P27 L15 ".. this "local" closure assumption.. "

> Since Merlivat and Jouzel (1979) assumed a global steady state of water cycle, the assumption (Rv=Re) has been commonly referred to as "global closure assumption". But, here, the authors termed this as "local" closure assumption. To avoid unnecessarily confusions, it is better to add short explanations about different terminology.

We believe the terminology used here is in line with the literature (e.g. Risi et al 2010, Pfahl and Sodemann 2014, and based on Merlivat and Jouzel 1979 and following Criss 1999. See also Landais et al 2008 and Barkan and Luz 2007). The global closure assumption, as we understand it, requires assuming a global steady state between evaporation and precipitation (whose average delta value is known), which then allows one to estimate R_e globally, from alpha_evap = R_o/R_e. By contrast, the “local closure” assumption as used here, assumes that local evaporation is the only source of the vapor in question (R_v = R_e; similar to the assumption used in the experiments of Barkan and Luz 2007). Equations 9 and 10 from Merlivat and Jouzel (1979) lead one to either of these closure solutions, depending on what is assumed about R_e and whether one closes the water cycle for the global average, or assumes it is closed locally, hence our terminology. Our terminology is described in detail in Section A2.1.

Here is an excerpt from Pfahl and Sodemann 2014: “Merlivat and Jouzel (1979) introduced a global closure assumption in which the isotopic composition of the surrounding vapour δ^i_v was assumed to be equal to the isotopic composition of global precipitation, which in turn equals the isotopic composition of global evaporation.” We do not believe that the assumption described here is the same as equating R_v to R_e (an assumption of local closure), but is properly termed the global closure assumption.

By the way, this section (A2.1) is interesting and includes important analyses. In fact, the different assumptions affect the T reconstructions (Fig A21). So, I think this section, at least some part, may be moved to main text.

Thank you. In order to keep the main text as easily-followable as possible. We prefer not to move this entire section to the main text. However, we appreciate the suggestion and have included mention of some of the most important findings in the main text.

(3) P20 L1-6 "We find smaller glacial-interglacial temperature change for East Antarctic sites compared to previous reconstructions .. The average warming at the two highest sites however, Dome Fuji and Vostok, is significantly less, just 6.9 degC or 59% of that at the lower sites."

This incomplete quotation of our text splices two very separate ideas: 1) comparison of our reconstructions to previous reconstructions, 2) comparison of temperature variability at different sites within our self-consistent temperature reconstructions.

> Previous T reconstructions at DF and Vostok are 7.5-7.8 deg C (e.g., Vimeux et al. EPSL 2002, Uemura et al., CP 2012) (depending on time intervals you choose). Thus, the difference between previous and reconstructions is only 0.6-0.9 degC, which is not significant. For objective comparison, it is better to describe the exact differences from past reconstructions and add short descriptions.

The pervious reconstructions show changes of site temperature up to 18% larger than our reconstructions for the same time intervals. We have included this description of the differences in the text. Moreover, we appreciate the suggestion of describing exact difference and have updated and moved a figure into the main text (previously in the supplement) to make this point clearer.

The “significantly less” language in the manuscript the reviewer quoted is comparing the temperature change at DF and Vostok to the temperature change at Siple Dome and WAIS, within our own self consistent temperature reconstructions. These differences are indeed significant. This has been made more clear in the updated text.

(4) P53. L11-13 "such as the value of the diffusive fractionation factors during transport.."

Does your model include the eddy diffusive fractionation during transport, like described in Hendrick et al. (GBC, 2000)? If so, please describe it in the Appendix.

Hendricks et al., GBC, https://doi.org/10.1029/1999GB001198

This is a great question. The diffusive fractionation factors mentioned in that line are described in Section A2.2, and refer to moisture diffusion during precipitation formation. The eddy diffusion described in Hendricks et al 2000 (and the fractionation associated with it) relate to their formulation of global moisture transport which they break into (1 dimensional) eddy-driven diffusive mixing and large-scale advection (and whose scheme was later updated by Kavanaugh and Cuffey, 2002, 2003). Their model framework considers moisture transport (and fractionation) on a spatial latitude gid. Our model construction does not; our grid variable is temperature (whose mapping to latitude we do not assume). We assume moisture transport to be pseudo-adiabatic, giving a pressure dimension to our moisture transport (see Figure 2). We then consider the influence of non-adiabatic mixing in modifying the total fractionation, a relaxation of the pseudo-adiabatic assumption of large-scale moisture transport (Section A5). In this way our model construction addresses similar key concepts to those addressed by Hendricks et al (large scale transport and mixing), but using a different framework.

(5) Difference between delta-age based temperature.

A recent paper by Buizert et al. (2021) claimed that Antarctic temperature during LGM is less than those estimated with the water isotopes. And they suggest that the difference can be attributable to an altered Antarctic temperature inversion during the LGM. This finding is very closely related to the topic of this manuscript. Maybe this is beyond the scope of this paper. I would like to ask about some comments about the differences between this and Buizert et al. (2021) (actually, the second author of this manuscript is a coauthor of Buizert et al. 2021).

Buizert et al. (2021) Antarctic surface temperature and elevation during the Last Glacial Maximum, Science, 10.1126/science.abd2897

The paper mentioned here was published several months after the submission of our manuscript. We have added reference to this important paper in the main text, but comment only briefly on the differences. There are many possible sources of difference between the temperature reconstruction of the Buizert et al paper and those presented here, including potentially large uncertainties in firn modeling and the fact that surface air temperature (the target of reconstruction of this paper) and surface ice temperature (the target of reconstruction of the Buizert et al paper) are necessarily different given atmospheric energy balance. Buizert et al. speculate, for example, that changes in the inversion strength can readily explain differences between our results, but this is speculative and not easily addressed. We discuss these comparisons on Pg 21 of the updated manuscript. A complete comparison of these techniques is beyond the scope of the present study.

Specific comments

Figure 5 left panels > I don not see each profile corresponds to which ice core. Please add appropriate legends in left panels.

We have updated this figure for clarity.

Figure 8 Legends > Please sort in order of condensation temperature, to make it easier to compare with color profiles in panel c and d.

Updated. Thank you for this suggestion.

Figure 9 panel a and b > I don not see each profile corresponds to which ice core.

Yes, this is true. The point of this figure is not to clearly these differences for each core, rather it is intended to show the range of differences for the reconstruction techniques. For compactness we have left this figure as is, however we have added an additional figure to the supplement that shows the difference for each core more clearly.

P8 L5-6 ".. and the changes in slopes across the parameter space are larger than these local changes.."

>I do not understand this sentence.

Thank you for this comment as we should clarify this point. For temperatures near T_c = -30degC, there is a notable change in the partial derivatives of isotope parameters and T_c. However the total change in these partial derivates from T_c = 0 to T_c = -65 deg C, are even larger than those changes localized around -30 deg C. We have updated the text to make this more clear.

P17 L3. " .. colder temperature then previous studies.."

> than

Thank you! Corrected.

P21 L21-28 "There is a long-standing debate regarding the interpretation of "spatial" and "temporal .."

> Since this topic is not discussed in the main text, it seems strange to suddenly come up with a conclusion. How about moving this topic to the introduction?

Thank you for this suggestion. We have done so.

P36 L31. ".. that condensation occurs at aa range of temperature up to.."

> a

Corrected, thank you!

Figure A23 > Colored profiles cannot be recognized.

We have attempted to make this more clear. However, please note that the differences are very small, particularly compared to sample-to-sample variability, thus they may be difficult to see regardless of color profile. Indeed this is part of the point of the figure. We have made this more clear in the text.

Citation: https://doi.org/10.5194/cp-2021-37-RC2