

Interactive comment on “Physically based summer temperature reconstruction from ice layers in ice cores” by Koji Fujita et al.

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Dear Reviewer #1

Thank you for your comments on our manuscript submitted to CP.

[reviewer comment] The manuscript entitled “Physically based summer temperature reconstruction from ice layers in ice cores” by Fujita et al. presents further understanding on the method to reconstruct summer temperature from ice layer thickness using an energy balance model. Given the complex interpretations of ice core stable isotopic record as a temperature indicator, a Physically based temperature parameter holds its merits for publication.

[author reply] Thanks for the positive evaluation.

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[reviewer comment] However, I concerned much about the feasibility of the method presented here. As indicated by the authors, applicable range of this method is likely from $-6 \text{ }^{\circ}\text{C}$ to $+1 \text{ }^{\circ}\text{C}$, while uncertainty in the reconstructed SMT is comparatively high. Errors derived from the density assumption and from the seasonal pattern range from 0.04 to 0.15 $^{\circ}\text{C}$, and from 0.78 to 1.57 $^{\circ}\text{C}$, respectively (Table S2). The situation might worsen when considering uncertainty with the density assumption and the seasonal pattern. Especially the firn densification model requires prior parameters on temperature and accumulation.

[author reply] We think that this method has good feasibility for SMT reconstruction; this is because, the reconstructed temperatures showed large inter-annual variability exceeding error range except for SE-Dome site (Fig. 7). In addition, as we wrote in the introduction, a few studies have reconstructed temperature from melt features in ice cores while the other studies described just warmer/cooler. Further, those previous studies have rarely presented error range of their estimates. So, we believe that it is valuable to provide the error ranges itself even if they are large. We also believe that it is valuable to provide alternative information with this "independent method" even if its applicable temperature range is narrow.

Because meltwater refreezing occurs in the first annual layer, we think that the uncertainty of density assumption in the current manuscript is reasonable (350 to 550 kg m^{-3} , L168). Although meltwater can penetrate into deeper firn layer in the model, we do not assume such "internal accumulation" in this study (see L321). Even if the density range was twice greater than the current range (250 to 750 kg m^{-3}), the error range would be still less than that from seasonal pattern (0.08 to 0.30 degC , <20% of that due to seasonal pattern).

The greater error range due to seasonal pattern suggests that the linear relationship between melt feature and neighboring instrumental temperature, which has been used in the previous studies, would not work. We will add this assertion around L340.

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Although the reviewer addressed "might worsen", uncertainty due to the seasonal pattern is estimated from the 35-year patterns so that uncertainty of the variability is already considered.

We do not catch what the last sentence means. Density profile by the densification model is not used for conversion from ice layer thickness to refreezing amount but used for temperature conduction. So, the densification scheme does not affect the density assumption for the conversion.

[reviewer comment] Ideal assumptions bear upon the energy balance model. How much uncertainty can these assumptions can bring about? Some assumptions require further confirmation, for instance, the authors assume that each snow layer can retain water with a volume content, and the exceeded water percolates into the next lower layer. The volume content might be partly dependent on the snow temperature distribution and ice layers.

[author reply] It is unfortunate that the reviewer describes just "some assumptions" without providing specific parameters. Anyway, we perform additional sensitivity test for parameters shown in Table S1. For reducing the calculation procedure, we do this test as a part of the sensitivity test with idealized meteorological input. We find that the largest SMT range is yielded by firn albedo (1.28 degC), followed by fresh snow albedo (0.29 degC), threshold air temperature for rain probability (0.28 degC), and then the minimum value of k (0.22 degC). This reasonably agrees with the importance of albedo setting concluded with the original sensitivity test (Sect. 4.3). Most of other parameters yield less than 0.01 degC. Water content does not affect SMT range (0.00 degC) even if the range was expanded to 3-10%. We will add the results to Table S1 and add the descriptions in Sect. 2.6.2 and 3.3.

[reviewer comment] (1) Possibly better to make use of cumulative temperature in summer than SMT?

[author reply] Does this mean positive degree day (PDD)? If so, we have confirmed

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that the relationships between PDD and ice layer thickness (see attached figure) were almost linear. However, for the purpose of the study, we do not think that it is meaningful to provide PDDs as a temperature index instead of the summer mean temperature. We will add the description on PDD around L340 but will not provide figure.

[reviewer comment] (2) What if the method were performed on the ice cores that are recovered at different elevations of the accumulation zone of the same glacier?

[author reply] We believe that we can retrieve temperature lapse rate between the sites. We would not add any description about this in the revised manuscript.

[reviewer comment] (3) Line 234: why Tibetan glaciers?

[author reply] Because we had data and performed the study. But this is just a trigger for the analysis. We conducted the analysis with ERA-Interim data in this study (not Tibet), and found the similar relationships among these parameters.

[reviewer comment] (4) Line 317: Is there independent evidence to support the correlation between SMT and accumulation?

[author reply] Yes, that is why model-based studies were cited at L200 and L357. If this means "observational evidence", we do not have it.

[reviewer comment] (5) Line 351: Is this reasonable given the more complicate albedo scheme?

[author reply] If such "complicated albedo scheme" can reproduce the snow surface condition more realistically, it could be better for improvement. But as we addressed in L352, effects of dust and black carbon should be more significant.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-97>, 2019.

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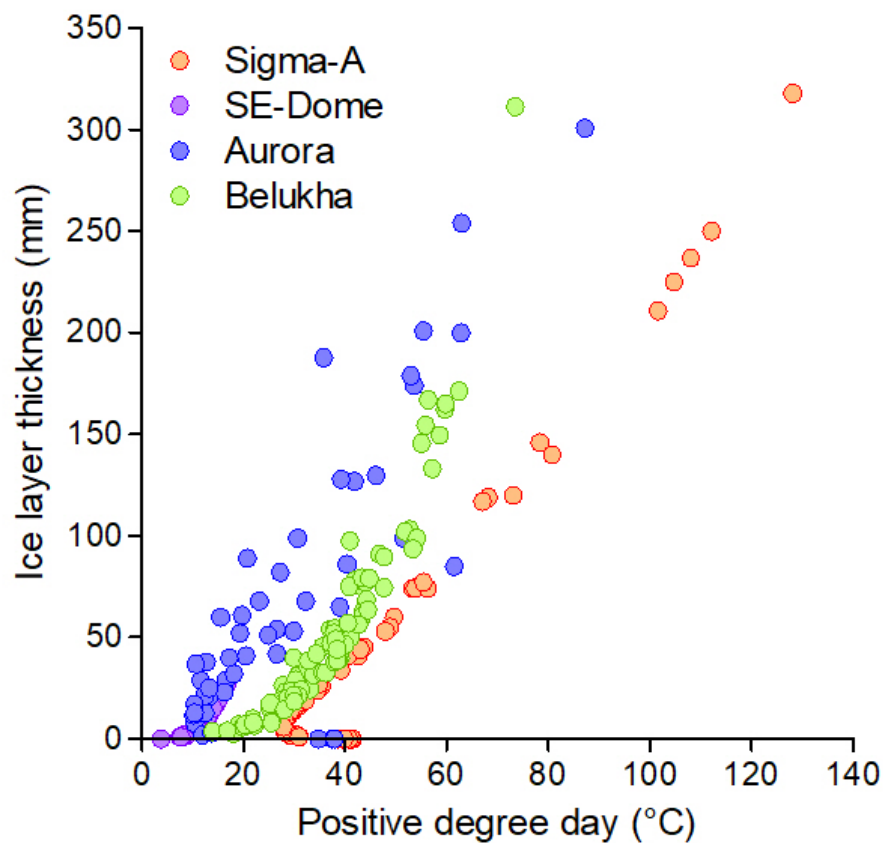


Fig. 1. ice layer vs. pdd

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