

## ***Interactive comment on “Where is the 1-million-year-old ice at Dome A?” by Liyun Zhao et al.***

**Anonymous Referee #1**

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In this manuscript, the authors model the age field in a 70x70 km region centered around Kunlun Station (Dome A). The modeling is based on the finite-elements code Elmer/Ice. It takes into account the anisotropy of the ice material due to its fabric, the mechanical behavior of the ice and the temperature field. An important assumption is that the ice sheet is in steady-state, so only a steady-state velocity field is computed. Various hypotheses are tested regarding the geothermal flux and the fabric. The model is compared to age observations at Kunlun station obtained by tracing radar layers to the dated Vostok ice core. It is found that the best agreement is obtained with a geothermal flux of 60 mW/m<sup>2</sup> and a fabric evolving from isotropic at surface to a girdle fabric at depth=2/3 of ice thickness. From there, the model extrapolates the basal age to the range 650-830 kyr BP at the base of the ice sheet at Kunlun station,

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that is too young to record the Mid-Pleistocene Transition (MPT). The model is also compared to horizontal surface velocity measurements, but it is found that it is difficult to discriminate between the different geothermal and fabric assumptions. The surface vertical velocity model is also compared to surface accumulation measurements, which allows to eliminate values of geothermal flux higher than 60 mW/m<sup>2</sup>. Finally, some locations for old ice recording the MPT are proposed, 1-2 km maximum far from the Kunlun station, on the flanks of a bedrock valley.

Generally, I enjoyed reading this manuscript which is clearly written. The modeling experiments presented are an advance with respect to the state-of-art ice flow and age modeling around Dome A, despite some rough assumptions. However, I have some major concerns explained below:

- there is no discussion on the Raymond effect, which occurs at domes with a non-linear rheology and which has an important influence on the age-depth profile. The Raymond arches should be present in their modeling experiments. In reality, the Raymond arches are probably not easily observable in the radar age observations, since the dome has probably moved during the past (a movement of only a few kilometers is sufficient to dilute the Raymond effect spatially). This is a clear limitation of the steady-state assumption when modeling the age of the ice in the vicinity of a dome. A discussion on this effect is mandatory.
- Why is the age model compared to the radar age observations only at Kunlun? The comparison could be done anywhere where there are radar data.
- if I understood correctly, to compare the modeled vertical velocity with the accumulation observations, the authors use an average accumulation which is calculated as a weighted average of glacial and interglacial accumulations. This is too rough an hypothesis. The authors should use the EPICA Dome C record to calculate a ratio between the present-day accu and the 800 kyr average accu. This way, the comparison with the modeled vertical velocity would be more relevant.

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- in a similar way, the authors should use a 800 kyr average value of the surface temperature based on the Dome C temperature variations (assuming the variations are the same at Dome A), rather than simply the present-day value at Dome A.
- there is a mistake at the beginning of section 4.1. At steady-state, surface vertical velocity should be equal to surface accumulation rate, not surface accumulation rate plus basal melting. This should be corrected.
- Because of these rough assumptions in the modeling, a perspective paragraph listing what could be improved in a future modeling study would be welcome.

I also have some minor points below:

- l.51: "Hou et al., 2007" -> missing space
- l.102: "special" -> "spatial"
- fig.1A is difficult to read. I would use a square region in a classical projection.
- l.165: "is gas constant" -> "is the gas constant"
- l. 173: "the components of..." -> missing space
- l. 242: why not using an intermediate value of the surface temperature between the present-day and the LGM? (Cf. comment above).
- l245: the no sliding assumption is quite rough. There is probably sliding where there is melting.
- l. 261: quite a big assumption here also, since the geothermal flux might change at a kilometer scale.
- l. 326: the reference is Ruth et al., not Urs et al. (Urs is the first name).

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