Review of “Late Holocene glacier variations in the central Tibetan Plateau indicated by the δ18O of ice core enclosed gaseous oxygen.” by Jacob J. Li et al, The Cryosphere.

General:

The manuscript presents new results of the isotopic composition of molecular oxygen ($\delta^{18}O_{bub}$) enclosed in the alpine glacier Tanggula in the central Tibetan Plateau. They document that there is a good correlation between the these measurements and the accumulation rate or melting rate of the glacier. Deeper firn layers go along positive $\delta^{18}O_{bub}$ values, whereas stronger glacier melting goes along with negative $^{18}O_{bub}$ values. Based on these findings, they discuss variations of the glacier and found four accumulation driven and three melt driven episodes. They also state that their measurements support a link between climate variations on the Tibetan Plateau and the North Atlantic Oscillation.

Major points:

There are major shortcomings that forces me to reject this manuscript for publications in its present form. The introduction is not focussed on the subject discussed on the paper but lists many areas of research done with these measurements at different places not comparable with alpine ice. No discussion is made about potential influences on d18Obub from processes such as firn thickness variations, melt water influences etc. Furthermore, the experimental setup is not well explained and lacks important information such as which absoluate horizons have been used for dating. The author state that the dating is very robust to +-2year, but no clear discussion is made that supports this statement. There is a good possibility to measure the air content of the ice when extracting the air from the ice. Has this be done, if not why? This would have been a possibility to compare it with the rough estimate based on images. It is also not clear from the description in section 2.2 what the exact procedure for the measurements of d18Obub is. Was it a online system or has the air been collected on a cold trap? Was the air further cleaned from water vapour, nitrogen, argon, CO2 etc or not? A scheme of the setup would be helpful here. Was only d18O measured or also other species (isotope and elemental ratios)?
Furthermore, nothing is said about how the glacier flow model lines are obtained (Fig. 3). No explanation of the model, the assumptions to run it etc.

Intro: The introduction is rather general and does summarize for what d180 on molecular oxygen has been and can be used. Yet, limited information is given about measurements on alpine glaciers in general. For instance influences of melt water on firn thickness, chemical reactions initiated through melt water and influences on gas diffusion etc. All these processes might be relevant for the observed variations of the isotopic composition of molecular oxygen enclosed in ice. Therefore, I would suggest to focus in the introduction to items relevant for the manuscript.

Along with d18O generally also d15N is measured. Do the author have these measurements?

Line 40f: Regarding the Dole effect: important contributions are missing

Line 49ff: I would rewrite this sentence to::
On the other hand, since the ice formation process in alpine glaciers is more sensitive to regional climate conditions and its variations could lead to changes in

the air component in the ice core, the climatic significance of the δ18Obub in alpine glaciers may be different from that in polar glaciers (Luz and Eugeni, 2011). However, few detailed studies have been carried out on this assumption.

Line 64: What about firn thickness and its variation over time. Are there any clues? Do you have measurements of d15N along those of d18Obub? This would be an indication of fractionation processes originating on site.

Line 95f: This means that the air content has not been measured but only roughly estimated based on images?

Fig. 2: Dating seems to be very vague and unclear. More information is required. It is helpful though to have the annual lines aligning the peaks of the three parameters. But there seems to be quite some uncertainty, at least in my view, that surpasses +-2 years stated.

Minor points:

Line 158: ….to large differences …

Fig. 3: Nothing is said about how the glacier flow model lines are obtained. No explanation of the model, the assumptions to run it etc.

Table 1: These measurements are reported against the lab internal reference standard. Please note this explicitly. What kind of standard is it, name it, is it pure ambient air, artificial air or what?

Line 206: you mean +-0.35 promille

Fig. 4: How has the zero line be defined? Generally, one assumes today's ambient air to be zero! The mean d18Obub value of the recent ice is 4 permil depleted!! How come? The offset of your ambient air on your reference is 0.32 permil.

since the Holocene…
you mean during the late Holocene

Line 215f: so what, how deep is the firn layer? Have you calculated the expected gravitational settling effect?

Line 221f: So what, how important is it?

Line 232f: not clear at all

Fig. 5: zu (h) what is the reference to these data?

Line 309: you may rewrite:
This result is consistent with those previous climatic and environmental studies in the TP, which showed that there is a close relationship between climate changes in the TP and the NAO (e.g., Chen et al., 2001; Zhu et al., 2015; Xu et al., 2019).

Line 312: The conclusion section is poor.