Dear Dr. Carlos Martin,

Many thanks for your time and efforts in managing our contribution, and many thanks to the reviewers for their thoughtful comments. Below we have made a point-to-point response to the comments. The comments are in black, and our response is in blue. We have also revised the original manuscript based on these comments. I hope that the revised manuscript proves satisfactory.

Sincerely yours,

Hou Shugui

Anonymous Referee #1:

1 General Comments

Zhang et al. "A new method of resolving annual precipitation for the past millennia from Tibetan ice cores" presents a detailed study on the average accumulation rate for 3 epochs in the last 2500 years for an ice core site on the Chongce ice cap, northwestern Tibetan Plateau. The paper combines annual layer thickness data (from ultra-high resolution ice core elemental chemistry) with a flow thinning model (constrained by water-insoluble organic carbon ^{14}C ages) to determine local net accumulation over 3 disjoint epochs. The authors have done a commendable job of addressing the issues raised during the previous review. In particular, I appreciate the information and revisions around my previous major comment concerning the data fitting for the flow model. However, there are some inconsistencies between the author response to the reviews (version 3-1) and the manuscript (version 5-2), at least for the versions I accessed. I recommend minor alterations and corrections detailed below.

2 Inconsistencies between authors response to reviews and manuscript

2.1 Minor Specific Comments

Original comment:

P2 2nd paragraph. This needs a restructure, at the moment, the sentence topics are annual layers, thinning, annual layers then thinning again. Suggest you move the sentence staring "In addition, the nonlinear" to after the sentence staring "The most common approach". Then change "The thinning parameter"→ "This thinning parameter".

The authors response is:

We agree with the reviewer, and have revised the sentence accordingly. The revised sentence is as follows;

The most common approach is to obtain annual-layer thickness based on the seasonal cycles of ice core parameters such as stable isotope ratio of oxygen in the water ($\delta^{18}O$), the concentration of major ions (e.g. Ca^{2+}, Mg^{2+}, NH_4^+, SO_4^{2-}), and the presence of melt layers (Thompson et al., 2018). In addition, the nonlinear thinning of annual layers caused by ice flow must be suitably constrained (Bolzan, 1985; Henderson et al., 2006; Roberts et al., 2015). This change is not reflected in the revised manuscript, with the second sentence still being before the first (as it was in the original manuscript).

Response: We apologize for this negligence. This change has been made accordingly in the current revision (lines 49-54).

Original comment:
P6 L188-189 These grouped peaks could also be from independent snow events with dry wind blown dust deposition between these snow events.
The authors response is:
We agree with the reviewer, and have revised the text accordingly.
The revised sentence is as follows;
These grouped peaks are interpreted as independent snow events with elevated element concentrations or with wind-blown dust deposition between these snow events. There is no such sentence in the revised manuscript.
Response: We apologize for this negligence. This change has been made accordingly in the current revision (lines 198-200).

2.2 Technical corrections
P6 L149 Delete “were”. Still need to delete first occurrence of “were”, now on line 157, i.e. change “in the ice were by filtration" to “in the ice by filtration”

There were several other cases where the authors response to minor comments stated that “Change has been made accordingly” but the manuscript does not reflect this. However, the cases not listed here were only for suggested word changes rather than an error, so I have not listed them.
Response: We apologize for this negligence.
All changes stated in the previous response have been reflected in the current revision.

3 New comments

3.1 Technical corrections
P3 L64, I think neither Roberts et al 2015 or Winstrup et al 2012 are appropriate references for alpine ice cores. Suggest either find a more appropriate reference, or reword sentence to “because of the difficulties in identifying annual layers and obtaining accurate chronologies in the deeper part of ice cores due to rapid thinning (Roberts et al., 2015; Winstrup et al., 2012), with this issue especially problematic for alpine locations.”
Response: We agree with this comment, and have replaced with two other appropriate references (Henderson et al., 2006; Yao et al., 2008).

P3 L70, suggest adding “e.g.,” to the start of the reference list.
Response: Change has been made accordingly.

P3 L83, change “Fig. S1” to “Fig. 1”
Response: Change has been made accordingly.

P3 L85-86, change “first time to preform the” to “first application of”
Response: Change has been made accordingly.

P7 L193, change “Fig.1” to “Fig. 4”. If this reference is really figure 4 then need to swap the order of figures 3 and 4 so that they are first referenced in the correct order.
Response: The “Fig. 1” has been deleted in the current revision.

P7 L189, change “Fig.1” to “Fig. 2”
Response: Change has been made accordingly.

P9 L244, delete “roughly”, the two age estimates agree within a small fraction of their uncertainties.
Response: Change has been made accordingly.

P11 L314-315, annual precipitation reconstructions based on ice cores has indeed already been achieved for both Greenland (e.g., Dahl-Jensen et al 1993) and Antarctica (e.g. Thomas et al 2017, doi:10.5194/cp-13-1491-2017). I suggest you include these (or other) references.
Response: According to the comments from Dr. Cole-Dai (Referee), we have focused on the alpine ice cores, therefore, we deleted “Moreover, ice core accumulation records could be used to quantify annual precipitation over Antarctica and the Greenland ice cap, where no other precipitation proxies exist.” in the current revision.

P12 L328, change “ice core model” to “ice cap flow model”
Response: Change has been made accordingly.

Jihong Cole-Dai (Referee):

I thank the authors for carefully and seriously responding to comments and questions from the reviewers and for addressing, in the revised manuscript, the major issues raised in those comments and questions.

Here I will comment on the response to my comment that the identification of annual layers and determination of annual accumulation (layer thickness and thinning correction) described in this manuscript is not new, as claimed in the original manuscript. The authors appear to agree with that comment and have removed the word “new” in presenting their method of annual accumulation determination. Their exact words are: “We agree with the reviewer that the novelty of the method only applies to mountain glacier ice cores. Annual layer identification can be achieved for the Greenland summit ice cores for the past tens of thousands of years (Rasmussen et al., 2006), but this is not the case regarding the mountain glacier ice cores due to their relatively short length and rapid thinning. Our study is indeed the first to apply this method, i.e. taking account of both annual layer identification at the millennial time scale and annual layer thinning, for the Tibetan ice cores. It has also potential to be applied to other alpine ice cores. We have clarified these points in the revision.”

I will not dispute that this is the first time the approach of annual layer identification and modeling layer thinning is applied to an ice core from the Tibetan Plateau. But this paper is about METHODOLOGY of reconstructing accumulation/precipitation history (note that the paper does not present an accumulation record from the Chongce ice cores based on annual layer identification). Again, the method presented here does not qualify as new or novel.

I sense that what is new or what the authors truly would like to present as the major contribution with this work is the APPLICATION of that method (annual layer identification in conjunction with modeling layer thinning) to Tibetan Plateau ice cores and potentially to all mountain (alpine) glacier

ice cores. The "novelty", if novelty is necessary, is that the paper demonstrates that that method can work for mountain glacier ice cores, as it has worked well for polar ice cores. I feel the papers misses this point, perhaps due to the desire to present something "new" in (the entire field of) ice core research. I see this desire in numerous places in the paper, where the authors imply a new method (Lines 81-82) and stress a "first" (Lines 93-94). This tendency to be "first" leads to exaggerating statements. An example of that is "Moreover, ice core accumulation records could be used to quantify annual precipitation over Antarctica and the Greenland ice cap, where no other precipitation proxies exist." (Lines 313-315), where the authors seem to imply that their "proposed" method would be valuable beyond mountain glacier ice cores. There is just one problem with this implication: long (millennia) accumulation records have been constructed from Antarctica and Greenland ice cores based on the exact method (annual layer identification and layer thinning modeling).

I see that one flaw with the structure of the paper is that the description and presentation of approaches, methods and result discussion cover both polar ice cores and mountain glacier ice cores. When discussing shortcomings and limitations of previous work, the authors do not always distinguish between polar and non-polar ice cores. In Introduction, the authors, after describing how high-resolution measurements can discern annual layers in long polar ice cores covering thousands of years, state (Lines 71-73) "The remaining challenge for reconstructing long-term accumulation records thus lies in establishing accurate thinning parameters, and this is largely dependent on the reliable dating of ice cores, particularly at deeper sections." This statement may well apply to mountain glacier ice cores. But, in my opinion, this would be a very shaky statement about polar ice cores, at least for the millennial time scale.

Response: We agree with the directions, and have focused only on the alpine ice cores in this manuscript. Therefore, we deleted the contents concerning to the polar ice cores in the current revision. Specially, we have added "alpine" before "ice cores, particularly at deeper sections" in line 73 (line 73 in the current revision).

I would recommend that the paper be revised to make a clear distinction between polar ice cores and mountain/alpine glacier ice cores. I would like to see clear acknowledgement of (giving credit to) the success of reconstructing long accumulation records (using the method of annual layer identification and layer thinning modeling) with polar ice cores. This would allow the authors to claim and demonstrate "novel" approach or new application of existing methodology and true potential value of their work (to mountain glacier ice cores). Hopefully, such a revision would prevent exaggerations such as that in Lines 313-315.

Response: We agree with the reviewer. We have replaced "novel" with "quantitative" in the title and deleted "Moreover, ice core accumulation records could be used to quantify annual precipitation over Antarctica and the Greenland ice cap, where no other precipitation proxies exist." in the current revision.

Below are several comments and questions from me about a few specific aspects of the manuscript. I hope the authors will consider these when revising their manuscript.

Line 50. What are "low temporal resolution sites"?

Response: To prevent misunderstanding, we have deleted this sentence "but may be centennial for low temporal resolution sites" in line 50.

Line 50-51. How can non-linear thinning be “constrained”?

Response: We have adjusted the structure of the sentences in lines 49-58 . The revised sentences are as follows;

The most common approach is to obtain annual-layer thickness based on the seasonal cycles of ice core parameters such as stable isotope ratio of oxygen in the water ($\delta^{18}O$), the concentration of major ions (e.g. Ca^{2+}, Mg^{2+}, NH_4^+, SO_4^{2-}), and the presence of melt layers (Thompson et al., 2018). In addition, the nonlinear thinning of annual layers caused by ice flow must be suitably compensated (Bolzan, 1985; Henderson et al., 2006; Roberts et al., 2015). This thinning parameter of ice cores is usually derived from an ice flow model constrained by the ages of absolute chronological markers, e.g., peak of beta and/or tritium activity from thermonuclear bomb testing in the second half of the 20th century, well-defined aerosol layers and/or tephra from large volcanic eruptions, and radioactive dating method based on ^{210}Pb activity decay (Uglietti et al., 2016; Zhang et al., 2015).

Lines 201-204. After describing how the uncertainty of the counted number of annual layers is determined, the uncertainty in reconstructed accumulation rate is presented. How is the accumulation uncertainty derived from the layer number uncertainty?

Response: The uncertainty of the mean annual layer thickness is derived from the layer number thickness. The upper limit of the mean annual layer thickness of each section was calculated through dividing the length of the section by the number of certain annual layers. The lower limit of the mean annual layer thickness of each section was calculated through dividing the length of the section by the total number of annual layers (including the number of certain and uncertain annual layers).

Line 205. In response to my previous comment, the authors “revised the wording” about using the StratiCounter program for layer counting. They removed the word “verify” regarding the program counting. However, here they state “the StratiCounter program (to) identify annual layers objectively”. I wonder in what way the StratiCounter program is objective.

Response: The StratiCounter was designed to facilitate the counting process and make counting less subjective (Winstrup et al., 2012). In fact, the StratiCounter program only provide a relatively “objective” mathematic expression in comparison with subjective visual identifications. To avoid misunderstanding, we have deleted “objectively” in the revised manuscript.

Lines 304-305 (and 96-97). The authors claim that “the method proposed in this study produces reliable results and has the potential to reconstruct high-resolution continuous precipitation time series.” The basis for the word “reliable” is entirely based on the comparison of the very limited results from this study with accumulation reconstructions from other studies of various methodology (Figure 4). The best word the authors use to characterize the comparison result is “consistent” (Lines 295 and 300). How does “consistent” lead to “verified” (Line 96) and “reliable”?

Response: We agree with the reviewer, and have changed “verified” to “evaluated” (line 96). We also revised the sentence in lines 304-305 as follows.

These results suggest that the method proposed in this study has the potential to reconstruct high-resolution continuous precipitation time series.