Dear Anonymous Referee,

Many thanks for your comments. We also clarified the objectives in the revised manuscript and corrected grammar and wording by a native speaker. In addition, we edited the introduction and study area according to your suggestion.

Study area: again this is rambling. Glacier classification into "continental" and "temperature glaciers" is erroneous, and references are outdated. References about climate data are also outdated. This is not a climate paper so such details are meaningless. Showing pictures of Everest in figure 1 does not bring anything to the paper.
Response: According to the advice of anonymous referee, we deleted the sentence "Glacier classification into "continental" and "temperature glaciers"". In order to indicate the distribution of glaciers in the southern slope and northern slope, we made details in climate. Showing pictures of Everest in figure is the purpose to intuitively demonstrate the highest peaks in the world.

Data and Methods: No new methods were used here, and the authors state that for some years they averages +/- 2 years. It makes it hard to know then what year the glacier outlines represent when for some areas various years of imagery were used. Reporting cloud cover for each scene is irrelevant since cloud cover can be outside the glaciers. It becomes clear only later on that the authors derived ne set of outlines per decade. In this case, why not use RGI which is also from 2000 and is a compilation? The authors would need to show the superiority of their approach over existing outlines, but this is not done in the paper.
Response: The purposes of this paper analyzed the glacier distribution and variation characteristics in the entire Himalayas, so the method of glacier boundaries extraction is not new. The RGI are based on the ASTER data and this paper we extracted glacier outlines used Landsat TM/ETM+/OLI in 1990, 2005 and 2015. In order to avoid the impact of data inconsistency, we obtained the glacier boundaries used Landsat data and we also corrected potential error by visual interpretation in our paper.

Glacier mapping also is based on established methods (band ratios) and this section does not need almost one page to describe. Just a technical detail, band ratio of 1 seems very low for OLI, while 1.8 for TM seems acceptable; the authors do not comment on this. Also, the authors refer to the 2nd Chinese inventory but do not add a reference, nor how it was used. Debris covered glaciers, as it seems, were mapped manually but the description is fuzzy and there is nothing innovative here.
Response: Thanks for the Referee's suggestion we have added some contents in the details of image thresholding for Landsat OLI imagery and marked them in red in lines 183-190. In order to make the Landsat OLI data match with the Landsat TM/ETM+ imagery, we set Landsat TM/ETM+ as a reference to make geometric correction to Landsat OLI scenes. We also add the reference for 2nd Chinese inventory in line 204 in the revised manuscript.

Error estimates are based only on a single glacier and seem incomplete. A study of such spatial extent would need a much more thorough error and uncertainty section.
Response: Many thanks for your advice and we have used the buffer method to calculate the accuracy in lines 245-247 in the manuscript.

Results –
The authors present glacier changes across the study area with respect to various factors: part of the range, elevation bin, type of glaciers etc. While this is of possible interest, especially with respect to the spatial distribution of glacier changes, the information presented is hard to distill and very dense. This needs much more synthesis. Also, the changes are referred to most of the times as simply "changes"- it should be mentioned when the changes are in glacier area, length or height etc.
Response: The "changes" in this paper is referred to the area change.

Some concepts need much more development, for example debris cover. For example l 492 - 495 the authors mention the melt inhibition due to thin debris- however in the recent years there have been a number of publications which point at the presence of supra-glacial lakes and ice cliffs and their effect on melting rates over thick debris. The references presented are also outdated. Also the authors claim they test the effect of debris on melting rates- "To investigate whether the debris of the Himalaya can inhibit the glacier melting" (l522) - but then they present area changes, while debris cover glaciers can get thinner yet display no area change. So area change as an indicator of surface melt is not an appropriate measure.
Response: The Referee's comments are good. However, the large area covered by glaciers in the Himalayas, it is more difficult to calculate the change in height, so we used area change as an indirectly indicator.

In general results are difficult to follow in the form they are presented. For example, the authors compare the glacier change analysis with other studies- but there is no reference as to which year, so this is quite meaningless without adding more details. For example, they show only 1.3% difference with Guo et al (2015) - but it is unclear what" regional Himnalaya" means. Results on the glacier distribution across the Himalaya are mixed with glacier changes and this is all hard to follow, for example phrases such as " The total area of the mountains above 4,000 m of the Himalaya is about 1.59×10^5 km^2 which provides a good topographical condition for glacial development" are not very meaningful, I am not sure what the authors mean. Same for l 373 to 381- is this relevant to the scope of the paper?
Response: Many thanks for the Referee's advice. We wanted to analysis the relationship between glaciers and topography, so we statistics the area in different elevation zones of the Himalayas mountain.

We appreciate for the Referee's warm work earnestly, and hope that the corrections will meet with approval.

Best wishes

Qin Ji