

Interactive comment on “Glacier variations in the Himalaya from 1990 to 2015 based on remote sensing” by Qin Ji et al.

Anonymous Referee #2

Received and published: 26 May 2020

Based on the title, the paper by Ji et al aims at presenting glacier variations in the Himalaya in the last decades. However, it is not clear what exactly the authors mean by glacier variations, and why the need for such a study while there are already regional studies on glacier changes, and lots of glacier inventories to be used. In the introduction the authors state that there is a need to study the glacier distribution and changes across the Himalaya, and they propose a new set of glacier extents to investigate changes from 1990 to 2005, along with glacier distributions.

First, to me, it seems that this is a duplicate effort to previous work done in the region by past inventories, including the revised GADNDAM as well as other regional inventories. The authors would first need to present outstanding issues with the existing inventories to justify yet another study. Second, as we know there is considerable effort needed

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to produce high quality inventories - and the methods used in this paper do not seem superior over the existing ones.

The English language throughout the paper is insufficient, and needs lots of improving. It is hard to estimate the value of the research conducted because the paper is very poorly written and not structured. Unfortunately I do not consider this submittable; the objectives need to be clarified and the writing needs much improving in order to assess the value of this paper.

Some general comments by section:

Introduction: The introduction is rambling and not to the point. It is not necessary in a journal specialist in cryosphere to explain what cryosphere is, for ex., or to mix lots of concepts, eg mass balance and SLR estimates, then water resources and formation of the Himalaya.

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 Glacier classification into "continental" and "temperature glaciers" is erroneous, and references are outdated. References about climate data are also outdated.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.

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 one 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.

The SRTM vs ASTER comparison version is also inadequately written-

Glacier mapping also is based on established methods (band ratios) and this section

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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.

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.

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.

Some concepts need much more development, for example debris cover. For example I 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" (I522) - 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.

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

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example, they show only 1.3% difference with Guo et al (2015) - but it is unclear what "regional Himalaya" 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 km² which provides a good topographical condition for glacial development" are not very meaningful, I am not sure what the authors mean. Same for I 373 to 381- is this relevant to the scope of the paper?

The authors go onto mention conditions for glacier formation- but this has nothing to do with the scope of the paper.

The rest of the results are hard to follow and the text is poorly written, so a proper review of the results and discussion cannot be provided in the current version of this paper.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-297>, 2020.

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