

Overall comment: I found the paper to be quite interesting and generally well-written, but in its current form it comes across as a response to an implied argument that has not actually been demonstrated to have ever been made. If there is indeed prior literature that has argued to ignore the smallest glaciers (and actually ignored them) when assessing total regional glacier volume and mass then this literature needs to be cited and the implications of the way these papers handled their data need to be addressed explicitly. Unless this is done, then there is no real context or motivation for the present paper, which reads like a straw man has been set up and knocked down. I suspect this is mainly a writing issue, but it is a significant one and the authors need to address it.

The title is appropriate. Generally the paper reads easily and well. Abstract is good. Methods are explained clearly and references appropriate, except that no work is cited that actually makes the arguments to which the paper appears to be a response! I did not identify any problems with formulae, symbols etc.

Detailed comments - referred to by line number

22: in response to melting of many smaller glaciers. While the world's...

24: Why are the smallest glaciers most likely to be overlooked? Is it because of the potential for confusion on imagery with snow patches - seasonal and perennial?

27: Is there evidence that their contribution has been summarily dismissed? If there is, cite the evidence. If this cannot be done, this reads like setting up a straw man so you can knock it down.

29: Delete "Moreover" - can't really start a paragraph with this phrase.

42: Define "glacier" early on. Later (line 75) it becomes clear that ice sheets are excluded - but this is not obvious at this point.

69: Are total mass and total volume equivalent in fact? I wonder whether the proportion of an ice mass that is firn, as opposed to ice might vary systematically with volume - or with the climate in a region - or with the long-term trend in mass balance in a region. If any of these were the case, the relation between volume and mass might be somewhat more complex than this statement implies.

105: I believe Shook and Gray were referring to patchiness within a disintegrating seasonal snowcover, not to perennial snowpatches in mountainous landscapes that might be confused with small glaciers. If so, is it reasonable to assume the two types of snowpatch would have similar number-size distributions? I would also say "power-law number-size distribution" in line 105.

109: Not obvious what this flowshed algorithm does or what it was used for in this context. Was it used to split icefields into constituent glaciers - and is this a legitimate thing to do in the present context. The Clarke reference is missing from the list of references at the end of the paper.

111-112: Not obvious to me why this regional subdivision of British Columbia Glaciers was needed for the purposes of this paper.

113-114: What is the implied significance of this statement - is the flowshed algorithm doing this differently from other methods at the smallest glacier sizes - and if so, what? Is the implication that the apparent deviation in the size distribution at the small end of the size distribution is a methodological artefact? Please clarify what is intended here.

115-120: I had the sense that the direction of the underlying argument was lost in this part of the text

124: sufficient for what purpose?

167: glacier sizes (ie delete s from glaciers)

Section 2.3: The authors really need to refer to published examples of calculations that have been made assuming that only the largest glaciers matter to demonstrate that there is a real debate here. Again I had the sense that we might just be discussing a straw man, rather than a genuine issue.

201: What about regions that contain a mix of glaciers and ice caps? How should they be handled?