

Interactive comment on “Thinning of the Quelccaya Ice Cap over the last thirty years” by C. D. Chadwell et al.

E. Berthier (Editor)

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Dear Authors,

With support from the reviewers, I have been considering your answers to their numerous comments/suggestions. Unfortunately, I do not think you provided sufficiently convincing responses to warrant consideration of a revised version of the manuscript. I was expecting a more detail response letter and it is unfortunate that some of the comments (in particular important point #5 and also points #6 and #7 from Referee #3) have not been addressed.

Among the main weaknesses of the paper/responses is the too speculative attribution of the thinning in the ablation and accumulation section of the transect. (i) In the accumulation zone, Referee #2 made an important comment (pg. 13, lines 6 - 9) about

inter-annual variability in accumulation in relationship to El Nino / La Nina events (high inter-annual variability) and the risk of estimating accumulation trends from only a few years of data and the severe dependency of the trend on the start/end point. This point was not really addressed in your responses. (ii) In the ablation zone, Referee #1 and #3 raised a strong contradiction in the TCD paper. In the one hand, you cited several studies describing strongly decreasing ice fluxes toward the ablation zone (on other mountain glaciers, Span & Kuhn, JGR, 2003 is another excellent reading on this topic), equivalent to decreasing emergence velocities and with thus a strong contribution to the observed thinning. But in the other hand, you still state in the abstract that "thinning is likely caused by a 1-2 m w.e. a-1 increase in melting and sublimation above steady-state." A conclusion not supported by your data.

Although the reviewers and I acknowledge your efforts to understand the cause of the thinning along this transect, it appears that you simply do not have sufficiently precise data to conclude convincingly.

Your geodetic measurements of elevation change remain solid and will be even more valuable when the complete error analysis you made will be described. The transect-wide mass balance remains more problematic. You did not justify why you used a very low density of 440 kg/m³ to convert the volume change to mass change in the accumulation area (a justification was requested by Referee #3). Such a low density would imply a drastic change in the density vertical profile between 1983 and 2013 that does not appear to have occurred (read among others Huss, TC, 2013 on this topic). You did not say neither how you would demonstrate that the single profile studied here is representative of the rest of the ice cap (your statement in the response letter: "given the size of Quelccaya we can make a case that our profile is representative of the entire ice cap" is not convincing).

I still strongly believe in the scientific values of your elevation change measurements in this remote and rapidly changing region. They will deserve publication in the future but requires a complete rewriting of the paper that goes beyond the scope of what can be

done in the framework of the present submission.

I am sorry for not being more positive. I hope that the reviewer's comments will help you to move forward with a prompt re-submission elsewhere.

Please do not hesitate to contact me in case you have any questions.

Best regards, Etienne Berthier – TC Editor

PS: Be prudent with statements such as "This reviewer's comments are insightful, yet suggest that their experience has primarily been with very accessible glaciers and/or modeling rather than fieldwork." All three referees that worked on your paper have a strong field experience in remote/harsh environment...

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-40, 2016.

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