

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

### **Anonymous Referee #1**

Received and published: 15 April 2016

Title: Thinning of the Quelccaya Ice Cap over the last thirty years Manuscript: The Cryosphere Discuss., doi:10.5194/tc-2016-40, 2016 Authors: C.D. Chadwell, D.R. Hardy, C. Braun, H.H. Brecher, and L.G. Thompson

General Comment:

This manuscript provides detailed information about surveying work conducted on the Quelccaya Ice Cap (QIC), which allows the authors to report on thinning and volume loss along a profile of the western part of the ice body over the last 30 years. The recent field work took place in 2013 and 2015, and was compared to a network established in 1983 – the same year the first deep ice cores were recovered from the QIC. The main contribution of this research is to present up to date numbers about thinning and the volume changes that have taken place since the early field work was completed,

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with the core of the manuscript describing the data collection and surveying methods. A continuity relation is also presented, with the purpose of providing information about the physical processes controlling the observed changes. These findings are a little more tenuous than the surveying results, which is evident in the manner in which they are described by the authors. A decision should probably be made as to whether these latter results are really necessary and/or if they really add to the understanding of the physical processes controlling the observed retreat. However, if they are not included there is a question mark over whether the surveying results on their own are enough in their present form to warrant publication. To aid in that decision the following comments about the manuscript are offered, which the authors may wish to consider should the paper be considered for publication in The Cryosphere.

Specific comments:

Please note that page number is referred to as (P) and line number is referred to as (L).

1. Structure: The overall structure of the manuscript could be refined to fall within a more conventional format. There is no clear delineation between methods-results-discussion in the manuscript in its present form. The aim and/or objectives should be more clearly defined at the end of the introduction and it would be valuable to provide information about how the manuscript is structured. The second section describes the data collection, which is followed by geodetic results (section 3). One could argue that the continuity relation and the methods used to characterise the physical processes in the ablation and accumulation zones (Sections 5.1 and 5.2 in the discussion) could be contained in a discrete methods section. The present discussion (Section 5) is not really a synthesis of all the results. The manuscript would be strengthened if a clearer methods section was defined, followed by key results and a synthesis of the importance of these in relation to previous and ongoing research.

2. Abstract: The first part of the abstract is generally well written and provides in-

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formation about the thinning and volume loss of the “unit-volume” (that needs further clarification in the manuscript – see below). May one ask why the first sentence refers specifically to the challenges of acquiring data from “accumulation zones” of tropical glaciers when the results represent changes in elevation along a transect of QIC? There is no clear definition of accumulation or ablation zones in the manuscript, and no information is given about the areal extent of each of these zones on QIC. The latter part of the abstract, starting with the sentence “increasing air temperatures” and those that follow are not strongly supported by the results contained in the manuscript. In particular, the statement about increasing air temperature being “a major driver of the observed changes” is not demonstrated by results presented in the manuscript. The sentences that follow are also quite “speculative” and not really supported adequately to be deemed as results. There is no insight as to what “steady-state” refers to and the causes of the surface lowering in the accumulation zone are questionable.

3. Introduction: The introduction is very specific to QIC and does not provide any context for the broader motivation for the research. The first paragraph is very detailed and would almost be better suited as a site description in the methods. If retained, then the area of QIC should be provided here, or elsewhere, as reference to QIC being the “largest tropical glacier” is not really adequate. The failure to provide the area in the manuscript prevents readers from fully appreciating what the “ice unit-volume” represents. As noted above, a clearer aim and some structural information about the manuscript would be of benefit to readers at the end of the introduction.

4. Data collection: This is detailed and provides readers with the necessary background about the approach. It might be of interest to reveal to readers what parties were responsible for the early measurements versus those responsible in 2013 and 2015. On P6, L7 the accuracy of the EDM instrument is described, which is the first of many numbers in the manuscript that contain an uncertainty. It would be useful to provide additional information about how this and other uncertainties are calculated. All the uncertainties in the manuscript are given as 2 standard deviations ( $2\sigma$ ) from

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the value presented, but it is difficult to know in many instances how these are actually calculated. Some effort to unpack these, especially for the surveying results would be useful to readers to help strengthen the findings presented. A table that provides a summary of the field measurements, including the number of stakes installed versus those re-measured and/or surveyed, with the dates when this occurred would help support the text provided to describe these important activities.

5. Geodetic results: As noted above, additional information about how uncertainty is calculated in the elevation change results would be useful to readers. To ensure that the volume changes are more meaningful, the authors should consider more clearly defining what portion of the QIC is represented by the 1983-84 survey network (P7, L23-24). “Using a variety of ice thickness measurements made over the past thirty years” is not very clear and the “unit-volume of ice along this profile” should be constrained in some way, otherwise the volumetric decreases are not that meaningful. The uncertainty in the annual mass balance is shown to be  $\pm 0.1$  m – again, how is this calculated and does it take into account, for example, uncertainty associated with applying specified snow densities?

6. Section 4: The use of the continuity equation to relate surface elevation, mass balance and emergence velocities (P9, L6-12) is justified by the close comparison of the measurements of surface lowering of the summit elevation and those calculated. How confident are the authors that the similarity of the presented means justify the use of the continuity equation to unravel the key physical processes in the ablation and accumulation zones? The uncertainty in these estimates is in the same order of magnitude of the values determined. Are they really that similar and how sensitive are they to the assumptions necessary to derive them?

7. Discussion: The description of the physical processes occurring in the ablation and accumulation zones (sections 5.1 and 5.2) do not read like a “discussion” section. Both sections are quite data rich and some of the data presented fall outside of the previously defined study period (e.g. Section 5.2 (P13, L15-33 – reference to snow pit

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sampling starting in 1974). The results presented in Section 5.1 do support the notion that increased ablation can account for the observed thinning and margin retreat, but this section does not provide enough evidence to show what physical processes are responsible. In the abstract, it is stated that it is “melting and sublimation above steady-state” – differentiating between these two is extremely important if air temperature is deemed responsible, which is not done in Section 5.1. The physical processes responsible for surface lowering in the accumulation zone remain poorly constrained using the approach adopted (Section 5.2) – the lack of atmospheric observations or application of a climate model (e.g. energy and mass balance modelling) prevents the authors from adequately distinguishing between the importance of changes in accumulation and densification of snow and ice (firnification rate) – acknowledged by the authors (P14, L3-5).

8. Conclusions: The first paragraph and the results presented in relation to surface lowering and volume loss are of interest. However, the second paragraph (like the abstract), starting with the statement that surface lowering is caused by a combination of “melt, sublimation and ice flux rate” does not provide any new insight – one would expect surface lowering to be a combination of these over most retreating glaciers. It raises the question as to how useful the continuity relation is to this research and if the latter part of the research is that valuable. The approach reads a little bit like a “back of the envelope” exercise – it succeeds in providing a framework to assess the possible causes of thinning and volume loss but ends up failing to fully account for the physical processes responsible. The same applies to the results from the accumulation zone – again, the authors acknowledge that the observations are not “statistically significant”. If the latter part of this research is to be retained, it requires further justification as to why it is useful and how it provides a building block to ongoing research. Some explanation as to why climatological data are not included to help resolve some of the uncertainty is almost necessary.

9. Final comment: The observational data is of interest to readers given the challenges

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to extract such valuable information from tropical glaciers – especially the largest one remaining on earth. However, the manuscript falls a bit short in adequately accounting for the physical processes responsible for the observed changes. It succeeds in showing some of the likely mechanisms responsible for the observed changes but is this really enough?

Minor technical suggestions

P1, L1: small t for tropical glaciers P2, Figure 1: It would be useful to have an insert that shows where QIC is in relation to the regions that surround it.

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

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