

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

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Dear Editor:

Again thank you for the additional time to respond to the reviewer comments. We authors were spread across several continents this summer.

We feel quite strongly that our geodetic results showing thinning and mass balance rate for the Quelccaya Ice Cap to be both robust, unique and certainly worthy of publication for the largest tropical glacier on Earth.

Two general responses will go a long way towards addressing the reviewers concerns. First, reviewers repeatedly noted the lack of details on the uncertainty calculations. Two of the authors have post-graduate degrees in geodesy and are well versed in geodetic

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approaches including propagation of errors through a sequence of non-linear functional relationships. All the uncertainties reported in the draft manuscript are rigorous. However, to save space we did not include the details. Clearly, the reviewers found this inadequate. This can be rather easily corrected, though a rough estimate is that this material would occupy ~ 5 journal pages. The question is (and this goes back to our original decision to leave out much of the detail) should we include this in the main text or is appropriate to include these details in a well-organized supplemental section? Can you advise us on the best approach?

Second, all reviewers provided several good suggestions on re-organization of the paper. We acknowledge and agree with these multiple suggestions and can re-organize the paper, in terms of distinguishing and separating the methods, results, and discussion sections.

However, we are somewhat baffled by some of the tenor of reviews .

We feel that we read plenty of papers, including in The Cryosphere, that determine a geodetic mass balance from ice surface elevation changes (usually based on comparing a DTM from old maps with LiDAR) and then explain and interpret that as best and as conservatively as possible using whatever additional data and evidence are available. That's pretty much what we did and that's science in the real world.

Reviewers seem somewhat fixated upon lack of and uncertainty of the field data used in the analysis of potential cause(s). We concede that the steady-state assumption / constant emergence velocities since 1983 highly-problematic. We can eliminate the steady-state assumption / emergence velocity assumptions. However, we emphasize these assumption/analysis do not change the fundamental geodetic results (and their relevance).

Thus, that leaves the question of how best to handle explaining the robust pattern of thinning which emerges from the study. We feel we can address most of the reviewer concerns with a re-organization of the sections and adding more details on the un-

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certainty calculations. We feel we can sharpen some of the discussion on possible cause(s), although our issues with the timing of annual visits/measurements, density data, and lack of pre-2004 climate measurements will always be limitations.

Finally, we welcome your input and advice on reorganizing the paper.

Our (AUTHOR in red) responses are in the attached document.

Best regards,

Dave Chadwell

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/tc-2016-40/tc-2016-40-AC1-supplement.pdf>

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

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