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The Cryosphere Discussions

Interactive comment on "A comparison of different methods of evaluating glacier response characteristics: application to glacier AX010, Nepal Himalaya" *by* S. Adhikari et al.

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Summary Assessment

This paper takes results from a previously published numerical model of glacier AX010 in Nepal to assess glacier response time scales. My overall assessment is that the paper has a significant number of errors or misconceptions and does not add significantly to the literature on response time scales. The paper creates the impression that the previous literature on time scales is not always well understood. Perhaps this is a matter of imprecise wording, but it requires substantial revision at the very least.

The results of the numerical model are interesting, but a lot of this is already published in a previous paper (e.g. Fig. 5). My recommendation therefore is that the paper should not be published in The Cryosphere; or at the very least not before significant revision.

Specific comments (in order in which they occur in the paper)

References are page.line

766.2: I don't know how many times I've read that a particular glacier is amongst the most sensitive to climate change. Can you back up this claim or just leave it out?

766.11: (and also Conclusions): A numerical model is NOT the only way to assess volume and area time scales; see, for example, Harrison et al. (2003, J.Glac.).

766.16: "... is particularly sensitive ...": What does the particular refer to? Compared to what?

766.20 -: "... glacier mass balance ... is most crucial to its survival": that's obvious, no?

767.14 -: Why exponential? I think this is correct in a linearization, that is, as an initial response. But I'm not so sure about large responses.

768.5: Is is true that the elevation-mass balance feedback is particularly important for steep glaciers? I would think it is particularly important for flat glaciers, because a small change in elevation can bring large areas from accumulation into ablation zones. This is at the heart of the Bodvarssen instability (Bodvarsson, 1955, Jokull).

770.25: You only show that Kathmandu is broadly representative of Chialsa, not the glacier

771.20 and Fig.3: It is not clear what is being correlated here. Is it terminus position versus temperature, or change in terminus position versus change in temperature? Is it summer temperature or annual temperature?

772.8-: An increase in precipitation does not always lead to higher mass balance. Changes in precipitation are usually linked to changes in temperature and therefore solid-to-liquid ratio.

773.2: Again, the exponential form of this is the result of a first order linear treatment, and the long term response does not have to follow that.

774.8: What's the terminus thickness?

775.16: Harrison et al. (2001, J.Glac.) give a clear definition of H: It is dV/dA. Estimating it is another matter ...

778.2: This is a misrepresentation of Harrison et al. (2001). First of all, they refer to Elsberg et al. (2001) for justification of using b_r. Then they do make the claim that abs(b_t) should be smaller than abs(b_r). But if you look at the definitions, then you see that b_r refers to the balance rate averaged over the area of change \Delta A. b_t is simply that same rate, but at the elevation of the former ice surface. b_t does not refer to the balance rate at the terminus in general, this approximation is only made when the method is applied to South Cascade. Interestingly enough, in this paper the authors make the same approximation.

779.24 and many other places: It is not clear whether you are refering to an initial steady state or a reference state. This is actually one of the major points of confusion throughout the paper. When you start a calculation of glacier changes in 1996 there will be two effects: 1) adjusting to the amount of imbalance at that time and 2) adjusting to ongoing changes. This is not clearly kept apart, and the Harrison papers make that point quite clearly.

780.5 -: I do not understand the concept of "initial terminus response time". This seems to be entirely a grid-dependent number that tends to zero with decreasing grid spacing. So the initial terminus response time is a meaningless concept.

780.27: The Oerlemans equation is only correct if \Delta L and \Delta E refer to a steady state. Is that indeed the case here?

781.22: C should not have units

784: The last paragraph of "Reaction time revisited" does not contribute anything to the topic. As a matter of fact, one walks away from this paper with the impression that response time is a very vaguely defined concept and not much is known. I believe that the Harrison papers (2001, 2003) have done a reasonable job to define volume and area time scales in a way that can be clearly followed. This discussion only adds confusion.

Summary

This paper should not, in its current form, be published in The Cryosphere. If the authors wish to resubmit I recommend that the paper be made much clearer, paying particular attention to the following:

1) When you talk of response times, give clear definitions of what is meant. Pay particular attention to Harrison et al. (2003, J.Glac.), of which the authors do not seem to be aware. The paper clearly discusses the differences between a "plastic glacier model" (immediate response of area to volume) and attempts to parametrize glacier flow response (through a relation V(A)).

2) Clearly keep initial or reference states apart from steady states. For "plastic glaciers" this does not matter, because area responds immediately to volume (Oerlemans' paper is an example of such a case). But your flow model is not plastic, so these issues become relevant.

Interactive comment on The Cryosphere Discuss., 3, 765, 2009.