The Cryosphere Discuss.,
doi:10.5194/tc-2016-116-RC2, 2016

Interactive comment on "Incorporating Distributed Debris Thickness in a Glacio-Hydrological Model: Khumbu Himalaya, Nepal" *by* James S. Douglas et al.

Anonymous Referee #2

Received and published: 3 August 2016

General Comments

The authors force an existing glacio-hydrological model with a bias-corrected RCM to simulate mass balance evolution and changes in runoff from glaciers in the upper Khumbu catchment, Nepal. The main novelty of the study is the incorporation of the effects of debris cover which is configured to represent 'thin' and 'thick' debris maxima and the effect of bare ice cliffs and ponds within the continuous debris area, in separate model runs. This is an important area of research and one of the first studies which attempts to investigate the impacts of debris cover on future glacier evolution. However, given the recent advances in more physically-based modelling of glacier melt beneath debris covers, the simple empirical melt model and rather crude

parameterisation of debris cover applied here seems like a backward step. Furthermore, methodological flaws in the calibration of ablation parameters undermine the work and make it difficult to interpret the outputs in a meaningful way. The discussion mainly relates to model weaknesses and uncertainties and the research doesn't deliver robust estimates of future Khumbu catchment glacier evolution, nor shed much new light on uncertainties or challenges in modelling Himalayan glacier response to climate change, that the community was already well aware of. Overall, the approach lags behind the state of the art in modelling the response of debris-covered Himalayan glaciers to future climate change (Ragettli et al., 2016, PNAS Early Edition, www.pnas.org/cgi/doi/10.1073/pnas.1606526113) and in parameterisation of the effects of debris cover on glacier melt (Carenzo et al. 2016. Advances in Water Resources, 94, 457-469, http://dx.doi.org/10.1016/j.advwatres.2016.05.001).

Specific Comments

1. The climate modelling and bias correction of the RCM is good work and should provide the best available meteorological forcing. The delta-h retreat curve in figure 2 is interesting and, on reflection, makes good sense. However, an important issue has been overlooked in the downscaling, since RCM outputs are statistically downscaled using data from Pyramid station, which is located on rock, meteorological input data will be appropriate only for debris-covered ice, which it could be assumed, has a similar near-surface climate. Bare ice surfaces create a surface boundary layer which is typically colder than the boundary layer over the valley sides at the same elevation under melt conditions. This means that air temperature over ice, and by extension, surface melt, will be overestimated in the simulations for a bare ice glacier. This affects interpretations of the impact of debris cover on glacier evolution relative to a bare ice glacier.

2. There are two major weaknesses in the melt model:

i. Debris melt reduction factor fdebris (equation 4) has no provenance.

ii. Calibration of ablation parameters (fM, rice, rsnow in equations 2 and 4) is flawed and application of these 'calibrated' parameters in future modelling scenarios gives meaningless results.

Taking each issue in turn:

On page 7, line 17 it is stated that "This melt reduction curve is based on the observed melt rates under debris from the nearby Ngozumpa Glacier whereby melt rates under 50 cm of debris are 50 % of surface melt rates (Nicholson and Benn, 2006)" I checked this reference and found that Nicholson and Benn did not measure melt rates at Ngozumpa glacier (they used data from glaciers in Europe), nor in their 2013 paper also cited in this study. So where did this "50% reduction under 50 cm of debris" value come from? This is a serious omission given the critical importance of melt reduction under debris to the entire study. Even putting this lack of provenance aside, calibration of the 'reduction curve' using data from field studies is extremely sensitive to the bare ice melt rate value – usually measured at a single point – which could lead to significant over- or under-estimation of melt. A better approach would be due use more physically-based bare ice and sub-debris ablation models to calculate an optimal reduction parameter and uncertainty range for use in the temperature-index model. The impact of debris cover on surface melt is the cornerstone of this study, but it has been modelled in a surprisingly haphazard manner.

The method of calibrating the ablation parameters and their application in forward modelling doesn't make any sense. First, the model is tuned to recent published geodetic mass balance results in the 1999-2010 period assuming a clean glacier, i.e. ignoring the fact that 37% of Khumbu glacier is debris covered in the 1999-2010 calibration period. So, these tuned 'bare ice' parameters implicitly account for the effect of this debris cover and are incorrectly applied as a 'bare ice surface' in subsequent simulations. Next, the model is tuned to the same geodetic mass balance results assuming a thick debris cover, although it isn't clear exactly how this was done, for example whether equation 4 was used with an fdebris value defined a priori and whether the thickness

mask was of variable thickness or constant, and how extensive was the debris cover? This produces a set of results in Table 4 which are counter-intuitive, i.e. ablation parameters for debris which are several times higher for debris-covered ice than clean ice. Although the debris-covered melt rate is reduced by the fdebris factor, it is highly questionable what these melt parameters actually mean and what their effects in the model will be, since both sets were calibrated for assumed conditions which don't represent the true glacier state under the geodetic calibration period. Their application generates the odd result that mass balance evolution is the same with and without debris cover (figure 8). Clearly, this cannot be correct and hence this seems to be an almost entirely meaningless modelling exercise. This conclusion also seems to be reached by the authors' in their own statement on p.12 line 7: "This is an example of highly parameterised models yielding the correct results for the wrong reasons". The statement in the first paragraph of the Conclusion that "…future simulations of glacier evolution are not strongly influence by explicit incorporation of debris…" is also misleading and is a consequence of this flawed calibration procedure.

Under the modelling approach used here, I would have expected fM, rice and rsnow to be the same for both clean and debris covered ice, with fdebris accounting for the insulating effect of a debris cover.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-116, 2016.