

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

Anonymous Referee #1

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Overview

The manuscript by Douglas and others attempts to investigate behaviors of debris-covered Khumbu Glacier under the future warming climate. Although many previous have dealt with debris effect on ice melting, runoff and mass balance of Himalayan debris-covered glaciers, no study performed for long-term changes in glacier runoff under the warming climate. In this regards, this manuscript may be a unique attempt. However, the present manuscript is difficult to catch up what the authors intended to mean because of calculation settings and grammatical errors.

Major comments

C1

Calculation strategy

I cannot understand what the bare ice configuration means. By calibrating parameters to fit the geodetically observational result, which was surely affected by debris cover, values of parameters should be significantly smaller (suppressing melting) than those of general debris-free snow/ice surface. It will result in unrealistically suppressed melt and thus delayed response of glacier shrinkage in debris-free portion of the glacier. What is physical meaning of these values? Although the authors addressed "Since this is a continuation of the calibration configuration for bare ice, it is therefore expected to produce realistic future volume changes." (line 37, page 8), I disagree this assertion. Base on the unrealistic setting, we cannot get any realistic projection. In addition, "bare ice" is a misleading name.

Parameter setting for debris-covered condition is also questionable. The parameter calibration based on Eq. 4 forces all uncertainties to the debris-free melt parameters (f_M , r_{ice} , r_{snow}). However, thickness distribution and relationship between debris thickness and melt rate should have larger uncertainties than that of melt parameters for the "really" bare ice/snow. In addition, because no ice-cliff nor supraglacial pond are not taken into account in the thick debris configuration, calibrated parameters should be larger (enhancing melting) than those of general debris-free snow/ice surface. It will result in unrealistically accelerated melt and thus rapid response of glacier shrinkage in debris-free portion of the glacier. Based on the enhancing setting, future projection will not be "realistic" (line 7, page 9). At least, for the debris-covered calibration, the configuration 4D is much more reliable than the other configurations of 4A-4C because this setting is rather "observational" and the validation geodetic mass balance is a result including all effects of debris insulation and of enhanced melting at ice-cliff and pond.

Both configurations 1 and 4A will result in unrealistically calibrated parameters for bare ice/snow parameters and then the future projection, in which area of debris-covered and debris-free portions will differ from present, should be unrealistic.

C2

I suggest that the bare ice/snow parameters should be calibrated with geodetic changes in debris-free glaciers in the Khumbu region or with mass balance measurements on neighboring glaciers (Vincent et al., 2016). Since then, the authors can further calibrate the other parameters presenting debris-cover effects.

For the future projections, it is unclear how the evolution/degradation of ice-cliff and pond are assumed or calculated.

Because the experimental settings are too unreasonable, I cannot evaluate the results and discussions. Re-think, re-design and re-analyses are definitely required.

Incomplete sentences

Although I am not native English speaker (or because of this?), it is difficult to understand what the authors intend to mean. I simply list (but not all) where they are:

Line 15, page 1: Debris cover as real world material cannot be incorporated into model code.

Line 23, page 1: Totally incomprehensible sentence.

Line 4, page 2: What does "data monitoring" mean?

Line 36, page 2: I do not understand why this procedure allows one to assess the reality.

Line 36, page 2: I do not understand what is "portable nature of the model".

Line 8, page 3: that between suggesting and these. I do NOT point out similar ones.

Line 10, page 4: English is incorrect. Model developed what? Even if English is correctly written, the model development is necessary to be described?

I gave up to point out one by one more.

Calibration with CDFs

C3

This procedure is totally unclear. What is transfer function, linear or nonlinear? How dry days were used in CDFs? What "monthly" CDF means? CDFs were calibrated for each month? Provide more specific information on the calibration procedure.

Incorrect citation

Some papers are cited wrongly.

Line 35, page 1: Kehrwald et al. (2008) did not deal with hydrological system.

Line 1, page 2: Bolch et al. (2008), Benn et al. (2012) and Worni et al. (2013) did not discuss glacier mass loss (or retreat) and GLOF issue.

Line 20, page 8: Thakuri et al. (2015) did not deal with ice cliff and pond on debris-covered glacier but discussed Imja Glacial Lake.

In my understanding, order of citation is from older to latest.

Minor comments

Line16, page 3: RGI ver3 is not up-to-date one. Nuimura et al. (2015) demonstrated how Khumbu Glacier has been differently delineated in previous studies, in the accumulation zone in particular. This could affect the long-term calculation in this study. About errors in the RGI, the RGI authors have generally excused that the RGI was prepared for the IPCC AR5. If so, the researchers have to use the latest version (and we have been forced to do it).

Line 16, page 3: If the original SRTM was used, add the description how many voids at high elevation were filled. If the gap-filled SRTM was used, change the description and cite an appropriate study.

Line 35, page 3: No reference? If this data firstly appeared, instrument and method information should be provided.

Page 4, 3.1. Climate Time Series: I IMAGINE that data from ICHEC-EC-EARCH

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(GCM) was downscaled by RCA (RCM), and that data from Nor-ESM (GCM) was downscaled by REMO. But it is impossible to understand from the present descriptions. Is not GCM abbreviation of General Circulation Model?

Line 31, page 6: In my understanding, mass balance profile is required to estimate glacier thickness. How this mass balance was estimated even though the mass balance was complicated due to debris-cover and highly calibrated.

Line 4, page 8: What is physical basis for constant ratios between three ablation parameters?

Line 15, page 9: configuration 4 means 4A? If "projection" means "future projection", I do not agree that the simulations are "reasonable" because the calibrated parameters of both bare ice and thick debris are unrealistic values.

Line 17, page9: I do not understand what this sentence means. Difference between what?

Line 20, page9: Specify what constant calibration parameters are.

Line 10, page 10: Note that uncertainty of Bolch et al. (2011) is "standard error" but that of Nuimura et al. (2012) is "standard deviation". So that both cannot be addressed "same".

Misc

Line 15, page 1: "crudely" is too crude expression on previous studies.

Line 26, page 1: "The sensitivity" to what?

Line 30, page 2: no Huss et al. (2008a) before this (2008b).

Line 10, page 6: 2008a?

"yr" should be "a" in EGU journals in my experience.

References

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Nuimura, T., Sakai, A., Taniguchi, K., Nagai, H., Lamsal, D., Tsutaki, S., Kozawa, A., Hoshina, Y., Takenaka, S., Omiya, S., Tsunematsu, K., Tshering, P., and Fujita, K.: The GAMDAM glacier inventory: a quality-controlled inventory of Asian glaciers, *The Cryosphere*, 9, 849-864, doi:10.5194/tc-9-849-2015, 2015.

Vincent, C., Wagnon, P., Shea, J. M., Immerzel, W. W., Kraaijenbrink, P. D. A., Shrestha, D., Soroco, A., Arnaud, Y., Brun, F., Berthier, E., and Sherpa, S. F.: Reduced melt on debris-covered glaciers: investigations from Changri Nup Glacier, Nepal, *The Cryosphere Discuss.*, doi:10.5194/tc-2016-75, accepted, 2016.

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

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