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

D. Bocchiola

daniele.bocchiola@polimi.it

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The manuscript in discussion present an interesting issue. However, as it is the manuscript is not suitable for publication in my opinion. The main flaw of the manuscript is that the authors do not rely upon a consistent data base which can substantiate their studies, and instead put forward a number of conjectures that are not based upon data, but rather hypothetical, especially with reference to glacier's morphology, debris cover, ice and snow ablation, ice flow, all fundamental for understanding of Khumbu glaciers' area dynamics, and hydrology. This attitude seems based, as from the motivations reported for each hypothesis, upon the belief of the authors that very little, if any, knowledge exists about the dynamics of glaciers, and hydrological cycle in the study area,

and no data were gathered in the field, and eventually that nobody can put forward consistent modeling of the area, and project forward evolution of glaciers, and hydrology in the area. Accordingly, the authors provide complex modeling of the process in the area by taking a number of arbitrary assumptions, that are neither data driven, nor credible in most cases. As a results, the modeling exercise, and the projections, albeit leading to results that are, broadly speaking, possible given the large uncertainties entailed in the exercise of sketching the future dynamics of natural systems, still are not objectively based. The authors seemingly display incomplete knowledge of the present literature, and are thus led to assume that their results represent a first, original attempt, while they do not in reality, because others already tackled study of the area. This clearly does not prevent the authors from studying the area themselves, but clearly they have to be consistent in comparing their results with the present state of the art. Indeed, while this area is uneasily reachable, still studies in the area are present, that are based upon field investigation, and data driven hypothesis, and modeling, which provide more credible and dependable results. These studies derive from field investigation, which, no matter how difficult need be carried out by everybody willing to investigate such areas with a reasonable degree of accuracy. Accordingly, the results provided here, that rely largely upon the assumption of lack of data, are little original, and scarcely dependable to me. I therefore feel like suggesting that the manuscript should not be published as it is, but resubmitted once more credible and robust (read data based) hypothesis, and conclusions can be sketched. Find below main comments, and in the pdf attached pathwise corrections, and full comment.

Detailed comments Page 1. Line 15. This is not true. There are many models including debris cover in glacio-hydrological modeling, and there is understanding of debris behavior. Fact is, these are based upon field investigation, which was not carried out here specifically.

Page 1. Line 18. This range of debris thickness seems improper to me. Why don't you simply provide an estimate of debris cover using the methods that are available

in the literature ? You could gather some data of debris thickness on the glacier, and calibrate e.g. remote sensing images to then estimate debris cover. Assuming that debris cover ranges arbitrarily from nothing (i.e. debris free) to very much seems not necessary. . .maybe this could be done for the future scenarios, but now there is a visible debris cover which can be estimated.

Page 1. Line 26. This large range derives from your assumptions about debris cover, see comment above. This would be more credible if you would measure ice ablation in the field, which was done already.

Page 1. Line 37. This seems wrong. The melting season is the monsoon season.

Page 2. Line 20. Here you state that ice melting is during monsoon. . ..

Page 2 Line 31. True ice thickness can still be estimated, albeit with uncertainty. Why don't you simply try to do so ?

Page 3. Line 12. There are recent glacial investigations on Khumbu glacier, just browse the literature.

Page 3. Line 22. Several recent studies display that debris thickness can be estimated, so this is not a valid argument.

Page 3. Line 36. This stage-discharge curve was developed by Personnel of Politecnico di Milano.

Page 5. Line 4. This is not clear. To test the validity of corrections of GCMs (or RCMs), I assume you should compare with statistics of observed climate. Melt season starts after 5 "warm" days ? Says who ?

Page 5 Section 3.1.3 This section is largely unintelligible, and the interpretation of bias seems far-fetched. Downscaling simply provides corrected series, that have statistics matching the observed ones. Further complication seems artificial.

Page 6. Line 1. Radiation is theoretical ? Corrected for topography ? Could you use

observed values ?

Page 6. Line 19. The use (and production) of "dh parameterization" is utmost unclear. Indeed, ice flow modeling schemes are easily available in the literature. Why don't you use such tools ? Also, do you have any ice thickness estimate ? Any ice flow velocity estimate ? Ice flow velocity measurements, and modeling for Khumbu glacier have been developed recently, so your approach seems too simplistic.

Page 35-36. Line 35. Observed ice thickness ? Which ones ? Erroneous thin/thick ice with respect to what ?

Page 7. Line 6. Critical debris thickness depends upon rock type. Specific investigation on the Khumbu glacier would be needed. Data are available for that in the literature.

Page 7. Line 17. You sure you can use Ngozumpa glacier results here ?

Page 7. Line 29. You should be able to infer precipitation from measurements in the area, albeit with some uncertainty for altitude lapse rate. This seems not a valid argument. Also, ablation estimates are present for Khumbu in literature. You should endeavor in finding out such information, and confront your results against that.

Page 37. Line 34. The problem seems ill-posed. You are calibrating 3 parameters against one only measurement (i.e. mass balance). Snow and ice melt need be validated separately.

Page 8. Line 5. If you have observed data, it seems not suitable to use downscaled series from models. This is because downscaled series represent statistically the reality, and not deterministically (i.e. with day by day representation), accordingly modeling of glacio-hydrological cycle during a past period (especially for model validation) would be done with observations of climate. This is different to what done for projections, that clearly are representing a statistically possible future.

Page 8. Line 33. The hypothesis of thin debris (configuration 1 apparently) makes no physical sense. . ..what reason may occur for that ?

Page 9. Line 2. To remove the effect of debris, you should know the actual bare ice melt factor, which you did not calculate.

Page 9. Line 4. Debris removal by whom ? This seems not reasonable.

Page 9. Line 9. This full range of uncertainty is artificially introduced by you. Again, if you would use methods for debris thickness estimation available largely, you would not need such far-fetched assumptions. Page 9. Lin 27. Given that everybody know the Khumbu glacier has large debris thickness, how can debris free configuration be realistic ?

Page 10. Line 6. Both configurations explain the mass balance because the problem is ill-posed, you have more unknowns than equations, because you never measured really ice melting to constrain ablation factors.

Page 11. Line 5. What is debris disintegration ? Why would this happen ? This is apparently odd to me. All these configurations are fictitious !

Page 11. Line 34. The fact that simple models can represent reasonably the observed patterns does not authorize neglecting the real processes occurring (i.e. pretending that debris does not exist).

Page 12. Line 7. You state it yourself that your model is wrong, and that only by chance you have acceptable results (for mass balance, and possibly stream flows, but it would not be the case if you would compare all the processes, i.e. ice and snow ablation, ice velocity, etc..), because you have no real field data. One wonders why you do not plan a field study to build a robust study, or at least gather more robust data from former studies.

Page 14. Line 5. Here, it is clearly demonstrated that your model of ice dynamics is unrealistic. . .here yopu clearly need to implement an ice flow model, with accumulation. . .data of velocity are available in the literature.

Page 14. Line 14. Baltoro glacier in Karakoram is totally different from Khumbu in

Nepal here. Immerzeel et al. (2013) did not have information of historical flow from Baltoro, and of ice ablation therein if I remember well, in which case their results can hardly be taken for reference. Karakoram anomaly is tackled in many studies recently, please refer there.

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Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-116/tc-2016-116-SC1-supplement.pdf

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