Reply to comments on the manuscript " Modelling runoff from a Himalayan debris-covered glacier" by K. Fujita and A. Sakai.

We thank three reviewers for the constructive comments and suggestions on our manuscript. Here, we address the concerns raised by the reviewers and explain how we revised the manuscript. The comments are in italic font, which are followed by our replies in bold font.

+++ Reply to Dr. Gao +++

1) The figures of model structure (Figure S1) and distribution of thermal resistance (Figure S4, S5) are shown in supplementary. Why do not you show it in the main manuscript? They are important information I think.

[Reply] We move these figures in the main text.

2) P42, L25-26: "The impact of air temperature on inter-annual variability is 23 times greater than that of precipitation." The authors used the standard deviations of air temperature (0.47oC) and precipitation (97mm) to estimate the sensitivity of runoff with the change of temperature and precipitation. For sure, the sensitivity analysis is a useful method to test the influence of climate change on glacier runoff. And I can understand that the authors would like to illustrate the change of temperature has more influence on the runoff than precipitation. However, I think the conclusion could be dangerous: 'It is clear that variability of the total runoff caused by air temperature variability is 23 times greater than that caused by precipitation variability' (P61, L26-28). Firstly, using the standard deviations of temperature and precipitation to do the sensitivity is controversial.

[Reply] We remove the related sentences in abstract and conclusion.

Secondly, please be noticed that glaciers also dynamically response to climate change, such as changing the area, the length or depth of ice to adopt to the ice thinning in glacier ablation and accumulation zone. You should mention your assumption somewhere that this sensitivity analysis is based on the present steady condition of glacier, without considering the glacier dynamic.

[Reply] Although we briefly mentioned this assumption in P2460L3 and P2462L14, we add the assumption more clearly in the revised manuscript.

Thirdly, this is only a simulation in this study site which you should mention it, to avoid of generalizing this conclusion.

[Reply] We have briefly mentioned that this glacier was somewhat different from the neighboring glacier by citing a previous study in P2459L25.

3) For the structure of Section 2, I recommend the authors to change it a little bit. In order to make it much clearer and easy to read, firstly you can describe the study site and your delineation methods, and then your forcing data of your model, after that you can describe your model structure.
For model structure, firstly you can describe the snow melt model combined with albedo calculation, because the snow melt simulation is included in three landscape classes, the debris-covered, the debris-free and ice-free terrain. Secondly, describe the debris-free and debris-covered glacier melt simulation. You'd better concentrate on the influence of debris on glacier melt, the thermal resistance value in your model. In the end, the simple bucket model for ice-free terrain. Please emphasize the difference between debris-covered and debris-free glaciers runoff simulation.

[Reply] We restructure the method section according to your suggestion as:

2. Location, data and models
2.1. Delineation and classification of the catchment
2.2. Meteorological and hydrological data
2.3. Thermal resistance
2.4. Models
2.4.1. Snow melt and albedo
2.4.2. Probability of snow and rain
2.4.3. Energy and mass balance of the debris-free glacier
2.4.4. Energy and mass balance of debris-covered surface
2.4.5. Runoff from ice-free terrain and the lake
2.4.6. Bucket model calculating river runoff

4) Interestingly, in Fig. 5, I found that the model overestimated the runoff at the beginning of melting season and then underestimated the runoff, especially in 1994 and 1995. To my knowledge the neglecting of the storage effect of snow pack could cause this overestimation of runoff at the beginning of melting season(Gao et al., 2012). In other words, melt water and rainfall is retained within the snowpack until it exceeds a certain fraction(Seibert, 1997). I think at least the authors should explain the reason of this discrepancy.

[Reply] Thank you for the interesting comment. We add one section in the discussion.

5) Please make a table of the parameters in the model and their values in this study.

[Reply] We provide a parameter table.

6) The conclusions are a little bit long. I suggest the authors to shorten the conclusions and make a list of the main conclusions. The details could be moved to the discussion part.

[Reply] We reduce the conclusion.

Minor comments:

P57, L26 and the whole manuscript: What is 'runoff height'? Did you mean the 'runoff depth'? (Gao et al., 2013)
P.48, L21: If the simulation in this study is in water depth, you do not need to mention the water equivalent (w.e.) for the whole manuscript.
[Reply] We replace "runoff height(s)" by "runoff depth(s)", and remove "w.e." from runoff related units.

P42, L10: change "establish" into "established". Furthermore, please be conscious of the choice of verb tense in the whole manuscript, such as 'evaluate and discuss' in P44, L26-27.
[Reply] We check the verb tense in the revised manuscript.

P42, L8: change "scales" into "scale". Please be conscious whether the noun is countable or uncountable in the whole manuscript, such as 'calculations' in P43, L18, 'measurements' in P44, L15 .,et al.
[Reply] We check nouns in the revised manuscript.

P42, L12: change "validated" to 'tested' or 'applied'.
[Reply] We revise here to "applied … to".

P42, L9: rephrase "because: : :", and make this sentence clear. What kind of characteristics of debris is hard to measure? And the necessity of runoff model.
[Reply] We make it clear that "distributions of thickness and thermal property of debris".

P43, L2: what did you mean by ': : :the delivery of water resources to: : :'?
[Reply] I revise here to "role as the water resources to".

P43, L6: did you mean ': : : depends partly on'. And please give a reference to this sentence.
[Reply] I revise it, and provide Scherler et al. (2011) as a reference.

P43, L9 and the whole manuscript: what did 'comparable' mean?
[Reply] We mean "comparable" as "nearly equal". We don't change this term.

P43, L19: remove 'systems'. Change 'in' to 'of'.
[Reply] We revise it.

P45, L5: remove '(the word "Tsho" means: : :)'

[Reply] We revise it.

P46, L5: from Eq. 5, we can find that we can get the Gd without the thermal resistance. Is this correct? Can you explain how you got Equation 5 from Equation 4?
[Reply] Gd is residual term so that we can obtain the thermal resistance from Eq. 5, not from Eq. 4.

P48, L25: why the condensation is included, but the evaporation or sublimation is excluded?
[Reply] Evaporation and sublimation are taken into account in the heat balance. In the water balance, we considered the liquid condensation.

P49, L11: change the function into Qs=…
[Reply] We revise it.

P49, L13: change the function into Qg=…
[Reply] We revise it.

P51, L7: change the function into Dt=…
[Reply] We revise it.

P55, L25: change 'gradient of' to 'corrected'
[Reply] We revise it.

P56, L3-5: Why did you put the results in the supplementary? This is part of your main results, which is important I think.
[Reply] We move Figs. S4 and S5 in the main text.

P57, L3-5: Did you mean 'We calculated both the root mean squared error (DRMS) and the Nash-Sutcliffe efficiency of simulation'?
[Reply] We revise it.

P57, L5-7: change into 'We found that the best estimation is obtained:…'.
[Reply] We revise it.

P57, L12-16: Rephrase this sentence.
[Reply] We split this sentence into two.

P57, L20: change the sentence into 'We further calculate the average value of each components in long term to understand:…'

[Reply] We revise it.

P57, L21: change '. . .runoffs for. . .' to 'runoff in'
[Reply] We revise it.

P57, L22: are you sure 'seasonal cycles' is a proper term? And change 'annual means' to 'annual average'
[Reply] We are sure the term of "seasonal cycle". We change the latter point.

P57, L27: Remove 'defined as area-averaged runoff'
[Reply] We remove it.

P58, L1-3: Rephrase this sentence.
[Reply] We split this sentence into two.

P58, L3-4: Why the similar runoff depth from debris-free glacier and ice-free terrain illustrates the debris free is in a steady state? Explain this point of view.
[Reply] We mean here that mass balance of the debris-free part is "balanced". Because a word "steady state" implies some kind of long-term condition, we rephrased this part as "a state of balanced budget" to avoid misunderstanding.

P58, L9: change '. . .the present. . .' to 'this'
[Reply] We revise it.

P58, L10: remove 'the' before 'calibration'
[Reply] We remove it.

P58, L18: What did you mean by 'resulting runoff'?
[Reply] We simply mean "the calculated runoff". I remove "the resulting".

P59, L1: what did you mean by 'control calculation'?
[Reply] We mean "the best estimation" in the Section 3.2. We do not change this term.

P59, L14: What did you mean by 'comparable'?
[Reply] We mean "comparable" as "nearly equal". We do not change this term.

Section 4.2: this section is the intriguing for me and well written. The simulation showed that the thin and dark debris increased the runoff, compared with debris-free debris covered glaciers. I *suggest the authors further explain the reason of your finding to some extent, such as thermal resistance and albedo et al.*

[Reply] Thank you for your words but the simulation shows that the debris cover is "less effective" to increase the runoff. Lower location of debris-covered area is the main reason of huge contribution of meltwater from the debris-covered area to the total runoff.

P60, L1: Is lake a topographic feature? Please make sure all the scientific terms are properly used.

[Reply] We change the term "topographic" into "surface".

P60, L2: What did you mean by 'lake dimensions'? Did you mean 'lake storage'?

[Reply] We revise here to "size of the lake".

P60, L12-16: Consider to move these sentences to the introduction.

[Reply] We understand that this part is rather introductive but we could not find an appropriate portion to mention this because we focus issues with respect to debris-covered glaciers in the introduction. We appreciate your understanding to keep this part as it is.

P60, L17-18: I totally agree.

[Reply] Thanks.

P60, L19: Please clarify what the projection is. The runoff or the glacier or others?

[Reply] We add "runoff" between "future" and "projection".

P62, L25: What did you mean by 'integrated runoff model'?

[Reply] We mean our model dealing with different surface conditions.

P63, L20: What did you mean by 'comparable'? Did you mean similar or different or something else?

[Reply] We mean "comparable" as "nearly equal". We do not change this term.

P64, L15: maybe change 'perturbation' to 'variability'

[Reply] We revise it.

P64, L24: ': : : on a larger scale: : :' could be better.

[Reply] We revise it.

+++ Reply to Dr. Mayer +++

Structure: I feel that quite a large part of the analysis is presented in the Discussion part. This would be better transferred into the Results part (all the experiments for sensitivity).

[Reply] In our feeling, the most part of sensitivity section discusses how and why the runoff responds to the changes of air temperature and precipitation. We do not change the discussion but reduce conclusion slightly.

Excess melt water: this expression is not clearly defined. This makes it sometimes difficult to assess the impact in the water balance. There are different definitions of excess melt water in the literature, e.g. the difference between melt on a clean glacier and the debris covered case, or excess with respect to balanced conditions. From a definition later in the manuscript it seems that excess is the additional water discharge compared with ice free terrain.

[Reply] We add an explanation "excess water is an additional water runoff compared with the ice-free terrain" in abstract and result section.

Temporal continuity of surface conditions: the authors claim that they implemented possible variations in surface conditions. This however, is only a very rough all or nothing approach which has already been applied by other authors (most recently e.g. Juen et al., The Cryosphere, 2014). There is no mechanism included which allows a realistic temporal evolution of the debris cover. Even though such comparisons are very instructive, it needs to be made clear that the results are just the two possible extremes.

[Reply] We add a relevant description in conclusion part.

Melt at the calving front: There is no mentioning of potential mass loss and enhanced melt at the front of Trambau glacier, which is in contact with Tsho Rolpa. Melt and ice berg calving (with subsequent melt in the lake) could be a considerable amount, strongly dependent on the water temperature. Was this calving cliff balance included in the sensitivity calculation in section 4.2?

[Reply] It is true that additional water should have been supplied to runoff by calving, which was not taken into account. In the result section, we add a description for our assumption, in which the calving is not taken into account in the calculation.

Steep slope terrain (steeper than 30deg) is classified as ice free: In a way this is a sensible classification, even though many of these slopes will be ice covered, but contribute to the glacier mass balance by avalanching. The reasoning behind this classification should be mentioned in the text.

[Reply] If ice or snow was identified by NDSI, the surface was firstly defined as "glacier" in this analysis. Slope definition is used to distinguish debris-cover or ice-free terrain. We add "without snow or ice" after "Steep slope terrain" in the classification part. We do not add the description

with respect to avalanche because we could not find appropriate portion to discuss such topographic effect, which is out of scope of this study.

A separate table of used variables would make it easier to follow the method development, because you could omit the units and the absolute values in the text.

[Reply] We provide a parameter table.

Assumptions in defining the conductive heat flux: Even though researchers dealing with debris covered glaciers are well aware of these assumptions, there should be at least a short discussion about the consequences and limitations.

[Reply] We already stated it in the last part of conclusion.

In my opinion it is unfortunate to assign "R" as a variable name to the radiative heat fluxes and the thermal resistance, because they are fundamentally different physical parameters.

[Reply] We change the abbreviations for radiative heat into H_SR and H_LR.

P. 2447, l. 3f: This definition seems a bit too complicated. Maybe it is easier to define "All fluxes are positive towards the ground.", because G_d is also positive from the debris surface downward.

[Reply] We change the definition of sign. Heat balance for the debris-free glacier too.

P. 2447, l. 4ff: This sentence is rather confusing. Probably turbulent heat fluxes are small at high altitudes because there is only limited mass flux in the low density air.

[Reply] We rephrase this part.

P. 2450, l. 4f: If you apply an iterative method, you need to specify you initial conditions, i.e. what temperature depth distribution to you start with?

[Reply] Temperature profile in the previous time step is used. At first, we obtain a surface temperature to satisfy the heat balance by assuming no conductive heat, calculate change in temperature profile forced by the surface temperature, and thus obtain a conductive heat flux. Inputting the conductive heat, we get a new surface temperature. We calculate the surface temperature and conductive heat repeatedly ant then narrow down to satisfy the all components. We do not describe the details because all was described in Fujita and Ageta (2000) and Fujita et al. (2007) as we mentioned in the main text.

P. 2459/2451: I do not understand the reasons behind the definition of the maximum water content and the bulk coefficient. There must be a reason why to use these numbers.

P. 2453, bucket model: This is a usual way of defining the water balance of the ground storage. However, here the different parameters are just assumed, without any reasoning. Are these values

based on experience in other water sheds, or derived from ensemble runs, or other sources?
Section 4.1: This section only derives the uncertainties due to scatter and temporal variability. It does not include potential systematic errors in the input parameters (e.g. radiative fluxes, surface temperature). This should be stated clearly.
[Reply] We used those values as the previous studies proposed. As another anonymous reviewer commented, all uncertainties are masked by the corrected precipitation. We add analysis and discussion on uncertainties due to relevant constants used in the model by changing each parameter by ±30%, and we found that the setting of firn albedo significantly altered the result.

P. 2453, l. 4f: the albedo of glacier ice varies considerably in dependence of air humidity and wind. The range should at least be mentioned in the text.
[Reply] In this model, albedo does not change with humidity and wind. We add analysis and discussion on uncertainty due to the setting of firn albedo (0.4) but do not mention about the effect of humidity and wind on it.

P. 2455, l. 20f: This sentence is confusing.
[Reply] We rephrase this part.

P. 2456, Distributions of thermal resistance and albedo: The given uncertainty is actually the variability based on temporally distributed data. However, each of the variables involved (Eqn. 5) has a certain error range (the energy fluxes from the gridded data and the surface temperature from the remotely sensed raster values).
[Reply] Although we have no way to evaluate the error range because no "robust data" is available in this study, Suzuki et al. (2007) demonstrated a better error range in thermal resistance than that due to different images if in-situ data was available. It suggests that the error range due to meteorological settings would be less than that derived from the four images in different dates. We already described the relevant statement in P2458L9.

P. 2457/2458: It is surprising that the relative contribution of debris-free ice and ice-free terrain is approximately equal. The sentence at the end/beginning of page 2457/2458 is a bit confusing with respect to evaporation influencing the run-off height.
[Reply] According to the comment from Dr. Gao, we separate the sentence.

Section 4.2: This result implies that the mean thickness of the debris over the entire glacier tongue is still enhancing ice melt and is therefore rather small. Is there any explanation for that, given that the glacier tongue below the ice fall shows a complete and dense debris coverage?
[Reply] We add descriptions for a link between existence of glacial lake and thinner debris, which have been discussed by Suzuki et al. (2007), Sakai and Fujita (2010) and Nagai et al. (2013).

P. 2461: The statement of temperature sensitivity is 23 times higher than precipitation sensitivity is a bit misleading. It should clearly be noted that this is just a comparison of natural variability under the current conditions. Referring to a different time span, this could already be very different.

[Reply] We weaken the assertion.

P. 2462: It should be stated that for the long term effects of variations in precipitation, there is only a static glacier and a static debris cover involved in the estimate.

[Reply] Although we already stated this in P2462L14, we added our "static assumption" in the section for "Long-term averages".

+++ Reply to anonymous reviewer +++

As the authors are well aware, melt of debris covered glaciers is for a large part controlled by ice cliffs and supra-glacial lakes. I think this effect is largely ignored when the thermal resistance approach is used. This should at least be comprehensively discussed and possibly it should be corrected for.

[Reply] This effect is already incorporated in the thermal resistance. Both existences of ice cliffs and supra-glacial ponds result in lower thermal resistance by colder surface temperature. This has been intensively discussed by previous studies which we cited in the manuscript. We briefly add a description in the conclusion.

I have some concerns on using re-analysis data in forcing small catchment models in mountain environments. NCEP-NCAR data have a resolution of several hundreds of kilometers and it has little relation with the local climate in Tsho Rolpa. Figure S3 shows that the correlations are indeed quite poor for most variables. Temperature does relatively well, but this is logical given the seasonality of the climate. Did the authors use the reanalysis data without correction to force the model? If so, I think that is not acceptable. If not, then it should be described how the re-analysis data are corrected using the observations that are available.

[Reply] Although we agree that temperature generally shows a better consistency because of the seasonality, statistical parameters shown with Fig. S3 strongly support the temperature representativeness. We add the above description. We clearly addressed that the reanalysis data were used without correction in P2455L22. We cannot make further action (analysis, discussion and rebuttal) when the reviewer has just said "not acceptable" without addressing the reason.

A discussion on the values used for the precipitation ratio and the gradient is required and if possibly substantiated with references. In addition the use of the precipitation gradient assumes that precipitation will increases with height unlimited, while in reality there will be an elevation of peak precipitation above which the atmosphere becomes so dry and cold that significant precipitation is unlikely.

[Reply] We already provided evidences for the precipitation ratio with Fig. S3 (scatter plot) and references for the gradient though it was obtained in another Himalayan catchment (Seko, 1987; Fujita et al., 1997). Although the argument concerning drier environment at higher elevation has a point, we have no way to confirm it at this moment. We address this point briefly in the conclusion as: "In particular, precipitation would decrease with elevation at extremely higher and thus colder environment. Mass balance data from such high elevation enable us to gain more insight on hydrology in the Himalayan catchment.".

APHRODITES is used for precipitation and the authors use a multiplicative factor and a lapse rate

with elevation to generate spatial fields. They estimate these factors using the Nash Sutcliffe Model efficiency. However this assumes that all other model parameters are correct and equifinality is not an issue, which to my opinion is a critical issue when such a (heavily) parameterized empirical model is used. A cynic may say that the precipitation correction can be used to mask all other errors in model parameters.
The authors address uncertainty in the thermal resistance by analyzing imagery from multiple dates. This a valuable addition to previous work. However the model they present consists of a comprehensive set of mostly empirical relations including some very strong assumptions (e.g. latent and sensible heat fluxes are equal to zero for clear sky days). Many of these relations and their parameters are presented as is, but I would like to know the basis of all of those assumptions. (e.g. maximum water content = 5 mm w.e., bulk coefficient for snow = 0.002, albedo of ice free terrain = 0.1, etc. etc.)
Uncertainty in thermal resistance if just one of the many uncertainties. The paper does not really discuss how the model is calibrated and it would benefit from a more rigorous discussion on model parameter uncertainty. At least a table should be included with the model variables/parameters, their assumed values and the basis for that assumption when possible. Ideally, in Figure 6 the model uncertainty should also be included in the shaded areas in addition to the inter-annual variation.
[Reply] Thank you for the thoughtful comment. Surely all uncertainties are masked by the corrected precipitation. We add analysis and discussion on uncertainties due to relevant constants used in the model by changing each parameter by ±30%, which is equivalent to the uncertainty of thermal resistance and albedo of debris cover. We provide a parameter list.

From equation 10 it seems that evaporation of water from the debris cannot occur, but only condensation. I would say that this could be a significant factor. Also for the other surface types it is not clear.
[Reply] Evaporation and sublimation are taken into account in the heat balance. In the water balance, we considered the liquid condensation.

It is assumed that all water that precipitates into the lake is immediate transferred to runoff. I would think that the lake has a strong buffering role and that a significant part evaporates from its surface. It is not clear if this is considered or not. It also seems that the model does not contain a routing routine. Is all runoff generated on a day immediately at the outlet? This needs further explanation.
[Reply] All runoff generated on a day are immediately put into the internal bucket as we already described. The bucket scheme used in this study generated some sort of buffering effect. According to a comment by Dr. Gao, we add a discussion with respect to overestimation of spring runoff, which might be caused by water stored in snow layer, internal conduit of the glacier and "buffering effect of the lake". Although it is true that the lake surface is a significant source of

evaporation loss, this will not affect the total runoff because of its small contribution to the total runoff (2%, Table 1). We add a brief phrase with respect to "evaporation source" in the section 2.4.5.