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The Cryosphere Discussions

Interactive comment on "Quantifying present and future glacier melt-water contribution to runoff in a Central Himalayan river basin" *by* M. Prasch et al.

Anonymous Referee #2

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General comments

The authors present the application of a hydrological model (PROMET) coupled with an energy balance glacier model (SURGES) to the Lhasa River Basin (32'000 km2) in the Central Himalaya. The model system is further coupled with the scaling tool SCALMET which provides the statistical downscaling of RCM inputs (45km x 45km scale) to the scale of the hydrological simulations (1km x 1km). RCM climate inputs are used to force the model, for both the validation period (1970- 2000) and for future simulations (until 2080).

The application of glacio-hydrological models to high elevation catchments in the Himalaya is a difficult exercise because of notorious data scarcity. The author's strategy to minimize data requirements is to use a "process-oriented" modeling approach, which

relies on "globally valid parameterizations". The climate input downscaling technique relies on "physical and statistical approaches" which are "completely general and can be applied without any further parameterization in various regions".

The intention to simulate present and future glacier melt water contribution to runoff in a large Himalayan watershed with a physically-oriented model is well-founded and certainly the Himalayas are in bad need for such studies. However, the applicability of the model system used by Prasch et al. to the study catchment is questionable for the following reasons:

a) The performance of the model is poorly validated: the lumped response of the catchment is assessed by comparing simulated with observed runoff at three locations, where streamflow is systematically overestimated. The Nash-Sutcliffe values are very low (Table 2). Which model components are responsible for the bias? Except for a 30yr mean value of glacier area and mass change which is compared to literature values the authors do not validate internal processes representation.

b) It is not clear how the authors justify that the parameterizations are "globally valid". PROMET, SURGES and SCALMET apparently have been developed for central Europe, for the GLOWA-Danube project (L5, p. 4567). The term "parameterizations" implies already that the models are not completely physically-based and that therefore the models might have to be recalibrated for a different setting, especially as climate and morphology are completely differing in the Himalaya from the Alps.

c) The authors do not specify the particular characteristics of the study region, in comparison to other study regions, where the model system has been applied with identical parameterizations, and how these particular characteristics are taken into account (this goes in a line with what is said in the previous point).

d) The authors do not mention all the relevant details of the models they are using: what are the parameters, the variables and the input data they are using. The authors also do not provide sufficient references for their models or they provide circular references

to publications of their own in non ISI listed journals, to publications which are under review or to conference proceedings.

Considering these points, the author's choice not to calibrate the model ("in order to be applicable also for changing future watershed conditions or climates" P. 4569, L3), is not convincing. The model performance is at least questionable for the present, and therefore any conclusions based on future projections might be misleading. Given the insufficient validation of the model for the present and the uncertainties about the performance of the model in general, the present study is not suitable for publication in The Cryosphere. The authors should first validate their model in the Himalaya for the present, providing a detailed description of the models they are using and of all "completely general physical and statistical approaches" which justify the application of the model system to the Central Himalaya without recalibration. Only then the model system can be used for future projections.

Further major comments

1. The state of the art for physically-based or satellite-based glacio-hydrological modeling in the Himalaya or other data scarce regions is not sufficiently presented (e.g. Bookhagen and Burbank, 2010, Immerzeel et al., 2012, Pellicciotti et al., 2012).

2. The model descriptions are very general and only few references are provided. For the SURGES glacier model no references are provided at all. How does this model compare to other state-of-the art models?

3. A static mass balance is applied in the SURGES glacier model and ice accumulates therefore endlessly above the equilibrium line altitude. For simulations over more than 100 years this might be a considerable quantity of water which is lost from the water cycle. What does this mean for model results?

4. How are initial ice thicknesses estimated? This is not explained in the text.

5. One should validate SCALMET results against station data, and not the RCM out-

puts. RCM results are not representative on the point scale, since they reflect the mean climate of a 45km x 45km grid cell. SCALEMET results for 1km x 1km raster cells with the same elevation and aspect as the stations would be much more representative. Based on this crude comparison of RCM outputs with station data it is not possible to say which downscaled GCM performs better.

6. Only two GCMs are used for simulations and only ECHAM-5 is retained for the discussion of modeling results. The reason for this choice is not clear, since ECHAM-5 was not the only model which performed well in the cited study of Kripalani et al. (2007). The application of more GCMs would allow attributing some uncertainty range to model outputs.

7. Given the main objective of the authors to quantify the contribution of glacier meltwater to runoff at different scales, the authors should make an effort to shed more light on the effect of scale on the connection between runoff evolution and changing contribution of glacier melt. The authors present as an "astonishing" finding that the fraction of ice-melt is increasing with time, despite the reduction of glacierization. Apart from the fact that this is hardly astonishing since similar trends have been observed in the Alps (e.g. Pellicciotti et al., 2010), this finding could be put more into focus: e.g. at which scale the effect of increasing glacier contribution becomes invisible in river runoff and when is the peak runoff reached exactly?

8. Is supraglacial debris taken into account? This important characteristic of many Himalayan glaciers (e.g. Scherler et al., 2011) and the corresponding effect on glacier melt is not mentioned in the paper.

Detailed comments

1. Abstract, L9: Mention here already which is the study catchment

2. P. 4559, L9: Some key references are missing: Bolch et al. 2012, Kääb et al. 2012

3. P. 4559, L11-L15: Here you could provide some references to applications of

physically-oriented glacio-hydrological on the smaller scale (e.g. Immerzeel et al., 2012).

4. P. 4560, L11: What is the definition of a 'complex' headwatershed? What does 'representative glacierized' mean?

5. P. 4560, L26: these are both no proper references.

6. P. 4561 L8-L10: the "synchronous ablation and accumulation period . . . determines the importance of glacier melt for water availability" – what does that mean?

7. P. 4562, L2-3: Use acronyms and show the water balance as an equation, and not in the text.

8. P. 4562, L11: please specify what is meant by "subscale approach"

9. P.4562, L14-L24: some of this seems obvious and can be removed from the text, e.g. that snow melt comes from both glacierized and non-glacierized cells, and that it does not make a difference for the water balance where it comes from. It is not true however that the same physical principles are valid on- and off-glacier. Also, if this was true, why are different model parameters used on- and off-glacier? Which are the model parameters exactly that are different? How is surface roughness estimated?

10. P.4562, L29: what is a "consistent" meteorological data set? Please specify.

11. P. 4563, L8: The Lhasa river basin is much smaller than 100'000 km2. Does this have any implications? Are there any trade-offs in model structure (more conceptual solutions rather than being physically-based) which were accepted in order be applicable on the very large scale?

12. P. 4563, L11: which are the parameterizations? Are there components which are really physically-based? For which components more conceptual solution had to be chosen?

13. P. 4564, L6: what is the temporal resolution of the RCMs?

14. P. 4564, L14: which are the "physical and statistical approaches"? Are they really "completely general"? Without further explanations this is not convincing.

15. P. 4564, L19: what is the resolution of the subscale units? Are they raster based or discretized in elevation belts? This is not clear from Fig. 3a.

16. P. 4564, L19: Is it the area-elevation-distribution which is parameterized? What are the parameters? Please specify.

17. P. 4564, L23: how are ice thicknesses calculated / estimated? This might be a crucial point for the modeling.

18. P. 4564, L24-25: are all these variables provided by the RCM and downscaled by SCALMET? Mention in the previous section 3.1.2 which are the variables that are downscaled by SCALMET and how.

19. P. 4565, L4: this is not the correct definition of katabatic winds.

20. P. 4565, L7: How are all these variables extrapolated exactly to the subscale level? Which are the parameterizations that are used and how are parameter values estimated?

21. P. 4565, L12-16: this section misses a detailed description of the energy balance model which is used to calculate melt. Variables, parameters and input data have to be specified.

22. P. 4565, L19: is there not glacier routing component, or at least a glacier reservoir which delays the injection of glacier melt (both snow and ice) into river runoff?

23. P. 4565, L20: what exactly is a "defined" number of ablation periods? Please specify.

24. P. 4565, L27: how are changes in the albedo calculated?

25. P. 4565, L27 "In the case of melt water. . ." until end of paragraph: it is somewhat

obvious that snow that is transformed into ice contributes to icemelt once it is melts. Consider removing or rephrasing.

26. P. 4566, L10-15: should be mentioned that this is a static mass balance approach. What are the implications for the modeling? In Prasch et al. 2011a a simple parameterization of ice flow is used. Simple parameterizations of glacier geometry changes are also suggested in Huss et al. 2010 or used in Immerzeel et al. 2010. What was the reason to choose a static mass balance approach rather than using a parameterization?

27. P. 4566, L19: what about ERA40? This GCM is mentioned only later in the paper.

28. P. 4566, L23-L25: How are CLM data downscaled? Are any station data used for that?

29. P. 4566, L26-L28: please specify for each of these datasets for which model parameters/model components they are required.

30. P. 4567, L7: please make clear what are the differences between Prasch et al. 2011a and this study. Some of the plots are identical (e.g. Figure 8 in Prasch et al. 2011a and Fig. 6a and 7a in the present study).

31. Section 4.1: Here a better discussion of the station data compared to the downscaled climate data would be essential. The paper does not show any seasonal course of temperature or precipitation. Further the elevation of the station and the elevation range of the CLM raster cells need to be provided. See also point 5 major comments.

32. Section 4.3: Why are model simulations with CLM ERA 40 and CLM ECHAM5 validated in a different way? The same plots and the same goodness-of-fit values should be presented for both model runs and then discussed.

33. Fig. 7: There should be a similar figure which shows runoff for the periods where the model does not perform well (Table 2).

34. Tables 2 and 3 could be combined. Why does the model perform so badly for the longer validation period? Do you have runoff overestimation only in summer or also in winter? This is not evident from Fig. 5.

35. P. 4568, L24: “the mean annual runoff is validated as climate signal”. This is not clear to me. What do you mean by that?

36. General comment on section 4: consider separating this section into a subsection in the ‘methods’ section and a subsection in the ‘results’ section. The methods which are chosen to validate the models are essential for the modeling experiment, and the performance of the models could be considered as a result. If the performance of the model is already assessed in Prasch et al. 2011a this has to be mentioned clearly.

37. P. 4569, L12: where are the ELAs in 2080, for each of the simulations?

38. P. 4569, L21-L23: “In order to . . .only occurs in the glacierized areas.” Ice-melt occurs only in the glacierized areas? This is evident. Please remove.

39. P. 4570, L9: “astonishing” is not very scientific, and neither is the use of the word justified here. See major comment 7.

40. P. 4570, L17: provide reference to equation, see detailed comment 7.

41. P. 4570, L24: 30% evaporate; how does this number compare to other studies, in the Himalaya or elsewhere?

42. P. 4571, L10-16: this should not go into the result section. Either remove or move to the introduction.

43. P. 4571, L20: “daily runoff course”? This should be annual runoff course.

44. P. 4571, L22-23: “runoff is low during winter. . .” This should go into a data section, as P. 4572, L8-9 “Accordingly, runoff generated. . .” and elsewhere in section 5.3. Much of this is data/study area description.

45. P. 4572, L14: In order to show future runoff evolution, show the seasonal course of runoff for different periods together on the same plot. Or plot annual mean runoff over time, for different scales, in order to discuss the effect of variable glacier contribution depending on scale (see also major comment 7).

46. P. 4573, L7: if insights can be considered “valuable” depends on the point of view. For whom exactly they are valuable. If this is a subjective judgment then rather remove.

47. P. 4573, L21: This is the first time that sublimation is mentioned in this paper. How is sublimation calculated? By PROMET or by SURGES? Does sublimation affect the water balance?

48. General comment on conclusion section: much of this really is model description, which should go into the method section. The conclusion does not focus on the main results of the study. It should be mentioned what were the main objectives and what were the corresponding results.

49. P. 4574, L12: “globally valid parameterizations”; not clear how this is justified.

50. P. 4574, L19-L22: a proper description of type and quality of input data is missing, which is exactly one of the reasons why the applicability of the model could not be demonstrated. Also, the validation for the Lhasa River catchment revealed that the model performance is insufficient for the 30 year validation period, and did not prove the reliability of the model.

51. P. 4574, L23: uncertainties in current GCMs; this is exactly the reason why more than just one GCM should have been considered for the discussion of modeling results.

52. P. 4575, L2: is the value provided by Oerlemans 2005 representative for this study region?

Interactive comment on The Cryosphere Discuss., 6, 4557, 2012.